THE HOUSE OF THE FATHER AS FACT AND SYMBOL

PATRIMONIALISM IN UGARIT AND THE ANCIENT NEAR EAST

by

J. David Schloen

EISEN BRAUNS
Winona Lake, Indiana
2001
For Sandy
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PREFACE

This book on “Patrimonialism in Ugarit and the Ancient Near East” is the first of two volumes on “The House of the Father as Fact and Symbol.” It consists of a lengthy theoretical introduction followed by a study of the Late Bronze Age kingdom of Ugarit (14th and 13th centuries B.C.) in its wider Near Eastern context. The second volume will deal with subsequent cultural developments of the first millennium B.C. in the Levant, and in ancient Israel in particular, under the heading “Tradition and Rationalization in the Axial Age.”

Both volumes have their origin in my doctoral studies in the Department of Near Eastern Languages and Civilizations of Harvard University. More specifically, I began thinking along these lines in a seminar devoted to the archaeology of Ugarit conducted in the spring of 1991 by Lawrence Stager, my doctoral advisor. I developed my ideas further in the departmental seminar on the history and literature of ancient Israel later the same year. My research papers in these seminars, in which I first investigated Max Weber’s ideal type of “patrimonialism” and its relevance to Bronze Age Canaan and Iron Age Israel, eventually grew into my 1995 doctoral dissertation, “The Patrimonial Household in the Kingdom of Ugarit: A Weberian Analysis of Ancient Near Eastern Society.” That dissertation has been extensively revised and expanded to form Part Two (chapters 9–14) of the present volume.

Before discussing ancient Near Eastern society in Part Two, I have inserted Part One (chapters 1–8), which deals with “The Interpretation of Ancient Social Action.” This supplies what I believe is often lacking in studies of ancient society, namely, an explicitly theoretical introduction that explains and defends the substantive argument that follows. The philosophical and sociological issues provoked by any attempt to reconstruct ancient social life are so complex and interconnected that I have felt it necessary to start by laying out my own assumptions and methodology in a systematic fashion. In particular, it has been necessary to explain what I mean by the dialectic of fact and symbol around which the entire work is constructed. After that, we can turn in Part Two to Near Eastern sociohistorical phenomena of the third and second millennia B.C., focusing on the patriarchal household as both fact and symbol in the Bronze Age Near East, and in the coastal Mediterranean kingdom of Ugarit in particular. The argument developed in this volume will be continued in the projected second volume on the “Axial Age” of the first millennium B.C., in which I will survey the continuities and innovations in the Bronze Age “house of the father” (again on both factual and symbolic levels) that are evident in Iron Age Israel and the subsequent Jewish society of the last few centuries B.C.

I am deeply grateful to Lawrence Stager for his guidance and support of my academic efforts over the years. Those who know his work will see its pervasive influence in what I have written, although he is by no means responsible for any errors of fact or method that may be found herein. I thank him especially for drawing my attention to theoretical issues, and to the writings of Max Weber and Paul Ricoeur, in particular. In his own work, Stager has provided a model of the problem-centered integration of archaeological and textual evidence in historical research. In a seminal 1985 article on “The Archaeology of the Family in Ancient Israel,” he applied Weber’s patrimonial model to Israel, implicitly questioning current functionalist trends in biblical history and archaeology. This is what prompted me to pursue the matter in my own research, culminating in this book.

I am grateful also to Frank M. Cross, from whom I have learned much about the interpretation of Ugaritic mythological literature and the history of the religions of Canaan and Israel. Other Harvard faculty members have had a profound influence on me as well, and I thank in particular the members of my dissertation committee, John Huehnergard, Peter Machinist, and Piotr Steinkeller. I have profited both from their superb teaching and from their critical acumen as readers of my work. Of great value to me also were Paul Hanson’s seminar on hermeneutical theory and Jon Levenson’s critique of an early formulation of my ideas about the “patrimonial principle” in ancient Israel.

It is a pleasure to express my sincere appreciation to my colleagues in the Oriental Institute of the University of Chicago, who have given generously of their time to read and comment on all or part of this volume. McGuire Gibson and Mark Lehner, especially, have given me excellent advice on many
points and have been of great assistance in encouraging (and prodding) me to finish the book. Dennis Pardee has given me the benefit of his unsurpassed knowledge of Ugaritic and of research on ancient Ugarit. In addition, I would like to thank John Brinkman, Miguel Civil, Fred Donner, Gene Gragg, Martha Roth, Donald Whitcomb, and Tony Wilkinson for their helpful comments and criticisms. I can think of no better place than the Oriental Institute in which to carry out the sort of wide-ranging, interdisciplinary project I have attempted in this book.

Other senior colleagues who have offered helpful advice and to whom thanks are due include John J. Collins of the Yale Divinity School and Gil Stein of the archaeological faculty of Northwestern University. Michael Coogan, director of publications at the Harvard Semitic Museum, has carefully gone over the entire book and has advised me on matters of style and format. Of course, any errors that remain are my own.

I have profited also from my conversations with a number of younger scholars and fellow students of my own generation, including F. W. Dobbs-Allsopp, Daniel Fleming, Timothy Harrison, Ignacio Márquez Rowe, Daniel Master, Bruce Routledge, Kathryn Slanski, David Vanderhooft, and Steven Weitzman.

I would like to thank the University of Chicago Division of the Humanities and its then dean, Philip Gossett, for the award of a Junior Faculty Fellowship in the summer of 1996 in support of my research for this book. My student Aaron Burke has ably assisted me with the figures. To him and to other students who have patiently listened to my ideas, I am grateful.

This book is dedicated to my wife Sandra, who has helped me more than anyone.

David Schloen
Chicago, February 2001
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### Abbreviations

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<td>RIMB 2</td>
<td>The Royal Inscriptions of Mesopotamia—Babylonian Periods, vol. 2 [Frame 1995]</td>
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<td>RIME 2</td>
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<td>RIA</td>
<td>Reallexikon der Assyriologie</td>
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<td>RN</td>
<td>royal name</td>
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<td>RS</td>
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<td>Studies on the Civilization and Culture of Nuzi and the Hurrians</td>
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<td>SD</td>
<td>Studia et Documenta ad Iura Orientis Antiqui Pertinentia</td>
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<td>Studia et Documenta Historiae et Iuris</td>
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<td>sg.</td>
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<td>Vestnik drevnei istorii</td>
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<td>WVDOG</td>
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<td>YES</td>
<td>Yale Egyptological Studies</td>
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<td>YOS</td>
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<td>ZA</td>
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<td>ZÄS</td>
<td>Zeitschrift für Ägyptische Sprache und Altertumskunde</td>
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<td>ZAW</td>
<td>Zeitschrift für die alttestamentliche Wissenschaft</td>
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<td>ZDPV</td>
<td>Zeitschrift des Deutschen Palästina-Vereins</td>
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This book examines the patriarchal household in the ancient Levant. It views this fundamental social unit stereoscopically, as both fact and symbol. From one perspective the “house of the father” is a basic demographic and economic fact; from another perspective it is a powerful political and religious symbol. My main contention is that neither aspect can be understood apart from the other. The persistence for many centuries of objectively observable patterns of “patriarchal” behavior in all manner of social contexts can be attributed to the subjectively experienced symbolic power of immediate household relationships. But just as the socially integrating symbol of the father’s house is rooted in the mundane facts of household life, these facts, insofar as they are social facts, are themselves molded and perpetuated by the symbols they generate.

Externally observable facts of social life and corresponding symbols of social relationship are here intimately related in a reciprocal fashion. On the one hand, the importance of the extended patriarchal household as the fundamental metaphor of ancient Near Eastern political relationship must be attributed to the constantly replicated experience of social interaction within such households. “Lived experience” of the father’s house was widely shared; thus it powerfully and persistently shaped a very durable common symbolization of the entire divine-human social order. An interlocking set of metaphors drawn from traditional household relationships—“father,” “son,” “brother,” “master,” “servant,” “heir,” etc., each of which could evoke the root metaphor of the “house of the father” or could be evoked by it—were creatively applied in a wide variety of situations beyond the ordinary household, serving to mediate and motivate social action of many kinds. On the other hand, traditional patterns of household behavior were themselves validated and sanctioned because of the constitutive role played by such homely metaphors at the most exalted political and cosmological levels. In this way, fact and symbol reinforced one another to provide a flexible but relatively unchanging social framework until well into the first millennium B.C.

In this book, moreover, the patriarchal household is viewed not just stereoscopically, as fact and symbol, but also panoramically, in the context of ancient Near Eastern civilization as a whole. The West Semitic cultures of the Levant were not unique in their use of the father’s house as a basic political symbol; on the contrary, this must be seen as part of a broader civilization-defining phenomenon. Of course, that phenomenon was manifested in a number of different ways, both as demographic fact and as political symbol, within the broad geographical and chronological range designated by the term “ancient Near East.” But I shall argue that these diverse manifestations were variations and developments of a coherent pattern of meaningful social action that was universally understood and so endured for thousands of years in the ancient Levant and its environs.

Now, extended patriarchal households and corresponding symbolizations of the social order are by no means peculiar to the ancient Near East. In the Near East, however, the dialectic of the “house of the father” as outer fact and inner symbol is richly documented in exceptional temporal depth by archaeological and textual evidence spanning the last three millennia B.C. Furthermore, during the first millennium B.C. the Near East witnessed a striking transformation in the “house of the father” that was parallel to similar changes in the Greek world. Best documented in this regard is the way in which the symbol of the patriarchal household took on a distinctive shape in the Israelite, and later Jewish, society that emerged within a broadly “Canaanite” West Semitic milieu.

The subsequent impact of Jewish thought and writings on Western civilization is often noted, but this impact has been most decisive, in my opinion, at precisely the point under discussion here. The distinctive Jewish and Jewish-Christian transformations of the primordial Near Eastern symbol of the father’s house became an important source of the increasingly “rationalized” symbolizations of cosmic social order that characterize both European and Islamic history. This momentous symbolic shift first became socially effective on a broad scale during the period of Assyrian

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1 The term “Levant” originally designated the eastern Mediterranean coastlands under Ottoman rule, from Istanbul to Egypt. In keeping with current scholarly practice, this term is here defined more narrowly as the region bordering the far eastern shore of the Mediterranean Sea, also called “Syria-Palestine.”
and Babylonian imperial rule in the seventh and sixth centuries B.C. The transformation continued well into the Roman period, as is clear from the Levantine literature preserved from the latter part of the first millennium B.C. In particular, various biblical writers, including the authors of the New Testament, narrated their new experiences of reality in terms of the old but fertile symbolism surrounding the extended patriarchal household.

Israelite and Jewish symbolic innovations must not be divorced, however, from the “factual” socioeconmic transformation or rationalization of those traditional patterns of action that constituted the mundane household itself. Accompanying the symbolic shift was a behavioral shift that reflected the fundamental changes taking place in the social and economic structure of the entire Mediterranean and Near Eastern world in the age of the first-millennium empires. Not just in ancient Israel but throughout the region there is evidence that these sweeping socioeconomic changes were intertwined with a rationalization of the patriarchal household that was parallel to what occurred in Israelite and Jewish experience. This parallel transformation is best known in classical Greece, but there are clues that similar changes occurred to some extent also in Egypt, Mesopotamia, and Iran (not to mention East Asia), long before the Hellenistic period inaugurated in the Near East by Alexander’s conquests. It should be recognized, therefore, that Israelite and Jewish developments, although distinctive, were part of a much broader trend.

Having said this, the evidence from ancient Israel and its literary and religious heirs remains uniquely informative. There is striking testimony, compiled over hundreds of years and preserved in the Bible, of both the archaic behavioral and cognitive pattern signified by the term “house of the father” and the epoch-making transformation of this age-old pattern in the course of the first millennium B.C. That transformation, as unpredictable and historically contingent as it may have been, can be understood in terms of a perennial human dialectic of “tradition” and “rationalization,” in which traditional symbols of social order do not simply die out but are reinterpreted and rationalized in the course of experiencing new life situations. The new, more rationalized symbolic traditions created by this transformation are of special interest, even today, because the biblically attested rationalization of the father’s house in terms of a distinctive form of monotheistic religious belief has subsequently exerted exceptional influence in Western and Islamic civilization.

In general terms, then, the principal questions arising from a detailed investigation of the ancient Near Eastern “house of the father” are how and why that original and durable social pattern changed—as it plainly did, both factually and symbolically—and what the cultural legacy of this change has been up to the present. In the hope of understanding better both the archaic household and its symbolic and factual transformation, I have attempted to trace the cognitive and material aspects of human life-experience within the kinds of patriarchal households, actual and metaphorical, which constituted the social order for so many centuries in the Levant, and the ancient Near East as a whole. In so doing, I have depended on both written and unwritten (especially architectural) evidence. This was done out of the conviction that observable facts of social behavior and the symbols by which that behavior was understood and motivated must receive equal attention if we are to disentangle, to any degree, the complex diachronic interplay of fact and symbol that has yielded the evidence we possess. It is this kind of post hoc explication of epoch-making changes in broad patterns of social action that I consider to be a prime goal of explanation-oriented ancient historians, whether their individual academic specialties lie in archaeology or in textual studies.

It is true that this rather ambitious interdisciplinary approach to the study of long-term sociohistorical developments has seldom been taken in recent years by students of ancient Near Eastern civilization. Near East specialists have tended to restrict their investigations to topics defined more narrowly in spatial and temporal terms, and they have often restricted their research still further according to the type of evidence, archaeological or textual, which is considered. Although that sort of specialization has undeniable advantages, and the alternative carries certain risks, the obvious weakness of narrowly focused historical research is its lack of a fully adequate intellectual and comparative context. This weakness arises when inherited assumptions and interpretive habits do not receive the scrutiny they deserve in light of general philosophical and theoretical developments and specific empirical discoveries in other fields. Over the years, many interpretations of ancient Near Eastern sociohistorical phenomena (including the surreptitious interpretations embedded in avowedly non-theoretical, “purely descriptive” studies) have, in my opinion, employed anachronistic and highly questionable analogies and concepts, especially with respect to political, legal, and economic institutions and behavior.
Moreover, these historical analogies and associated conceptual models tend to be adopted rather casually, rarely being defended in terms of a theoretically informed and explicitly articulated comparative methodology. I have therefore found it necessary in what follows first to explain my own sociohistorical presuppositions and corresponding methodological choices, with particular reference to twentieth-century developments in philosophical hermeneutics and related debates in social theory and historiography. Next, I have thought it appropriate to introduce some of the fundamental interpretive issues raised by the ethnographic, architectural, and documentary evidence pertaining to premodern households and urban life in various periods within the wider Mediterranean region.

The consequent diversity of sources and methods discussed below—ranging from the philosophical and sociological to the archaeological, and from the economic and demographic to the legal and literary—does not indicate a lack of focus, however. The theoretical and empirical issues presented in Part One, although perhaps unusual in their present combination, have been carefully chosen to form an interlocking framework for the argument developed in Part Two, and in a planned second volume on the first millennium B.C. They are all relevant to the explication of the multifaceted phenomenon of the patriarchal household, which was both an inescapable fact of life and an all-encompassing symbol of ancient Near Eastern social experience.

Once this requisite interpretive framework has been established, I go on to consider in some detail the distinctive textual record and physical architecture of the father’s house in the ancient Levant within its broader Mediterranean and Near Eastern context. The relatively well-documented Late Bronze Age kingdom of Ugarit serves as a primary case study in Part Two of the present volume, and I will build on the same theoretical basis in the second volume, in which ancient Israel will be the focus of attention. In that volume I will review archaeological and biblical evidence of the patriarchal household in the Iron Age kingdoms of Israel and Judah (provisionally discussed below in chapter 8), and I will consider the transformation of the Israelite household, both as a demographic and economic fact and as a religious and political symbol, in the period stretching from Assyrian to Persian imperial domination. In a section on “The House of the Father as Ideology and Utopia” I will also make some suggestions concerning the subsequent effects of this transformation in the course of the last few centuries B.C., noting the ongoing rationalization and sedimentation of the symbol of the “house of the father” within classical Jewish and Christian belief systems, in light of its modern appropriation and critical deconstruction.

The contents of the projected second volume are outlined here in order to indicate the overall trajectory of a line of argument whose beginning is articulated in the present volume. When completed, this investigation will span the period from the Bronze Age to the Roman era, tracing the development of a single symbol—the father’s house—that was rooted and replenished in individual experience, but also provided the framework for group action on a much wider scale. Indeed, it is because this ordering symbol was so deeply rooted in everyday life that it was able over the centuries to work simultaneously both to legitimate the prevailing social order and to critique it in the name of an imagined community of love and justice.
PART ONE

THE INTERPRETATION OF ANCIENT SOCIAL ACTION
Chapter 1. Hermeneutics and the Human Sciences

The distinction that I am making between outer facts and inner symbols, and my emphasis on the constant and irreducible dialectic between them, are drawn from the hermeneutical and phenomenological traditions in philosophy and the corresponding “interpretive” paradigm in social theory. In the early decades of the twentieth century, Edmund Husserl propounded an influential phenomenological method that stimulated a number of important new philosophical approaches, including that of Martin Heidegger. Heidegger combined Husserl’s phenomenological approach with key elements of Wilhelm Dilthey’s hermeneutical theory to create a distinctive “hermeneutic phenomenology” of human existence, which was most famously expressed in his 1927 treatise *Being and Time* (Heidegger 1996). Heidegger’s so-called existential hermeneutics in turn became the basis of the more fully elaborated “philosophical hermeneutics” of Hans-Georg Gadamer (1976; 1989), who explored the implications of Heidegger’s insights for history and the other human sciences in his book *Truth and Method*, first published in 1960.

The hermeneutical approach of Heidegger and Gadamer has been developed further in recent decades by the prolific French philosopher Paul Ricoeur (1969; 1974; 1976; 1981; 1991). Most importantly, Ricoeur has purged hermeneutics of the lingering Romanticist tendency that is still detectable in Gadamer’s writings. Various critics have seen in Gadamer’s work an overly optimistic belief in the possibility of immediate or transparent understanding, whether of oneself or of others. In his own theory of interpretation, Ricoeur has taken into account the serious challenges to Gadamer’s optimism presented by Freudian psychoanalysis and by the neo-Marxist ideology-critique of Jürgen Habermas and the Frankfurt School. He has also adopted some of the insights of French structuralism—although in so doing he has vigorously challenged structuralism’s claim to furnish a complete paradigm for the human sciences. Looking beyond Continental philosophy to a quite different philosophical tradition, Ricoeur has also incorporated into his hermeneutical theory the insights of English-language authors in the fields of analytical philosophy of language, literary criticism, action theory, and the theory of history.

Ricoeur’s remarkable inclusiveness and accommodation of so many different viewpoints have not yielded an inchoate theoretical eclecticism, however. Ricoeur deals sympathetically but critically with a wide array of competing intellectual alternatives in order to retrieve valuable if seemingly contradictory conclusions, which he then places in a larger context and reconciles through incorporation into his own tightly constructed and internally consistent theory of interpretation. The comprehensiveness and rigor of Ricoeur’s hermeneutics, and his ability to synthesize productively the insights of a variety of apparently antithetical approaches, have won his views substantial influence in many fields within the human sciences. For our purposes, however, what is most important is Ricoeur’s extension of the hermeneutical paradigm of text interpretation to the interpretation of symbols, and thus of symbolically constituted social action in general. He has developed this approach most fully in his three-volume work *Time and Narrative*, published in the 1980s (Ricoeur 1984; 1985; 1988).

Ricoeur presents a highly refined and open-ended philosophy of human experience which builds on the pioneering work of Dilthey, Husserl, Heidegger, and Gadamer, but illuminates more fully the dialectical relation of symbolic texts and factual action, and so lays bare the underlying homology between the theory of interpretation and the theory of social action. Ricoeur’s hermeneutical model for historiography exploits to the full the often misunderstood “agent-oriented” or “interpretive” (verstehende) sociohistorical method of Max Weber. Together, Weber and Ricoeur provide a tightly woven yet flexible conceptual scaffolding for the construction of nuanced and rigorous interpretations of human social action, past and present, whether that action is evident through works of discourse or through material artifacts.

Of course, other theoretical approaches are available, ranging from positivist functionalism to structuralism and neo-Marxist critical theory, not to mention the radically historicist stance adopted in more recent poststructuralist or “deconstructionist” social
theory. This is not the place for a detailed evaluation of these alternatives or for a comprehensive defense of Ricoeur’s approach, a task which other writers and Ricoeur himself have accomplished quite effectively, in my view. It should be noted, however, that I have drawn heavily on Ricoeur, and on hermeneutical theory in general, because I believe that here can be found the most comprehensive and least reductionist of the available critical approaches to the interpretation of human social action, past and present.

Inherent in Ricoeur’s thought, of course, and that of other hermeneutical writers, is the presumption that no theory can be final. All conceptualizations are rooted in time-bound preunderstandings and hence reductions of a more complex—indeed, inexhaustible—reality. But Ricoeur inherits from phenomenology a reluctance to begin his analysis with refined abstractions rather than with the prior unitary experience of the human “lifeworld.” This accounts both for the breadth of application of his concepts and for his ability to bring into fruitful dialectical partnership the partial insights that other theorists have too often presented in a one-sided manner.

Moreover, the strong ties between Ricoeur’s fusion of interpretation theory and historiographic theory, on the one hand, and the parallel but more narrowly focused tradition of verstehende sociohistorical research, on the other, provide a bridge between more general and basic philosophical considerations having to do with human existence, language, and interpretation, and the specific Weberian concepts and models that I will employ in the substantive interpretations attempted below.

With respect to the topic of the present work, the hermeneutical perspective championed by Ricoeur and others in philosophy, and by Weber and his heirs in social theory, calls into question existing positivist and functionalist approaches to the archaeology and history of the ancient Near East, and of Canaan and Israel in particular, and leads to quite different interpretations of many ancient social phenomena. Positivist assumptions, whether explicitly acknowledged or largely unarticulated and implicit, remain deeply rooted in Near Eastern archaeology and social history. In what follows, therefore, I will challenge a number of widely accepted and rarely questioned notions about ancient Near Eastern society. In so doing, I will attempt to revive certain older and now forgotten or discarded conceptions employed by an earlier generation of Near East specialists. This is done not out of nostalgia for some golden age of scholarship, but in order to retrieve genuine insights from the early days of phenomenology and interpretative social history in the first half of the twentieth century that have been caricatured or misunderstood in recent decades.

Obviously, this is an undertaking that requires careful justification, beginning with a detailed presentation of the theoretical basis of my own interpretations. The necessarily abstract, even didactic, generality of this introductory material might strike some readers as odd. But to prevent any misunderstanding of the attention I am giving here at the outset to hermeneutics and social theory—still an unusual gambit in ancient Near Eastern studies—it is worth belaboring the point that no scholarly interpretation of past social phenomena is free of debatable presuppositions. Tempting as it may be to avoid explicit theorizing, the fact remains that contestable theoretical choices are embedded in even the most “obvious” and innocent-looking of “common sense” interpretations in archaeology and socioeconomic history. The unceasing effect of one’s theoretical assumptions (whether or not these are explicitly articulated), and the necessity, even in “purely descriptive” studies, of employing conceptual models derived from more familiar historical analogies (however partial and provisional such models may be), call for a systematic defense of one’s choice of presuppositions and models, at least when these are likely to be unfamiliar to one’s readers or might otherwise be misconstrued.

This does not mean that in the deductively organized discussion that follows I have ignored the evidence of my sources by first selecting an extraneous theoretical approach and then imposing a resultant abstract and oversimplified model on the data. On the contrary, it is precisely to avoid the unknowing imposition of alien categories on ancient Near Eastern evidence, against which eminent Near East specialists have repeatedly warned since Benno Landsberger’s famous 1926 defense of the “conceptual autonomy of the Babylonian world” (reprinted in Landsberger 1976), that I have taken the trouble to begin at the beginning and examine critically the presuppositions that inform my subsequent interpretations.

Moreover, I believe that my choice of historiographic assumptions and specific models responds directly to the particular evidence available to me and reflects the importance I attach to avoiding reductionism by taking seriously the “native” self-expressions of ancient lived experience. In forming and applying conceptual models I hope to be neither brutally deductive nor incoherently inductive, but to assert the irreducible methodological dialectic of data and concepts, neither of which should be permitted to take priority. This dialectic is, of course, the modern scholarly version of the quintessentially human inter-
pretive dialectic of fact and symbol—the interplay between what is given and what is understood—around which the present work is constructed.

Before considering Ricoeur’s interpretation theory and its historiographic implications in more detail, it will be useful first to review the principal assumptions and conclusions of twentieth-century hermeneutics and to rehearse its arguments against various forms of objectivist “scientism” or “naturalism” in social theory and historiography. Until quite recently, most Near East archaeologists and historians received their training in a generally positivistic milieu, whether Marxist or non-Marxist. Many no doubt remain suspicious of attempts to bring to bear on the rigorous scientific task of deciphering ancient documents and artifacts an unfamiliar hermeneutical approach that may seem more at home in the farther reaches of literary criticism. The central assumption of the present work, however, is that the hermeneutical approach constitutes a coherent intellectual paradigm that is both superior to and fundamentally opposed to the positivistic paradigm which has characterized so much sociohistorical scholarship on the ancient Near East.

I will therefore take pains here to define concepts and terms which, far from being merely a fashionable adornment to historiographic prose or restricted in their relevance only to literary texts, do in fact signal important methodological choices. These choices have determining effects on substantive interpretations of all manner of individual texts and material remains, as well as on broader reconstructions of ancient institutions and social behavior. Hence the necessity of the philosophical “detour” (as Ricoeur would call it) that I must take here in order to progress with confidence toward my historiographic goal.

I should state at the outset, however, that I reject the “radical hermeneutics” of authors such as Jacques Derrida and Michel Foucault, whose approach to textual and historical interpretation has been adopted increasingly of late by scholars intent on deconstructing scholarly depictions of the ancient Near East, and of Canaan and Israel, in particular. With respect to Foucault’s historiography, Charles Taylor and Jürgen Habermas (among others) have pointed out the inconsistency in Foucault’s vision of history as “a series of hermetically sealed, monolithic truth-regimes” (Taylor 1985b:182). Such monolithic relativism “only seems plausible if one takes the outsider’s perspective,” as Taylor puts it, yet this relativism itself implies that a detached perspective is impossible, for whatever truth we propound is deemed subordinate to the system of power in which we are embedded. Paradoxically, however, “Foucault sounds as though he believed that, as an historian, he could stand nowhere, identifying with none of the epistemai or structures of power whose coming and going he impartially surveys” (ibid.).

Following Ricoeur, Taylor, Habermas, and others, I shall argue that the most fruitful course in sociohistorical research lies between the objectivism of traditional positivist historiography and the radical historicism of the poststructuralists. Of course, I can do no more here than summarize the main issues of a complex theoretical debate that has by no means been concluded, but I hope to show the relevance to the study of the ancient Near East of the intertwined developments in hermeneutics and social theory that are outlined below.

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2 Developments in hermeneutics up to the late 1960s, with special attention to the writings of Heidegger and Gadamer, are described in Palmer 1969. Bleicher 1980 is a concise survey that contains well-chosen excerpts from the writings of leading hermeneutical thinkers; and Shapiro and Sica 1984 is a useful anthology of important essays covering a range of hermeneutical issues. Thiselton 1992 provides a detailed survey of a wide variety of recent hermeneutical approaches, with particular emphasis on biblical hermeneutics (cf. Jeanrond 1991). See Dreyfus 1991 for a detailed commentary on Heidegger’s Being and Time, in particular, and Grondin 1994 for a concise exposition and defense of Gadamer’s philosophical hermeneutics. Thompson 1981 is a detailed comparison of the thought of Paul Ricoeur and Jürgen Habermas with respect to social theory (although both of them have subsequently published additional major works). Ricoeur’s essay “The Task of Hermeneutics” (in Ricoeur 1991) is an excellent brief overview of the main insights of Schleiermacher, Dilthey, Heidegger, and Gadamer in light of Ricoeur’s own contribution.

3 So-called deconstructionist approaches are more prominent in biblical studies than in ancient Near Eastern studies as a whole (but see Asher-Greene and Asher 1998; for biblical studies, see the survey in Thiselton 1992:103–32). A recent example concerning the history of ancient Israel is Keith Whitelam’s The Invention of Ancient Israel (1996), which builds on Edward Said’s (1978) critique of “Orientalism,” which itself draws upon the ideas of Michel Foucault; cf. also Silberman and Small 1997 on the relationship between political ideology and archaeology in modern Israel. Much of this recent work takes the form of an objectively presented moralizing critique of the iniquities of previous scholarship, and so is actually not very postmodernist, being more dependent on the longstanding Marxist tradition of ideology-critique than on Derrida’s notion of deconstruction, which dwells on the indeterminacy of all textual meaning, or on Foucault’s notion of the relativity of “truth” as a function of power.

1. Explanation and Understanding

Early in the nineteenth century, the German theologian Friedrich Schleiermacher was the first to propose a general hermeneutical theory that transcended any particular textual genre (biblical, legal, or literary) and instead aimed to describe the conditions and process of understanding as such. Schleiermacher rearticulated the old concept of the “hermeneutical circle,” the paradoxical dialectic of grasping the whole and analyzing the parts that constitutes every act of understanding. He refined the idea of the circular nature of interpretation—that the whole can only be understood from the parts, even as the parts can only be understood from the whole—by distinguishing two related operations which constitute this circle. These operations are the “divinatory” leap of intuition involved in the subjective grasp of an author’s individual mental experience, and the objective “grammatical” analysis of the author’s linguistic and historical context.

Underlying Schleiermacher’s description of the “art of understanding,” however, was a Romanticist model of interpretation as a dialogue in which one aims to think the thoughts of another—that is, to use language as a medium through which one reconstructs and appropriates the inner mental experience of a dialogical partner. In hindsight, this sort of psychologism, which tries to go behind language to thought, was shown to be highly problematic, and more recent hermeneutical theory has been skeptical of the Romanticist faith in the possibility of gaining direct access to intentions and mental processes.

Later in the nineteenth century Wilhelm Dilthey attempted to extend and refine Schleiermacher’s hermeneutical insights. In reaction to the increasing dominance of positivist “scientism” or “naturalism”—the application of natural-science models of interpretation in all areas of scholarship, which was inspired by the impressive progress of the physical sciences—Dilthey drew a sharp distinction between the natural sciences and the “cultural” or “human” sciences (Geisteswissenschaften). According to Dilthey, interpretation in the natural sciences is characterized by the “explanation” (Erklären) of particular occurrences in terms of general “laws” or externally observable statistical regularities, whereas the human sciences rely on intuitive “understanding” (Verstehen). Natural scientists necessarily develop abstract conceptions and procedures by which they isolate objects of study in the physical world that are detached as much as possible from the investigators’ personal experiences. The natural scientist seeks recurrent and objectively verifiable relationships among natural phenomena that can be explained without regard to the scientist’s individual identity and social milieu. The human sciences, however, deal with phenomena that cannot be abstracted from the investigator’s own experiences as a human being, and therefore cannot be explained from a purely external point of view. Indeed, for Dilthey the researcher’s own “lived experience” (Erlebnis) within a given historical setting is the necessary basis for interpreting any cultural phenomenon, not only in the humanities but also in disciplines such as history, economics, sociology, and political science (see Dilthey’s Introduction to the Human Sciences [1989], first published in 1883).

This sort of inner lived experience is not characterized by conceptual abstraction but consists of the immediate pretheoretical meanings and impressions stimulated by interaction with one’s physical and social world. As Fritz Ringer puts it, Dilthey is referring here to “an initially unanalyzed complex of present sensations, memories, and anticipations, of perceptions, intentions, and evaluative orientations” (Ringer 1997:27). Individual lived experience in turn is expressed and “objectified” in the form of social institutions and actions—not least of which are the social actions involved in the creation of those written and nonwritten works that are preserved and studied by later generations.

Dilthey maintained that what the human sciences aim to interpret, in distinction from the natural sciences, are just such expressions of human experience, namely, the laws, literature, art, sciences, religions, and political and economic structures which constitute “culture.” But these cultural phenomena are not to be explained in terms of externally derived laws in the manner of the natural sciences, as the influential positivists of his day would have it. They must rather be understood on the basis of the researcher’s own meaningful experience of human society, which forms the indispensable starting point for every human science. To set this starting point aside is to ignore the distinctive origins and functions of cultural phenomena, which are rooted not in natural laws devoid of human meaning but in the particular human motivations and historical experiences that these cultural phenomena express. And it is because the motivations and experiences of others can only be grasped by analogy with one’s own motivations and experiences that the intuitive understanding of cultural expressions of human experience plays a central role in Dilthey’s methodology of the human sciences. This approach is summed up in his famous epigram: “The one who studies history is the one who makes history.”
For Dilthey, understanding in the human sciences begins with the objectified expressions that constitute culture, but it moves from these outer expressions to a “reliving” or “reenactment” (Nachleben) of the lived experience of others. Obviously, he risked a crippling subjectivism here, because a researcher’s understanding of cultural phenomena will inevitably be conditioned by his or her historical setting and personal experiences. But Dilthey argued that understanding as the characteristic mode of interpretation in the human sciences could indeed be objective, in the sense that all attempts at understanding have as their ultimate goal the understanding of human history as a whole. Because all participate in a common historical life there is a level at which the understanding of individual expressions of that life is not merely subjective, for such understanding contributes to a common self-understanding of what it means to be human. By studying objectifications of lived experience, the human sciences increasingly approach a comprehensive and objectively valid understanding of the totality of human experience, even though this goal can never finally be achieved. For Dilthey, the totality of history is the whole “text” that provides the necessary context within which the many parts of that history must be interpreted, and in light of which all provisional interpretations may eventually be reconciled. The human sciences, then, are simply the hermeneutical circle writ large.

In spite of his concern with the ultimate objectivity of intuitive understanding as the mode of knowledge peculiar to the human sciences, Dilthey also insisted that human existence is fundamentally historical, being rooted in a given situation; thus all interpretations are historically conditioned. But he was able to reconcile this historical relativism with his concern for objectivity only because of his rather optimistic Romanticist belief that there is a basic unity and continuity in the multitude of empirical human cultures, a unity which can be recognized and can contribute to a shared self-understanding of human beings as historical beings in the present. Accordingly, he attempted to escape radical historicism through an appeal to the totality of cultural expressions as the “objective spirit” or “mind” (objektiver Geist) to which one’s own lived experience provides access.

Many, however, have been doubtful of Dilthey’s attempt to secure the objectivity of the human sciences through an appeal to shared participation in the totality of human experience. There are echoes of Hegel’s absolute idealism in Dilthey’s appeal to “objective spirit,” and this is odd because Dilthey himself explicitly rejected Hegel’s metaphysics and Hegel’s correspondingly speculative and teleological philosophy of history. Indeed, in contrast to Hegel he insisted on an empirically grounded historical method and on the need to understand each historical epoch on its own terms.

In this he followed the great nineteenth-century historians Leopold von Ranke and Johann Gustav Droysen, who did so much to professionalize historical scholarship and whose anti-Hegelian historical approach Dilthey himself refined and articulated. Thus Dilthey’s approach was not idealist and speculative at heart, but rather historicist and empirical. In fact, it was for this very reason that Dilthey, like Ranke and Droysen, emphatically rejected the ahistorical Anglo-French positivism of the day, exemplified by Auguste Comte, John Stuart Mill, and Émile Durkheim, who advocated the use of natural-science models of explanation in sociohistorical research, thereby expressing a scientistic version of Hegel’s “top-down” holistic evolutionism.

But despite their rejection of positivism, the problem of objectivity continued to plague Dilthey and his fellow historicists, for they nonetheless retained a view of what constitutes secure and objective knowledge that is fundamentally the same as that which underlies positivism. This produced a contradiction between the historical relativism implicit in Dilthey’s approach and his overriding concern for some kind of scientific objectivity. As Gadamer has pointed out, this contradiction arose because Dilthey continued to assume that the Cartesian dichotomy between a disembedded knowing subject and its objects of consciousness must be the starting point for any theory of knowledge, an assumption that was later to be radically challenged by Heidegger. Thus Dilthey’s questionable resort to a quasi-Hegelian objective totality of human experience was motivated by a typically positivist view of the nature of objective knowledge. As Richard Palmer (1969:99) puts it: “We sense in Dilthey some of the fundamental conflicts in

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5 The British archaeologist and philosopher of history R. G. Collingwood (1946) later adopted a similar approach, stressing the importance in historical scholarship of the imaginative “reenactment” of past experience.

6 Ranke is often mistakenly called a positivist because of his concern to professionalize history as a rigorous “scientific” discipline which avoids value judgments. In philosophical terms, however, Ranke espoused historicism, not positivism, and he believed that historical scholarship plays an important cultural and political role in the present (see Iggers 1997:29). The confusion arises because of the unresolved tension in Ranke (and in German historicism as a whole) between the desire for scientific objectivity and the relativism inherent in his philosophical position.
nineteenth-century thinking: the Romantic desire for immediacy and totality even while seeking data that would be ‘objectively valid.’”

In this regard, Dilthey has been criticized for falling prey to the very scientism that he set out to oppose, for in the end he attempted to impose the objectivity of the natural sciences within the domain of the human sciences that he had so sharply distinguished from them. Gadamer (1989:231–42), in particular, has criticized Dilthey on this score, and in his own work he has rejected any attempt to impose the natural-science ideal of objective method on the human sciences. Others, however, fault Dilthey for establishing too rigid a dichotomy between the modes of interpretation that obtain in the human versus the natural sciences, neither of which is as “objective” as the nineteenth-century positivists assumed. Subsequent work in the philosophy of science and in social theory has cast doubt on the view that the natural sciences are strictly nomological and the human sciences are necessarily particularistic or “idiographic.” Indeed, although leading hermeneutical theorists such as Heidegger and Gadamer oppose the use of objectivist explanatory methods in the human sciences, their view has been challenged even within the camp of hermeneutics by Ricoeur, who sees a necessary dialectical relationship between the hermeneutical understanding of human motivations and objectifying sociohistorical explanations in terms of causal laws (see the essay “Explanation and Understanding” in Ricoeur 1991).

Ultimately, as Ricoeur has argued, Dilthey’s failure to free himself from Schleiermacher’s psychologism, which is not entirely expunged from Gadamer’s philosophy, is at the root of the too-rigid dichotomy between the human and natural sciences that has plagued hermeneutics. Dilthey retained a Romanticist optimism concerning the possibility of somehow intuitively reliving the inner experiences of others. Thus he neglects the role to be played by objectifying and distanciating explanation, as does Gadamer, who, despite his critique of Dilthey and of Romanticist hermeneutics, underestimates the barriers to understanding within the human sciences which objectifying explanation must be called upon to remove. But in twentieth-century thought, the influence of Marx, Nietzsche, and Freud—the three great “masters of suspicion,” as Ricoeur has called them—has been such that few any longer share Dilthey’s naïve faith in an immediate and transparent understanding of others, or even of oneself.

In spite of these criticisms, subsequent hermeneutical thinkers have been indebted to Dilthey for his emphasis on the primacy of actual lived experience as the basis for all interpretation in the human sciences, and for his related view of the fundamental “historicality” (Geschichtlichkeit) of human existence. In the realm of sociohistorical method, moreover, Max Weber went on to develop an “interpretive” (verstehende) approach that rejected Dilthey’s psychologism but preserved and clarified his key insights. Weber followed Dilthey in emphasizing the necessity in sociohistorical research of employing an intuitive understanding of typical motivations for action, rather than simply studying social action from an external point of view. But by formalizing the researcher’s understanding of such motivations in terms of objectively defined “ideal types,” Weber showed that it is indeed possible to proceed in a scientific manner in proposing and testing recurrent “causal” connections between inner experiences and their outer cultural expressions (to use Dilthey’s terms), thereby connecting subjective understanding to a form of objective explanation. Weber’s “causes,” however, are not deterministic laws but are objective regularities or statistical probabilities conceived in terms of the typical logical connections that exist between patterns of subjectively meaningful actions (see Kalberg 1994; Ringer 1997).

Weber’s methodology and the sociohistorical implications of Dilthey’s distinction between explanation and understanding will be discussed in more detail below in chapter 3, but it is worth pointing out here that many archaeologists and historians of the ancient Near East have tended to neglect the important methodological role played by the understanding of ancient lived experience, preferring instead to devise explanations in terms of general laws or external functional exigencies that are thought to account for observed regularities in social behavior. In Near Eastern archaeology, in particular, various forms of positivist science have been influential for decades. Unfortunately, there is still only limited awareness of the serious critiques of positivism which have been advanced over the past century and of the complex methodological debate that was initiated by Dilthey and his contemporaries and continues to this day. Yet this debate is highly significant for research on the ancient Near East, in my view, because it calls into question a number of widely accepted assumptions concerning the structure of ancient society and the causes of social change.
2. Lifeworld and Environment

Hermeneutics took a markedly different path after Dilthey with the publication of Martin Heidegger’s *Being and Time*. For Heidegger, hermeneutical theory is not simply a matter of the methodology of the human sciences; rather, it is the basis for a general philosophy of human existence. Interpreting is not just something that human beings do as disembodied knowing subjects who have somehow detached themselves from the “objective” environment in which they live. More fundamentally, interpretation is what human beings are, because to be human is first of all to understand in some way the experiential situation, the “lifeworld,” into which one has been “thrown.” This understanding is revealed in the first instance not in theoretical terms but in our habitual, skillful practices or “know-how,” as we cope with the world in which we live. As Dilthey had emphasized (although he himself failed to develop this point adequately and so became entangled in self-contradiction, tending to relapse into objectivism), historically situated lived experience, involving our motivated practices in the world, is more basic than any theoretical abstraction predicated on the distinction between subject and object. Pretheoretical experience of the lifeworld in terms of embodied practical activity must therefore be the starting point for any theory of knowledge, and for any philosophy of human existence in general.

In emphasizing the fundamental status of the lifeworld, Heidegger, like Husserl, thereby rejected both Romanticist subjectivism and positivist objectivism, attempting to move beyond the impasse between them which had developed in nineteenth-century philosophy. Despite their differences, these two opposing approaches are both characterized by “the search for a reality before and behind the cultural world to which that world can be reduced” (Rabinow and Sullivan 1987:9), a search that Romanticism conducts in an idealist fashion and positivism conducts on the basis of atomist and materialist assumptions. But it is this deeply rooted presupposition of an externally observable social reality ontologically prior to individual lived experience which Heidegger called into question, with important implications for the practice of the human sciences.

It should be noted immediately, however, that accepting Heidegger’s antioobjectivist view does not commit us to radical skepticism concerning the objective validity of scientific knowledge of our physical environment, as some have argued. A distinction must be made here between the study of the cultural world as opposed to the natural world. Heidegger by no means denied that the natural sciences can arrive at objectively true descriptions of a natural reality that is prior to individual human experience, even though he was not a metaphysical realist in the traditional sense. Thus Hubert Dreyfus (1991:251–65) defends what he calls Heidegger’s “minimal hermeneutic realism” against the skeptical relativism concerning the objectivity of scientific knowledge championed by Richard Rorty and others (cf. Guignon 1991). As Dreyfus puts it:

Anyone who claims to have a description of ultimate reality claims a point of view outside of all particular, finite interpretations, and both Heidegger and Rorty think, given their understanding of understanding, that the very idea of such an interpretation-free understanding of what ultimately is does not make sense. But Rorty seems to think that this is an argument against minimal hermeneutic realism, whereas I (and Heidegger too, I have argued) would say, on the contrary, that one can reject the claim that there is a correct description of reality and still hold that there can be many correct descriptions, including a correct causal description of objectified physical nature.

Note that it was Heidegger’s mentor Husserl who favored the term “lifeworld” (*Lebenswelt*), whereas Heidegger simply uses the term “world” (*Welt*), in two quite different senses, to designate what I am distinguishing here as lifeworld versus environment (see Dreyfus 1991:89ff.). Heidegger contrasts the physical world or universe (environment) with the meaningful world of human activity (lifeworld), distinguishing between “the ‘world’ . . . as a totality of objects and the world as the organized equipment and practices in which *Dasein* [being human] is involved” (ibid., p. 248). As our own experience constantly demonstrates, the lifeworld is ontologically prior to the objectified environment; indeed, our perception of an externally observable environment consisting of independently existing objects (including the self viewed as one object among others) depends upon our prior experience of an immediate lifeworld in which this distinction has not yet been made.

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8 Dreyfus (1991:252f.) elaborates on this point by saying that: “It is a non sequitur to claim that because physical theories are developed by means of scientists’ practices and the authority of science is constituted by way of other social practices, physics does not discover truths about nature and so has no legitimate authority. . . . If one wants to undermine the illegitimate authority of natural science, especially physics, in our culture, it would be sufficient to demonstrate that although natural science can tell us the truth about the causal powers of nature, it does not have a special access to ultimate reality. This is exactly what Heidegger attempts to show. It might at first seem that Heidegger’s way of answering skepticism, viz. that we have direct access to objects in our world because, as absorbed openness, we are the shared social world existingly, commits him to the view that we cannot have access to an independent reality—that we can never know the contents of the uni-
Setting aside the question of objectivity in the natural sciences, it is clear that for Heidegger, as for Dilthey, the human sciences present quite different problems. It may be possible to formulate “a correct causal description of objectified physical nature,” but the same cannot be said of human culture and history, in which the observer is already implicated as an interpreter and self-interpreter—that is, as a human being. The hermeneutical approach thus challenges the scientific assumptions of a sociohistorical positivism that proposes a natural-science type of causal description of an objectified social world. On the other hand, Heidegger rejected the idealist notion of an independent realm of “mind” or “spirit,” accessible through one’s subjective intuition, to which one can appeal in accounting for human culture.

In rejecting both positivist objectivism and idealist subjectivism, Heidegger adopted Husserl’s phenomenological critique of the Cartesian dualism of mind and body which had come to dominate Western intellectual life (see Kockelmans 1967 on Husserl and the phenomenological approach he pioneered; for more recent assessments, see Smith and Smith 1995). At the beginning of the modern period in the seventeenth century, René Descartes had argued that the only absolutely certain starting point for human knowledge was the fact that “I think” (ego cogito)—the thinking subject conceived of as a unitary substance, over against which are objects of consciousness conceived of as mind-independent things-in-themselves. Descartes maintained that we have direct knowledge only of our own inner mental states, not of external objects; yet he failed to explain adequately the relationship between these mental states and objects in the external world.

Faced with the paradoxes of Cartesian dualism, which in some circles led to radical skepticism concerning the possibility of objective knowledge of the world, Immanuel Kant subsequently argued that our knowledge of the world is not a matter of the mind conforming to objects, but rather of objects conforming to the mind. Thus in place of the Cartesian dichotomy between subject and object, Kant proposed a “Copernican revolution” in which objects of knowledge are viewed not as independent of the mind but as actively constructed in conformance with the pre-existing structure or “categories” of the mind. Indeed, Kant saw “mind” itself not as a Cartesian substance contemplating other substances, but rather as a synthesesizing activity or process. Kant did not deny the existence of objective things-in-themselves, but he argued persuasively that our knowledge of them is always mind-dependent at some level. Kant’s approach has been enormously influential, but the course of nineteenth-century philosophy seemed to show that post-Kantian constructivism resulted in one variety or other of unsatisfying relativism and subjectivism. For those who wanted to find a secure basis for objective knowledge, a new approach was felt to be necessary.

Husserl joined the renewed quest for certainty at the beginning of the twentieth century. Unwilling to accept the relativism that seemed to be inherent in Kant’s solution, he went back to the fundamental issues that Descartes had raised, although like Kant he rejected Cartesian dualism and the positivism or “naturalism” it had inspired in the human sciences. Building on the insight of the psychologist Franz Brentano that consciousness does not simply occur in itself but is always directed or intentional—it is always consciousness of something—Husserl argued that the theory of knowledge cannot start with an illicitly substantivized knowing subject who is somehow detached from independently existing objects of consciousness. For Husserl, the fundamental epistemological starting point is not Descartes’s lonely ego cogito but the ego cogito cogitatum—the irreducible dialectic between what is known (noema) and the intentions and anticipations of the knower (noësis) through which the very identity of the self in its world is constituted. To explore this dialectic, Husserl advocated the “bracketing” (epoché) or suspension of all of one’s culturally inherited assumptions in order to pay attention to phenomena as they actually appear in one’s immediate lifeworld (hence the name “phenomenology”).

More disputably, however, Husserl believed that it is possible in this way to obtain secure “apodictic” knowledge of things-in-themselves. By means of phenomenological bracketing, Husserl held that we can achieve certainty through direct intuition of the “essence” of what we observe. But this is perilously close to the idealism he sought to avoid, for it implies an atemporal realm of ideas lying behind the flux of experience. It could be said that Husserl bracketed all of his assumptions except the assumption that there are ideal essences. As a result, although many philosophers were attracted to Husserl’s phenomenological method and accepted the necessity of suspending the naturalistic assumptions we have inherited from Cartesian rationalism, few could accept Husserl’s claim that this method provides apodictic knowledge of the kind that he sought.
Heidegger, in particular, took over many elements of Husserl’s phenomenological approach while rejecting his claim to have attained absolutely valid knowledge. More fundamentally, he rejected Husserl’s intellectualist tendency to regard the human being as primarily a disinterested spectator for whom objective certainty is of paramount importance. For Heidegger, a phenomenological analysis of human experience shows that the sheer undeniable facticity of human existence, of the lifeworld, is more basic even than human consciousness; thus he questioned Husserl’s emphasis on the acquisition of disinterested knowledge at the expense of considering other, more fundamental modes of being human (or, as he called it, Dasein, “being there”). Heidegger’s own phenomenological analysis demonstrated that, once we shed our Cartesian spectacles and attend to human existence as it actually appears, our human “being-in-the-world” is seen to be characterized above all by “care” (Sorge).

“Care” is oriented toward and reflects the fundamental temporality or historicality of human existence, because one’s temporal situation supplies both the preconditions for and the future horizon of one’s ongoing experience of the world. This lived experience is not disinterested but involves memory, intention, and anticipation—the active “projection” of oneself into an undetermined future which constitutes a “living-ahead.” For Heidegger, being human is thus not a matter of reacting passively to things in the external world; rather, it is constituted by active involvement with things. Phenomenological analysis reveals that objects as we experience them most basically in our lifeworld are inherently available (zuhanden, “ready-to-hand”), having some potential value or use for us, and are not merely occurrent in the environment (vorhanden, “present-at-hand”), as in the Cartesian worldview.9

And this is where hermeneutics reenters the picture, although at a far more basic level than even Dilthey had proposed. For according to Heidegger, we understand something only inasmuch as we actively experience it as available to us for some purpose. Interpretation is first and foremost a matter of making explicit the possibilities that things have for us, of which we are implicitly aware as soon as we experience them pretheoretically as ready-to-hand objects in our lifeworld. Understanding is itself what constitutes the projection or living-ahead that fundamentally characterizes human being-in-the-world, as we actively consider alternative possibilities that will concern us in one way or another in the future.

As Richard Palmer puts it:

For Heidegger, understanding is the power to grasp one’s own possibilities for being, within the context of the lifeworld in which one exists. It is not a special capacity or gift for feeling into the situation of another person, nor is it the power to grasp the meaning of some “expression of life” on a deeper level [contra Dilthey]. Understanding is conceived not as something to be possessed but rather as a mode or constituent element of being-in-the-world. It is not an entity in the world but rather the structure in being which makes possible the actual exercise of understanding on an empirical level. Understanding is the basis for all interpretation; it is co-original with one’s existing and is present in every act of interpretation. [Palmer 1969:131]

An important implication of this view is that interpretation can never proceed from an Archimedean point outside of concrete historical experience (a “view from nowhere”) but always presupposes an already existing lifeworld. Here Heidegger elaborated Husserl’s concept of the lifeworld (called simply “world,” Welt, in Heidegger’s terminology), emphasizing the extent to which the human lifeworld is logically prior to and distinct from the objective world or environment that is observed from an external scientific perspective. Unlike the environment, the lifeworld cannot be contrasted with the “self” because this would assume the very subject-object dichotomy that the notion of lifeworld is intended to oppose. The lifeworld as the primary datum of human existence is prior to this conceptual distinction. Indeed, the self-versus-environment distinction that contrasts the thinking subject with neutral objects, as useful as it may be, is itself the product of a particular and derivative mode of understanding. As Charles Taylor says:

Heidegger’s intention is plainly other than just reminding us of what it is like to live in the world at an everyday level. . . . The aim is to show that grasping things as neutral objects is one of our possibilities only against the background of a way of being in the world in which things are disclosed as ready-to-hand. Grasping things neutrally requires modifying our stance to them that primitive has to be one of involvement. Heidegger, like Kant, is arguing that the comportment to things described in the disengaged view [of modern rationalist epistemology] requires for its intelligibility to be situated within an enframing and continuing stance to the world that is anti-

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9 On the use of the terms “available” and “occurrent” instead of “ready-to-hand” and “present-at-hand,” as in the standard English translation of Being and Time, see Dreyfus 1991:xi. Dreyfus provides a detailed discussion of Heidegger’s concepts of “availableness and occurrentness” (ibid., pp. 60–87).
The lifeworld in itself is thus neither subjective nor objective; it is the relational whole that is primordially given as the medium of human existence. Moreover, within this relational whole is embedded a structure of meaning that prestructures understanding because it is the ground from which understanding as the projection of human possibilities takes off. This view of understanding renders meaningless the very notion of a totally “objective” interpretation without historically contingent presuppositions. The hermeneutical circle from preunderstanding to new understanding is part of the very structure of human existence, and one’s temporally situated preunderstandings are the only point of entry into this circle. Thus no scientific or scholarly investigation is merely empirical. Empirical evidence is always theory-dependent, at some level, because what counts as evidence and how that evidence is interpreted are influenced by the researcher’s own preunderstandings.

The implications for sociohistorical research of this concept of a primary, interpreted lifeworld, as opposed to the derivatively objectified environment that is studied by scientific means, will be discussed below in chapter 3. In particular, this distinction is related to the problematic relationship between individual “agency” and constraining social “structure” that has been the subject of much debate in social theory. To put it another way, the distinction between lifeworld and environment prompts consideration of the structuring role in social life of subjectively meaningful symbols, including such symbols as the ancient Near Eastern “house of the father.” These structuring symbols are rooted in an immediate lifeworld of everyday lived experience, on the one hand, but they also take on a life of their own as part of what may be studied scientifically as the “objective” environment of individual social action, being inculcated as normative constraints on action through the process of socialization.

Objectifying explanatory methods may therefore be called upon to elucidate recurring interconnections among meaningful symbols and various environmental facts, including social facts, provided that the motivated practices of individual persons (so-called engaged or embodied agency) remain the ultimate basis of sociohistorical analysis. The patterns of motivated action we call social structures can indeed be viewed as external facts, much like physical features of the environment. On the other hand, we must avoid illicitly reifying these structures by regarding them as collective entities that exist somehow outside of the temporal events of individual experience.

The crucial question of whether to adopt this sort of “methodological individualism” rather than “methodological holism” in sociohistorical research is treated from a philosophical perspective in Ricoeur’s essay “Hegel and Husserl on Intersubjectivity” (reprinted in Ricoeur 1991; see also Ricoeur 1978: 135ff.). Hegel’s ontology of “objective spirit” underlies the sociological holism of Marx and Durkheim, among others. This holism has exercised a powerful, if often unacknowledged, influence for more than a century, and it is one source of the objectivism that characterizes both functionalism (whether Marxist or non-Marxist) and French structuralism.

The hermeneutical answer to Hegelian holism is Husserl’s concept of “intersubjectivity,” described in terms of the “analogy of the ego”—the capacity to understand the behavior of others in terms of one’s own motivations, as the actions of “another I” (an “alter ego”) who is like oneself.10 The intersubjective individualism of Husserl and his followers does not picture human beings as isolated autonomous individuals—it does not deny that, as Ricoeur (1978:138) puts it, “human phenomena can . . . be regarded as irreducibly social for the purposes of description and explanation.” At some level, sociohistorical explanations must necessarily refer to the behavior of groups (“we”), rather than merely to the actions of isolated individuals (“I”). But, as Ricoeur says:

One can indeed ask whether, to understand “we,” it is not first of all necessary to know the meaning of “I.” But such a derivation of “we” from “I,” as sketched by Husserl . . . consists in saying that one cannot understand the meaning of an institution, a group belief or a collective symbolism unless these phenomena are related to a “we.” But one can speak of “we” only

10 Ricoeur notes that Husserl’s theory of intersubjectivity, which was developed further by Alfred Schutz (1967), rests on “the principle of analogy implied in the initial act of pairing between diverse temporal fields, those of our contemporaries, those of our predecessors, and those of our successors. These fields are analogous in the sense that each of us can, in principle, exercise the ‘I’ function in the same way as everyone else and can impute to himself or herself his or her own experience. It is here, as we shall see, that imagination is implied. . . . It is the transcendental principle according to which the other is another self similar to myself, a self like myself. The analogy proceeds here through the direct transfer of the signification ‘I.’ Like me, my contemporaries, my predecessors, and my successors can say ‘I.’ It is in this way that I am historically bound to all others” (Ricoeur 1991:179f.).
if every member of it can say “I”; thus meaning proceeds from “I” to “we” and thence to the group, the beliefs, the institutions. [ibid.]

According to Ricoeur, Husserl’s innovation consists in “his uncompromising refusal to hypostatize collective entities and... his tenacious will to reduce them in every instance to a network of interactions” (Ricoeur 1991:244). Furthermore, Ricoeur notes that the validity of Husserl’s abstract theorizing about networks of intersubjective interactions is confirmed in Max Weber’s interpretive sociology, because Weber supplies the necessary methodological articulation and empirical content to make the concept of intersubjectivity persuasive. Weber’s approach refuses to reify the patterns of individual social action we call institutions in terms of the natural-science metaphors of “organism” and “system” (understood as stable or homeostatic systems) that became so pervasive in sociology after Durkheim. Rather, interpretive sociology responds to the appearance of objectivity accorded to institutions and aims at reducing it to the predictability of a certain course of action. It is the probability of a certain course of action that we reify into a separate entity.... By their statistical regularity, certain relations behave like things.... This is why the sociologist can be satisfied with this “naive” position and base the postulation of some collective entity on the laws governing empirically established regularities. The critical epistemology of sociology, however,... must dissipate such precritical naiveté. What one is ultimately required to presuppose is a certain course of action ascribable to these or those members of society, hence a certain course of motivation [described objectively in terms of ideal types]... To speak of an organization, even if it is the State, is to speak of a certain probability of action.... Weber denounces in this regard the trap of organic metaphors; they have, for him, at most a heuristic value. They allow us to identify and delimit the realities to be described; the trap is to take the description of an organic totality for an explanation capable of being substituted for interpretive understanding.... This systematic work of desubstantializing collective entities is pursued with great fervor by Weber in what follows in the great descriptive and programmatic chapter of Economy and Society. [Ricoeur 1991:242f.]

Husserl’s concept of intersubjectivity is therefore mirrored in Weber’s definition of “social action” as “meaningfully oriented behavior” (sinnhaft orientiertes Verhalten). Social action is not just any human behavior, but behavior that is subjectively meaningful for the individual actor because it is oriented intersubjectively to the actor’s understanding of the behavior of others. Hence Weber’s well-known definition of sociology as a science concerning itself with the interpretive understanding of social action and thereby with a causal explanation of its course and consequences. We shall speak of “action” insofar as the acting individual attaches a subjective meaning to his behavior—be it overt or covert, omission or acquiescence. Action is “social” insofar as its subjective meaning takes account of the behavior of others and is thereby oriented in its course. [Weber 1978:4]

Thus the longstanding methodological debate between intersubjectivist individualism and objectivist holism in sociohistorical research—between “Weberian” and “Durkheimian” approaches—reflects a fundamental philosophical issue. Indeed, according to Ricoeur (1991:244), what is ultimately at stake is our view of what it means to be human beings who can exercise “critical agency,” for “[t]he substitution of intersubjectivity for the Hegelian objective spirit preserves... the minimal criteria of human action, namely, being able to identify this action through the projects, intentions, and motives of agents capable of imputing their action to themselves. Let these minimal criteria be abandoned and one begins again to hypostatize social and political entities, to raise power to the heavens, and to tremble before the State.”

Note, again, that this sort of methodological individualism does not necessarily imply Lockean atomism (see Taylor 1985b:187–210). Because human society and language are intersubjective phenomena, individual persons are not simply autonomous atoms who cooperate on a contractual basis to achieve utilitarian ends. In an important sense, individual persons are themselves constituted by intersubjective social interaction within a given cultural setting. But this does not lessen the analytical primacy of individual motivated action in sociohistorical research. A philosophical starting point in the phenomenology of human existence as we experience it, rather than in the reified rationalist abstraction of the subject-object dichotomy, leads us to give analytical priority to the subjective meanings of human action, for to do otherwise is to neglect our distinctively human character as “self-interpreting animals” (Taylor 1985a). The anthropologist Clifford Geertz (1973:5) expresses this idea with characteristic flair in his essay “Thick Description: Toward an Interpretive Theory of Culture”: “Believing, with Max Weber, that man is an animal suspended in webs of significance he himself has spun, I take culture to be those webs, and the analysis of it to be therefore not an experimental science in search of law but an interpretive one in search of meaning.”

For our purposes, it is important to note that Weberian methodological individualism (which follows
from the hermeneutical emphasis on the ontological priority of the lifeworld of meaningful lived experience over the secondary and derivative self-versus-environment distinction) provides a theoretical basis for integrating “interpretive/symbolic” and “functionalist/ecological” modes of investigation in the study of past societies, without permitting either to neglect the other. In the academic disciplines of archaeology and history, as currently constituted, there is a recurring tendency to ignore either interpretive understanding or functionalist explanation, and to identify the particular subject matter and techniques of one’s discipline exclusively with only one of these intellectual modes. This creates problems of miscommunication and mutual misunderstanding, especially among researchers of different backgrounds (i.e., “humanistic” versus “social-scientific”) who are engaged in the historical archaeology of past cultures that have left both written and nonwritten remains.

As we shall see, the hermeneutical approach can make room for atemporal or synchronic generalizing explanations, as long as they take their place within the framework of the temporal and fundamentally narratival understanding of sequences of contingent motivated actions. Indeed, Ricoeur has shown that the hermeneutical approach itself depends on such explanations. Nevertheless, the ontological primacy of the subjectively meaningful lifeworld—the priority of “engaged agency” before and behind any subject-object dichotomy—means that an intuitive understanding of motivations (“reasons for acting”) must encompass and integrate every explanatory model. I will discuss this further in the next chapter, “The Sense and Reference of Human Works,” where I outline my own view of the nature and limits of socio-historical explanation in archaeology per se, and thus archaeology’s relationship to text-based history. But first we must survey the important developments in post-Heideggerian hermeneutics, as found in the works of Gadamer and Ricoeur.

3. Distanciation and Belonging

Heidegger radically redefined hermeneutics by shifting the focus to “fundamental ontology” rather than remaining within the realm of the epistemology of the human sciences, with which Dilthey had been concerned. As Ricoeur (1991:64) puts it, “a new question is raised: instead of asking, how do we know?, it will be asked, what is the mode of being of that being who exists only in understanding?” This shift was necessary in order to escape the insoluble problems raised by conceiving of interpretation solely in terms of knowing subjects and their objects of knowledge, for at the level of epistemology the hermeneutical circle between historically conditioned preunderstandings and new interpretations has the appearance of a vicious circle. In the human sciences the subject is also the object, as Dilthey took pains to assert; thus the interpreter cannot rid himself of prejudice with respect to what is to be interpreted, casting doubt on the possibility of any sort of objectivity. When viewed from the perspective of the ontological structure of human existence, however, pre-understanding or prejudice is not a methodological failure but rather a positive aspect of the projection or anticipation that constitutes being-in-the-world: “prejudice can be understood only in terms of the anticipatory structure of understanding. The famous hermeneutical circle is henceforth only the shadow, on the methodological plane, of this structure of anticipation” (Ricoeur 1991:68).

It is necessary, however, to make the return journey from ontology to epistemology. Otherwise, hermeneutics has nothing to say to practitioners of the human sciences and its universal claims with respect to the nature of all interpretation remain unsubstantiated. The path from ontology back to epistemology was not taken by Heidegger himself, but the obstacles along its way have been studied by his student Hans-Georg Gadamer, who saw the need to relate Heidegger’s ontology to the ongoing debate over the methodology of the human sciences. In his discussion of epistemology, Gadamer is sharply critical of all attempts to devise an objective methodology for the human sciences. He sees in objectifying explanatory methods (e.g., the “statistical and formal methods” in historical research that are “fostered by the acceptance of Anglo-American methods and modes of inquiry” [Gadamer 1989:551]) the illicit imposition of a positivist model of knowledge, borrowed from the natural sciences, which introduces a destructive distance between the interpreter and the thing to be interpreted.

It has frequently been remarked that Gadamer’s famous book Truth and Method might better have been entitled Truth or Method, because it revolves around the irreconcilable conflict he sees between the “alienating distanciation” (Verfremdung) inherent in objective method and the relationship of “participatory belonging” (Zugehörigkeit) that characterizes human experience within a given historical stream of tradition. Here, then, in another form, is the “expressivist” reaction to atomistic Enlightenment rationalism that has characterized German Romanticism and its intellectual successors since the late eighteenth century (see Taylor 1979), a reaction which emphasizes the expressive unity of all aspects of human life within one’s historical community.
For Gadamer, the fact that we belong to a particular historical tradition is what makes interpretation possible in the first place. Following Heidegger, he regards human existence as fundamentally both temporal and interpretive. Understanding is our primary mode of being-in-the-world, and it presupposes a temporally situated lifeworld characterized by active participation rather than neutral contemplation. Furthermore, Gadamer (1989:383–491) goes on to show in considerable detail how this lifeworld is mediated by language. What Dilthey called the “historicality” of human existence (Geschichtlichkeit) must be understood in terms of the universal “linguisticality” (Sprachlichkeit) of human experience. Language is not an objective tool that we use for certain purposes, to give utterance to thoughts that are somehow prior to or independent of language. Language is not an object at all, but the basic precondition for any human experience of the world. Moreover, because language is by its very nature intersubjective, it creates a zone of shared understanding between persons. According to Gadamer, this shared understanding can transcend time and place, despite the differences between disparate historical traditions, because the lifeworlds created by language are all human worlds. This is what makes interpretation possible in aesthetic, historical, and textual studies.

Gadamer calls the process of understanding another person or text through language the “fusion of horizons.” As Ricoeur (1991:282) notes, “this is a dialectical concept that results from the rejection of two alternatives: objectivism, whereby the objectification of the other is premised on the forgetting of oneself; and absolute knowledge, according to which universal history can be articulated within a single horizon [as in Hegel’s philosophy].” Both objectivism and absolute knowledge are excluded because interpretation has its starting point in one’s own finite historical horizon and in the preunderstandings or prejudices that it supplies. Yet it is only in the encounter with the horizon of another that one’s prejudices become apparent and productive.

Ricoeur remarks further that “only insofar as I place myself in the other’s point of view do I confront myself with my present horizon, with my prejudices. It is only in the tension between the other and the self, between the text of the past and the point of view of the reader, that prejudice becomes operative and constitutive of historicity” (p. 283). This has important implications for the epistemology of the human sciences, for it means that:

Historical knowledge cannot free itself from the historical condition. It follows that the project of a scientific. 

tence free from prejudices is impossible. History poses meaningful questions to the past, pursues meaningful research, and attains meaningful results only by beginning from a tradition that interprellates it. . . . history as science receives its meanings, at the outset as well as the end of research, from the link it preserves with a received and recognized tradition. The action of tradition and historical investigation are fused by a bond that no critical consciousness could dissolve without rendering the research itself nonsensical. [ibid.]

Our belonging to a lifeworld is therefore effected by a historical tradition that envelops us through language and equips us with the necessary prejudices (in a positive sense) to engage in interpretation. In this way, Gadamer rehabilitates the concepts of prejudice and of the authority of tradition, which had been cast into disrepute by the Enlightenment. At the same time, however, he retains the notion of a “correct” as opposed to an erroneous interpretation. As he says:

the prejudices of the individual, far more than his [critical] judgments, constitute the historical reality of his being. . . . What appears to be a limiting prejudice from the viewpoint of the absolute self-construction of reason in fact belongs to historical reality itself. If we want to do justice to man’s finite, historical mode of being, it is necessary to fundamentally rehabilitate the concept of prejudice and acknowledge the fact that there are legitimate prejudices. Thus we can formulate the fundamental epistemological question for a truly historical hermeneutics as follows: what is the ground of the legitimacy of prejudices? What distinguishes legitimate prejudices from the countless others which it is the undeniable task of critical reason to overcome? [Gadamer 1989:276f.]

This is the key question, of course. Gadamer’s answer to it elaborates on Heidegger’s (1996:143) widely quoted assertion that “what is decisive is not to get out of the [hermeneutical] circle, but to get in it in the right way. . . . [Interpretation’s] first, constant, and last task is not to let fore-having, fore-sight, and fore-conception be given to it by chance ideas and popular conceptions, but to guarantee the scientific theme by developing these in terms of the things themselves.” But how is this done? Gadamer’s comments on this issue are worth quoting at length:

All correct interpretation must be on guard against arbitrary fancies and the limitations imposed by imperceptible habits of thought, and it must direct its gaze “on the things themselves”. . . . A person who is trying to understand a text is always projecting. He projects a meaning for the text as a whole as soon as some initial meaning emerges in the text. Again, the initial meaning emerges only because he is reading the text with particular expectations in regard to a certain meaning. Working out this fore-projection,
which is constantly revised in terms of what emerges as he penetrates into the meaning, is understanding what is there. . . . The only “objectivity” here is the confirmation of a fore-meaning [prejudice] in its being worked out . . . All that is asked is that we remain open to the meaning of the other person or text. But this openness always includes our situating the other meaning in relation to the whole of our own meanings or ourselves in relation to it . . . The important thing is to be aware of one’s own bias, so that the text can present itself in all its otherness and thus assert its own truth against one’s own fore-meanings. [Gadamer 1989:266–69]

Gadamer speaks here rather vaguely of “openness” and of being “pulled up short by the text.” But is this a sufficient guarantee of “correct” interpretation? Jürgen Habermas has attacked Gadamer on just this point. Drawing on the neo-Marxist tradition of the “critique of ideology,” as developed by the Frankfurt School, Habermas challenged the universal scope of Gadamer’s hermeneutics in his book Knowledge and Human Interests (1971) and in subsequent writings. He points out that Gadamer is much too optimistic about the risk of misunderstanding because he does not reckon with the possibility of “systematically distorted communication”:

The area of applicability of hermeneutics is congruent with the limits of normal everyday speech, as long as pathological cases [e.g., psychosis] are excluded. The self-conception of hermeneutics can only be shaken when it appears that patterns of systematically distorted communication are also in evidence in “normal,” let us call it pathologically unobtrusive, speech. This is the case in the pseudo-communication in which the participants cannot recognize a breakdown in their communication; only an external observer notices that they misunderstand one another. Pseudo-communication generates a system of misunderstandings that cannot be recognized as such under the appearance of a false consensus. [Translated in Bleicher 1980:190f.]

For Habermas, it is the task of the “critical social sciences” to unmask the systematic distortions of communication introduced by ideological uses of language that legitimate and institutionalize relations of power. According to Gadamer, however, the alienating distanciation introduced by critical explanatory methods conceals our primordial relationship of belonging to a tradition. Such methods do not reveal the truth concerning human being-in-the-world that is contained in what is to be interpreted; they reveal only the truth (or error) that is implicit in the assumptions of the method itself. Thus they inhibit the properly prejudiced encounter with the historical other that is made possible only by virtue of our belonging to a tradition. Indeed, Gadamer’s riposte to Habermas is to point out that the Enlightenment’s critique of prejudice and tradition as articulated by Kant and Marx is itself a historically situated tradition. The critique of ideology is itself an ideology, for there is no fundamental standpoint outside of ideology from which critical reason can proceed independently of hermeneutics (see Gadamer’s essay “On the Scope and Function of Hermeneutical Reflection,” reprinted in Gadamer 1976).

Ricoeur agrees, noting that hermeneutics can sustain its universal claim in view of the fact that the critique of ideology itself depends on a powerful but nonetheless historically contingent interest in emancipation, which underlies Habermas’s “regulative idea” of undistorted communication that is free from constraint. This interest in emancipation does not come from nowhere but depends on a creative reinterpretation of past tradition:

This tradition is not perhaps the same as Gadamer’s; it is perhaps that of the Aufklärung, whereas Gada-mer’s would be Romanticism. But it is a tradition nonetheless, the tradition of emancipation rather than of recollection. Critique is also a tradition. I would even say that it plunges into the most impressive tradition, that of liberating acts, of the Exodus and Resurrection. . . . If that is so, then nothing is more deceptive than the alleged antinomy between an ontology of prior understanding and an eschatology of freedom. We have encountered these false antinomies elsewhere: as if it were necessary to choose between reminiscence and hope! In theological terms, eschatology is nothing without the recitation of acts of deliverance from the past. [Ricoeur 1991:306]

Gadamer’s defense of prejudice and tradition is provocative and illuminating, and perhaps not as easily defeated as Habermas had supposed. On the other hand, the way in which Gadamer frames the relationship between participatory belonging and alienating distanciation as a stark choice between “truth” and “method” is criticized in turn by Ricoeur from the perspective of hermeneutics itself. Here Ricoeur acknowledges the validity of critical theory’s doubts about the total rejection of objectifying method, arguing that Gadamer establishes an untenable alternative: on the one hand, alienating distanciation is the attitude that renders possible the objectification that reigns in the human sciences; but on the other hand, this distanciation,

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which is the condition of the scientific status of the sciences, is at the same time the fall that destroys the fundamental and primordial relation whereby we belong to and participate in the historical reality that we claim to construct as an object. Whence the alternative underlying the very title of Gadamer’s work *Truth and Method*: either we adopt the methodological attitude and lose the ontological density of the reality we study, or we adopt the attitude of truth and must then renounce the objectivity of the human sciences. [Ricoeur 1991:75]

In place of this sterile dichotomy, Ricoeur proposes a productive role for distanciation and thus for explanatory method within the framework of a hermeneutics of tradition. He does this by going beyond Gadamer’s undifferentiated appeal to “language”—beyond Gadamer’s invocation of the general linguisticity (*Sprachlichkeit*) of human existence—in order to develop a more precise concept of the linguistic text itself as the “paradigm of distanciation in communication,” by virtue of the fact that language in the form of a text is always “communication in and through distance.” Ricoeur points out that understanding in the human sciences occurs paradigmatically through the reading of texts, not through face-to-face dialogue. Furthermore, written texts are public works of discourse in which there is an “intentional exteriorization” or alienation of the author’s spoken meaning, which opens the door to critically distanced, objectifying methods of interpretation.

Here we must be careful to distinguish writing from speaking. Writing is qualitatively different from speaking because it involves several interrelated forms of distanciation. For one thing, the event of saying is distanced from the meaning of what is said. The event is lost but the meaning is fixed by writing. Because of this the meaning of the text is also distanced from the mental intention of the author. In a situation of spoken discourse the speaker’s intention normally coincides with the meaning of what is said, but once a text is detached from its author’s situation, it becomes autonomous with respect to the psychology of the author: “What the text says now matters more than what the author meant to say” (Ricoeur 1991:148).

But Ricoeur goes on to argue that a text’s meaning (“what it says”) consists of both its internal “sense,” which is immanent in the structure of the text, and its external “reference,” in which it projects a possible way of being-in-the-world.12 The inherent distance between sense and reference that discourse-as-text makes possible in turn permits objectifying formalist or structuralist analyses that purposely suspend the text’s reference in order to explicate its internal dynamics (its sense). In so doing, these objectifying explanations, far from being alien to interpretation, actually enable subjective understanding more adequately to comprehend the “world” disclosed by the text (its reference).

Explanation is thus not foreign to interpretation but is in fact necessary for understanding, for “to understand a text is to follow its movement from sense to reference: from what it says, to what it talks about” (Ricoeur 1976:87f.). Moreover, this movement from “explaining the sense” to “understanding the reference” in turn reveals still another kind of productive distanciation, which is especially evident in literary texts, namely, the distance between the lifeworld of the reader and the lifeworld referred to by the text, through which the text may propose to the reader another way of being-in-the-world.

With respect to the objectivity of interpretation, Ricoeur suggests, following E. D. Hirsch (1967), that we view the process of interpretation as a dialectic of “guess” and “validation,” in which subjective understanding contributes an initial guess as to the meaning of the text and various objective explanatory methods seek to validate or correct that guess.13

12 Here Ricoeur builds on the distinction that was made by the philosopher of mathematics Gottlob Frege (1848–1925) between “sense” (*Sinn*) and “reference” (*Bedeutung*) as two different aspects of meaning, a distinction which has subsequently had a great impact in the philosophy of language. As Frege puts it: “The regular connexion between a sign, its sense, and its reference is of such a kind that to the sign there corresponds a definite sense and to that in turn a definite reference, while to a given reference (an object) there does not belong only a single sign” (Frege 1952:57). W. T. Jones (1975:149) gives some simple examples of this distinction: “[the mathematical signs] ‘2’ and ‘4’ have the same reference; that is, these signs are names for the same number [16], but ‘2’ and ‘4’ have different senses, since the thought of multiplying 2 four times (2 × 2 × 2 × 2) is quite different from the thought of multiplying 4 twice (4 × 4). Similarly as regards ‘morning star’ and ‘evening star’: these two expressions have the same reference: the planet Venus. But the sense of the former term is of a star that appears in the morning, and the sense of the latter term is of a star that appears in the evening, and these are quite different thoughts.”

13 Note, however, that Ricoeur’s notion of the semantic autonomy of the text conflicts with the position eloquently espoused by Hirsch in his book *Validity in Interpretation* (1967). Hirsch defends the traditional view that “a text means what its author meant.” He does so by distinguishing the timeless “verbal meaning” of a text from its changing “significance,” acknowledging that the latter may be different for different readers. But Palmer (1969:60–65) observes that Hirsch’s argument for returning to the older “philological” approach to hermeneutics simply adopts, without defending them, various realist philosophical presupposi-
These methods of validation permit the interpreter to argue that a given interpretation is more or less probable because they supply publicly available criteria shared by a given community of interpreters. Hence, if it is true that there is always more than one way of construing a text, it is not true that all interpretations are equal . . . The text is a limited field of possible constructions. The logic of validation allows us to move between the two limits of dogmatism and skepticism. It is always possible to argue for or against an interpretation, to confront interpretations, to arbitrate between them, and to seek for an agreement, even if this agreement remains beyond our reach. [Ricoeur 1991:160].

To be sure, all interpretation is still ultimately based in a historical tradition, as Gadamer has shown. Every initial guess is rooted in the prejudices provided by the tradition to which the interpreter belongs. As Gadamer himself insisted, however, the inescapable functioning of prejudice does not mean that there are no constraints on interpretation—that all interpretations are equally likely.14 But without explanatory validation of the kind that Gadamer himself rejects, it is difficult to see how these constraints might operate. What Ricoeur has done, then, is to give a more defensible articulation of Gadamer’s notion of being “pulled up short by the text.” He has done this by demonstrating the necessary role of distanciation at the very heart of the Sprachlichkeit of human existence, insofar as the mediation of human experience by language is manifested under conditions of historical distance as Schriftlichkeit, mediation by texts.

Ricoeur’s rehabilitation of objectifying explanation marks his emphatic rejection of the Romanticist tendency in hermeneutics that had persisted since Schleiermacher’s day. In Romanticist hermeneutics, the paradigm for interpretation is the intuitive grasp of the mental life of another in a situation of dialogue, in which mutual understanding is relatively unproblematic because it is assisted by many non-linguistic factors pertaining to the dialogical situation. But the productive distanciation characteristic of written texts means that the work decontextualizes itself, from the sociological as well as the psychological point of view, and is able to recontextualize itself differently in the act of reading. It follows that the mediation of the text cannot be treated as an extension of the dialogical situation. For in dialogue, the vis-à-vis of discourse is given in advance by the setting itself; with writing, the original addressee is transcended. The work itself creates an audience, which potentially includes anyone who can read. [Ricoeur 1991:298]

Gadamer failed to stress this point because, despite his criticism of Romanticist hermeneutics, he did not sufficiently free himself from the dialogical model of understanding, and so did not develop the implications of the post-Heideggerian concept of the autonomy of the text.

For Ricoeur, moreover, critical acts of explanatory validation are not limited to structuralist or linguistic methods but may also employ the modes of “suspicion” advocated by the critique of ideology. In his book *Freud and Philosophy*, Ricoeur (1970:28–36) compares the two opposing schools of interpretation in the twentieth century. On one side is the “school of suspicion” headed by the three great “masters of suspicion,” Marx, Nietzsche, and Freud, and perpetuated by neo-Marxist critical theorists such as Habermas, and in a rather different way by French poststructuralist authors such as Michel Foucault and Jacques Lacan. On the other side are practitioners of the “recolletion of meaning,” represented by exponents of the phenomenology of religion such as Mircea Eliade and by biblical scholars such as Rudolf Bultmann. The first group are “demystifiers” who see traditional symbols as emblems of “false consciousness” to be exposed and destroyed. The latter group are “demythologizers” who are animated by “faith” (albeit a rational and postcritical faith) rather than by “doubt,” and who therefore seek to uncover the deep truths about human existence which they believe are revealed by such symbols.

In his essay “Hermeneutics and the Critique of Ideology,” Ricoeur (1991:301) explains how both attitudes are part of a properly hermeneutical approach to interpretation. If distanciation involves suspicion and the critique of one’s most cherished
illusions and modes of false consciousness, then “appropriation”—the recognition of belonging to a tradition and the recollection of its meaning—is its dialectical counterpart. This is so because the recognition of belonging to a tradition itself depends on the potentially alienating encounter between one’s own lifeworld and the possible worlds proposed by texts, including utopian texts (such as The Communist Manifesto) that mediate “the subversive force of the imaginary” by calling into question one’s prior understanding of the world. The resulting distanciation from oneself “is not a fault to be combated but rather the condition of possibility of understanding oneself in front of the text” (ibid.). At the same time, the critique of false consciousness is itself rooted in and recollects a historical tradition of emancipation, which it appropriates in its attack on all of the masks and illusions of power.

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What does this discussion of hermeneutics have to do with the subject of this book? It is intended to define certain key concepts that underlie the argument to follow. It also highlights Ricoeur’s achievement in offering a cogent philosophical articulation of the dialectical relationship between understanding and explanation, belonging and distanciation, recollection and suspicion, that characterizes interpretation in the human sciences, including ancient history and historical archaeology.

The ancient Near Eastern “house of the father,” in particular, is subject to both demystification and de-mythologization. Many historians and archaeologists, both Marxist and functionalist, regard the ancient political symbol of the “house of the father” as simply a mask for the “real” relationships of domination and subordination which are to be uncovered and studied. Others take a more positive view of this symbol, seeing it as not merely an epiphenomenon but an expression of social solidarity and the desire for humane political ties of obligation and responsibility, which we would do well to emulate in the present. On the one hand, the role of critique in a hermeneutics of symbols, as Ricoeur has articulated it, demands acknowledgment of the distortive effects of a symbol like the “house of the father.” On the other hand, there is no standpoint for critique outside of a symbolic tradition, which means that we must recognize that such symbols first integrate and legitimate the social order before they can distort it (see the essays “Science and Ideology” and “Ideology and Utopia” in Ricoeur 1991). Methodologically speaking, this means that we must pay close attention to native linguistic expressions of social relationship and not simply dismiss them as symptoms of false consciousness, for social reality is linguistically constituted and all social action is symbolically mediated, including acts of domination and repression.

In any case, interpretation of the ancient symbol of the “house of the father” must take into account the degree to which it forms part of the Western tradition to which most modern interpreters belong, not just as mediated through Christian and Jewish theology, but also as conveyed in a long tradition of political thought that leads from the classical Greek oikos to contrasting atomistic (“market”) and communitarian (“household”) models of society in modern liberalism and socialism, respectively (see Booth 1993 on the role of the household concept in Western economic and political thought). We should recognize the ways in which we belong to this tradition, as well as the critical distance we must take from it in light of modern egalitarian and libertarian ideals. This issue will be discussed further in my planned second volume on the “Axial Age” of the first millennium B.C., under the heading “The House of the Father as Ideology and Utopia.”

4. The Model of the Text

The preceding review of hermeneutics prepares the ground for a consideration of Ricoeur’s widely cited 1971 essay “The Model of the Text: Meaningful Action Considered as a Text” (reprinted in Ricoeur 1991), in which the paradigm of textual interpretation is applied to the interpretation of social action in general. This transfer from the hermeneutics of written texts to the hermeneutics of historical action is what justifies the universal claim of hermeneutics to be the foundation of the human sciences.

Ricoeur summarizes the paradigm of the text in terms of four interrelated traits: (1) the fixation of the event of discourse as a repeatable meaning;15 (2) the autonomy of the text’s meaning with respect to the intention of its author; (3) the text’s reference to a possible way of being-in-the-world; and (4) the text’s openness to new interpretations, such that its meaning is never fully articulated but is always “in suspense.” Ricoeur then applies these criteria of textuality to the object of study of the social sciences, which Max Weber defined as “meaningfully oriented behavior.”

First, Ricoeur notes that social action forms patterns that must be interpreted in terms of their “inner

15 By “discourse” Ricoeur means linguistic usage versus linguistic system or code; i.e., parole as opposed to langue, in the parlance of the structural linguistics pioneered by Ferdinand de Saussure.
connections”; that is, in terms of typical motivations for action, as opposed to the external “causes” which in positivist social science are held to determine behavior. Such a pattern of motivated action can be expressed as a repeatable meaning by means of what Weber called “ideal types.” A classic example of such a type is the so-called economic man (homo economicus), characterized by rationalistic economizing motivations, who is prevalent in modern capitalist society. With the aid of ideal types, the meaning of an action can be detached from the event of the action, just as a written text can be detached from the original event of discourse.

Second, the autonomy of a text with respect to its author corresponds to the way in which a social action becomes detached from the acting individual and produces unintended consequences. Indeed, an action is “social” to the extent that it causes effects which are not easily traceable to the actor, making the ascription of responsibility and blame a sometimes difficult matter of interpretation.

Third, a text’s reference to a “world” is analogous to a similar sort of disclosure that is accomplished by the patterns of meaningful social action we call “social structures.” Ricoeur points out that, like traditional myths, social structures are also attempts to cope with existential perplexities, human predicaments, and deep-rooted conflicts. In this sense, these structures, too, have a referential dimension. They point toward the aporias of social existence, the same aporias around which mythical thought gravitates. [Ricoeur 1991:166]

Fourth, like a text, human action is an “open work.” No final interpretation of the course of human history is possible, for it constantly “opens up new references and receives fresh relevance from them” (ibid., p. 155).

These basic points of similarity between texts and action are not accidental but stem from the fact that human social action is always symbolically mediated and thus is experienced in and through language. The linguisticality of human existence, in general, and the textuality of human existence, in particular, justify the application of the paradigm of textual interpretation to all of the human sciences. But this also means that the role of explanatory validation is the same in history as it is in textual interpretation. In Ricoeur’s hermeneutics, unlike Dilthey’s or Gadamer’s, objectifying explanation is not foreign to historical interpretation.

Moreover, historical explanation can take different forms, modeled variously after the modes of explanation employed in linguistics and in the natural sciences. On the one hand, there are synchronic structuralist or formalist analyses of the internal correlations among units of social action (described in terms of Weberian ideal types) that have been abstracted from the course of events, just as synchronic systems consisting of binary oppositions of phonemes and morphemes are explicated in structural linguistics. Such correlations have been detected in structuralist studies of both texts and social behavior (e.g., kinship and marriage systems). For example, Ricoeur cites Claude Lévi-Strauss’s landmark analysis of the Oedipus myth, in which the internal dynamics of the text (the “sense,” in Ricoeur’s terms) are explained in terms of a synchronic system of binary oppositions between plot elements (“mythemes”) that have been abstracted from the narrative (see “The Structural Study of Myth” in Lévi-Strauss 1963). Lévi-Strauss pioneered the application of this “semiotic” approach, derived from Saussurean linguistics, to semiological systems in social, economic, and ideological realms.

On the other hand, relationships between patterns of action may be explained not in correlational but in genetic terms using functionalist “causal laws,” according to a model of explanation borrowed from the natural sciences that has been employed in positivist social science since the mid-nineteenth century. A classic defense of this approach was offered by the philosopher Carl Hempel in his influential article “The Function of General Laws in History” (reprinted in Gardiner 1959). Hempel argued that explanations of historical events do not differ in principle from explanations of physical events, for in both cases a conjunction is proposed between a set of initial conditions (e.g., preceding events or circumstances) and a general law or regularity that is observed to exist between certain sets of initial and final conditions. The main difference, according to Hempel, is that in practice it is difficult to formulate precise historical laws that reliably predict future events. Nonetheless, for him the goal of scientific history is to refine historical concepts and methods to the point where such laws may be formulated.

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16 On Weber’s concept of “ideal types,” see Ringer 1997: 110–21. This concept was developed further by Alfred Schutz (1967) in terms of the ideal types employed not just by social scientists but by social actors themselves in everyday contexts. Schutz combined Weber’s action theory with Husserl’s phenomenological approach in order to explore the way in which social actors learn and revise interconnected typifications of the phenomena experienced in their own lifeworlds. Such typifications constitute the “stock of knowledge” of social actors, enabling them to engage in subjectively (and intersubjectively) meaningful social action. On the phenomenological tradition in sociology, see Luckmann 1978.
In both types of historical explanation, however, whether correlational or genetic, Ricoeur insists that objectifying explanation does not operate independently but is at the service of hermeneutical understanding. This is analogous to his claim that in textual interpretation, explanation and the modes of distanciation it requires are only an intermediate stage between the initial subjective understanding or “guess” as to the meaning of the text (which is rooted in the interpreter’s belonging to a tradition) and the full critical comprehension of this meaning in terms of the text’s reference to a human lifeworld. Thus structuralist analyses, for their part, suspend the existential reference in order to explicate the internal sense, but in both textual and sociohistorical studies it is impossible to leave it at that. Ricoeur notes that:

As a matter of fact, nobody stops with a conception of myths and of narratives as formal as this algebra of constitutive units. . . . First, even in the most formalized presentation of myths by Lévi-Strauss, the units that he calls “mythemes” are still expressed as sentences that bear meaning and reference. Can anyone say that their meaning as such is neutralized when they enter into the “bundle of relations” that alone is taken into account by the “logic” of the myth? . . . Finally, the kind of language game that the whole system of oppositions and combinations embodies would lack any kind of significance if the oppositions themselves, which, according to Lévi-Strauss, the myth tends to mediate, were not meaningful oppositions concerning birth and death, blindness and lucidity, sexuality and truth. Besides these existential conflicts there would be no contradictions to overcome, no logical function of the myth as an attempt to solve these contradictions. [Ricoeur 1991:164]

As valuable as the semiological model of explanation may be, its true role in interpretation is to assist understanding; that is, to permit the interpreter to move from an initial insight to a fuller appreciation of the lifeworld disclosed by the structures he detects. If this were not its function, such correlational analysis would be, as Ricoeur rather scathingly puts it, “a sterile game, a divisive algebra,” which reduces the theory of myth to “the necrology of the meaningless discourses of mankind” (ibid.), and reduces history to “an absurd play of errant signifiers.” Similarly, the causal laws that describe regularities in social behavior in terms of a genetic model of explanation cannot stand on their own in historical interpretation. This is because all historical accounts, correlational or genetic, ultimately have a narrative structure that presumes the ability to follow a story.

Ricoeur describes this ability as follows (drawing upon work in the theory of history by English-language philosophers in the post-Wittgensteinian “ordinary language” tradition, especially W. B. Gallie and Louis O. Mink):

Between recounting and following a story there is a reciprocal relation that defines an entirely primitive language game. . . . Following a story is indeed understanding a series of actions, thoughts, and feelings presenting at once a certain orientation and offering surprises (coincidences, recognitions, revelations, and so on). Given this, the conclusion of the story can never be deduced or predicted. This is why we have to follow its development. But neither should the story be disconnected: although not deducible, its outcome still has to be acceptable. In every story told, there is thus a tie of logical continuity that is wholly specific, since the outcome must be at the same time contingent and acceptable. Such is the basic comprehension without which there would be neither stories nor histories. The reader’s interest is addressed, not to so-called underlying laws, but to the turn taken by this singular story. [Ricoeur 1991:141]

Even in modes of history-writing that promote the “eclipse of narrative” there is an implicit appeal to narrative understanding.17 This is true both of the Anglo-American positivist historiography (and archaeology) influenced by Carl Hempel’s “covering law model,” and of the widely imitated statistical approach to social history championed by Fernand Braudel and the French Annales school. Braudel and his followers denigrate the “history of events,” proposing that “the object of history is not the individual but the ‘total social fact’ . . . in every one of its human dimensions—economic, social, political, cultural, religious, etc.” (Ricoeur 1984:102; cf. the essays on method in Braudel 1980). But Braudel’s focus on slowly changing structures of the long time-span, la longue durée, rather than l’histoire événe-mentielle, should not obscure the fact that his own historical interpretations are framed in terms of an implicit narrative plot. Ricoeur points out that Braudel’s famous book The Mediterranean and the Mediterranean World in the Age of Philip II (Braudel 1972) is organized around the plot of “the decline of the Mediterranean as a collective hero on the stage of world history” (Ricoeur 1984:215). It is a drama of the clash of civilizations, for “just as Homer picked from the stories of the Trojan War the set he chose to tell in the Iliad, Braudel picks from the great conflict between civilizations in which the Occident and the Orient alternate the conflict whose protagonists are

17 Ricoeur explores the relationship between historiography and narrative in considerable detail in Time and Narrative, vol. 1 (Ricoeur 1984; see also the essay “The Narrative Function” in Ricoeur 1981). Here he draws upon the analysis of the tropes of historical writing in Hayden White’s Metahistory (1973) and other works.
Spain and Turkey at the time of Philip II and whose framework is the decline of the Mediterranean as a historical zone” (ibid., p. 216). The geography, demography, and economic patterns of the Mediterranean coastlands set the stage for this drama, and Braudel’s great achievement is to relate this setting to durable civilizational structures. Nevertheless, far from rejecting events as the object of history, the historian of the long time-span invariably focuses on the “event” of the change or disappearance of these structures, however long or short this may be in terms of absolute duration. Ricoeur says:

in contrast to the sociologist, the historian in dealing with structures is attentive to their breaking points, their sudden or slow deterioration, in short, to the consideration that they die out. In this respect, Braudel is no less preoccupied with the decay of empires than the traditional historian. . . . Once again, the fragility of human works comes to the foreground and with it the dramatic dimension, from which the long time-span [la longue durée] was supposed to be free history. [Ricoeur 1984:217]

Furthermore, as Jack Hexter (1979) points out, conceptual difficulties have arisen as Braudel pursues his stated intention of writing “total history,” drawing on all the social sciences. Braudel’s rather arbitrary tripartite division of scales of historical change into short-term political events, medium-term economic and demographic changes, and long-term climatic and geographic effects (la longue durée), and his bias toward the latter two, have led to an unnecessary compartmentalization of ideal and material factors, which are assumed to operate on different time scales and so are inadequately related to one another in his attempts to solve particular historical problems. As Braudel’s critics have noted, ideas and social institutions (such as “monarchy”) can be of very long duration, while economic and environmental factors may affect human society in the form of brief cataclysmic events. Moreover, short-term events involving individual decisions and actions cannot easily be explained in terms of longer-term material conditions alone, as Braudel appears to suggest when he speaks of the “history of events” (and the beliefs and feelings that produce it) as “surface disturbances, crests of foam that the tides of history carry on their strong backs” (Braudel 1972:21). In other words, Braudel’s approach has a tendency to reductionism because—despite the early Annales’ emphasis on mentalité—he fails to include ideas and the institutions that reflect them among the long-term factors accounting for social behavior and historical change.

On this score, Guenther Roth (1979) directly compares Braudel to Weber, pointing to the anticipation of Annales views in the German socioeconomic history of Weber’s day, and noting the concern that Weber shares with Braudel for the analysis of long-term structures. For Weber, however, unlike Braudel, ideal factors are as important as material factors even in la longue durée, so that the problem is not to explain one kind of causal factor in terms of the other but to analyze their interaction in specific historical cases.

What, then, is the place of general laws in histories that remain ineluctably narratival at heart? Even the most ardent positivists have recognized that the historian qua historian does not formulate causal laws. But historians may well use laws borrowed from demography, economics, ecology, and other synchronic disciplines in order to advance understanding of their narrative “plots.” Hempel was perfectly correct in describing the structure of historical explanation in terms of the application of general laws, but he failed to consider the purpose of such explanation, “namely, that explanation is what allows us to continue to follow a story when spontaneous understanding is impeded. . . . [The historian] does not aim to place a case under a law but to interpose a law in a narrative, in an effort to set understanding in motion again” (Ricoeur 1991:142). Indeed, it is the fundamentally narratival character of historical compositions, which therefore call for the same faculty of understanding as other kinds of narrative texts, that distinguishes history from the ahistorical social sciences.

Ricoeur (1984:91) insists that his defense of “the ultimately narrative character of history in no way is to be confused with a defense of narrative history.” There is no question of the fruitfulness of quantitative methods and formal causal models in historical research, and there is no special status granted to traditional political history. Furthermore, historical narratives are not fiction; they aim to be “true” narratives that are constrained by objective evidence and objective explanatory methods, as defined by a given community of interpreters. According to Ricoeur, however, there is a deep similarity between fictional and historical narratives, for both engage in an “emplotment” of human action so as to redescribe reality (what Aristotle called mimésis), thus disclosing ineradicably temporal ways of being-in-the-world.

In terms of practical methodology, Ricoeur sees a finely grained interpenetration of causal explanation and narrative understanding in historical interpretation. He therefore resists the simple dichotomy between “cause” and “motive” that some Wittgensteinian action theorists have used to distinguish what they see as the two incommensurable “language games” in which one speaks of events in nature as opposed to actions performed by people. Ricoeur
The history of the ancient Near Eastern “house of the father” as both fact and symbol reflects this dual allegiance to “force” and “sense” in human experience. In my own narration of its vicissitudes as a civilizational structure of *la longue durée*, I will freely incorporate demographic, ecological, and economic explanations that involve the application of general laws. But this use of objectifying explanation is not a concession to environmental or technological determinism. Material circumstances do of course constrain individual choices, but rarely do they limit social actors to only one course of action; hence there is considerable scope for symbolically mediated and thus historically contingent patterns of action. Furthermore, the patterns of meaningful social action whose effects are studied by formally rigorous demographic and economic methods (e.g., marriage and household formation systems and systems of material exchange) should not themselves be reified as quasi-physical impersonal forces that determine human behavior, for they can always be related to the linguistically expressed symbols that mediate even the most habitual social action.

My use of causal explanation therefore does not constitute a rejection of methodological individualism and intersubjectivity in order to hypostatize patterns of action as collective entities. Such explanation is made possible because individual motivations for social action can be *experienced* as external forces, not because such individual experiences can be reified for analytical purposes as collective external facts; for these individual unconscious motivations—these compelling desires—are always expressed in terms of meaningfully interrelated symbols such as the “house of the father.” Indeed, symbolically structured patterns of action are what constitute the patriarchal household as a demographic, ecological, and economic fact in the first place. We must not lapse into objectivism by separating social behavior into a realm of physical causation, on the one hand, and a realm of conscious motivation, on the other. We must rather preserve “the minimal criteria of human action, namely, being able to identify this action through the projects, intentions, and motives [conscious or unconscious] of agents capable of imputing their action to themselves” (Ricoeur 1991:244).

Objectifying causal explanation notwithstanding, my history of the “house of the father” thus remains a singular narrative rather than a causal model of a dechronologized system. It certainly aims to be a “true” narrative, not fiction, but it has a “plot” which is characterized by a “change of fortune”: it is a drama whose protagonist experiences birth, growth, and death—and perhaps even resurrection in a new

(1991:132–38) observes that this dichotomy is another version of Dilthey’s overly rigid dichotomy between “explanation” and “understanding,” for it implies that causation has only to be explained, whereas motivation need only be understood. But it is David Hume’s very restrictive view of causality that is assumed here, in which a cause is regarded as logically independent of its effect, whereas a motive is not a cause because it cannot be stated without describing the action of which it is the motive. The dichotomy of causation and motivation rests on the notion that a human motive or intention logically implies the action it produces, unlike a physical cause, which is statistically correlated with the occurrence of its effect but does not logically imply it, and thus can be discussed quite separately from it.

In rejecting this simple dichotomy, Ricoeur (1991:134) suggests that “we are dealing instead with a scale that would have as one of its end points causation without motivation and, on the opposite end, motivation without causation.” Most human action falls along the middle of this scale, calling for a mixture of causal explanation and motivational understanding. Thus causal explanations in terms of general laws are needed not only to account for purely external physical causes that are logically distinct from their effects, but also to explicate the consequences of certain inner motives which are felt as external constraints that “cause” the actions they imply.

Indeed, the concept of social “structure” assumes that certain motivated patterns of action are experienced as externally constrained or as the product of unconscious “forces,” having become deeply engrained through a process of socialization. Consequently, in place of the simple opposition between cause and motive, which does not faithfully depict the practical reality of human experience, we need a more complex concept—the notion of deeply internalized and symbolically expressed “desires” as both “a force that compels and a reason for acting.” And this leads us back from methodology to ontology, to the question:

What is the being that makes possible this double allegiance of motive to force and to sense, to nature and to culture, to *bios* and to *logos*? One would have to reflect here upon the very position of the human body in nature: it is at once one body among others (a thing among things) and a manner of existing of a being capable of reflecting, of changing its mind, and of justifying its conduct. . . . Human being is as it is precisely because it belongs both to the domain of causation and to that of motivation, hence to explanation and to understanding. [Ricoeur 1991:135]
form. Following Ricoeur, I see this narrative element as the inescapable horizon of all historical interpretation, reflecting the narrativity of human self-understanding, which always starts from a relationship of belonging to a narrated tradition.

Yet one can comprehend one’s own tradition only in the encounter with objects of historical interpretation such as the ancient Near Eastern “house of the father.” Such objects must indeed be explained as well as understood, but narrative understanding—the ability to follow a story—“precedes, accompanies, concludes, and thus envelops explanation,” even as “explanation, in turn, develops understanding analytically” (Ricoeur 1991:142).
Chapter 2. The Sense and Reference of Human Works

In this book I invoke both archaeological and textual evidence in narrating the history of the ancient Near Eastern “house of the father” as fact and symbol. This raises the complex issue of the relationship between archaeological and historical modes of explanation, which has been much discussed over the past few decades. My own approach to this problem, although explicitly hermeneutical, is quite different from that of many self-confessed “interpretive” archaeologists—not to mention the large number of positivist archaeologists who reject the hermeneutical paradigm altogether.

In particular, I emphasize the methodological necessity of textual interpretation in reconstructing ancient social structures, understood as patterns of meaningful social action. To be sure, architectural remains and other artifacts must be studied using the full array of modern archaeological techniques, not just for descriptive purposes, but with a view to uncovering systematic spatial and temporal correlations, as far as possible by formal statistical means. But the connection between such artifacts and past social structure (as opposed to externally observable behavior), will be discerned largely through contemporary documents, augmented by less proximate written texts from which plausible ethnohistorical analogies can be drawn. This old-fashioned view of the limitations of purely archaeological evidence is demanded, I believe, by the principle of the linguisticity of human existence that underlies philosophical hermeneutics and the related tradition of interpretive social theory.

1. Material Culture as Text?

The British archaeologist Ian Hodder cites Ricoeur’s “model of the text” in defense of his own “interpretive” approach to archaeology (Hodder 1991:153; 1992:171; 1993; Hodder et al. 1995:168). Since the early 1980s, Hodder has been one of the chief instigators of a sometimes fierce dispute between the proponents of positivist and antipositivist approaches to archaeological interpretation.1 An explicitly positivist theory of archaeology first emerged in American circles in the early 1960s, as might be expected given the intellectual climate of the day (which is not to say that earlier archaeologists were free of positivist assumptions). This theory was articulated by Lewis Binford and his followers under the influence of Carl Hempel’s neopositivist theory of history and contemporary antihistoricist or “neoevolutionist” trends in cultural anthropology.2 The positivist approach to archaeology became known as “processualism” because it stresses the need to formulate and test general laws of “cultural process” in order to explain the dynamics of “cultural systems,” as opposed to the traditional archaeological concern with a more particularistic “culture history” (see Flannery 1967). Despite occasional reservations,3 processualism became the dominant archaeological paradigm, especially in North America.

The ongoing theoretical debate in archaeology can be viewed as a parochial version of similar debates that occurred somewhat earlier in most other social sciences. The closest analogue may perhaps be found in human geography, which experienced its own “quantitative revolution” in the 1960s and a vigorous antipositivist reaction in the 1970s (Johnston 1986; 1991). Unfortunately, the positivist-antipositivist debate in archaeology has not been marked by notable philosophical sophistication on either side. Most positivist archaeologists who have attacked hermeneutical approaches in archaeology seem to be unaware of the basic assumptions of philosophical hermeneutics. The debate is thus sidetracked into epistemology and the problem of “objectivity” before the processualists have dealt with the radical challenge that Heideggerian ontology presents to the Cartesian assumptions.

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1 A representative sample of positivist (“processual”) and antipositivist (“postprocessual”) interpretive approaches in archaeology may be found in the essays collected in Preucel 1991 and in Preucel and Hodder 1996.

2 Binford’s landmark article is entitled “Archaeology as Anthropology” (Binford 1962). In it he commends Julian Steward’s (1955) “ecological” approach to human culture and Leslie White’s (1959) technologically deterministic model of “cultural evolution.” A less biologistic but equally positivist view of archaeology was championed in Britain by David Clarke (1968). For an overview of these developments, see Trigger 1989:289–328.

3 For example, Kent Flannery (1973) derided the rather trivial “Mickey Mouse laws” of cultural process which positivist archaeologists tended to produce; e.g., “as the population of a site increases, the number of storage pits will go up.” At about the same time, Robert McC. Adams (1974) questioned the processualist claim that ecological-systemic processes are universal in scope and can account for all social change, arguing instead that the sudden and unpredictable effects of human “consciousness” via goal-motivated social action must also be taken into account.
that underlie positivist social science. As a result, processualist criticisms of the hermeneutic view of the relationship between the human and natural sciences which might be valid if directed against Dilthey or Collingwood are far less relevant to the hermeneutics of Gadamer or Ricoeur.

For example, in Binford’s (1989:27–40; 55–68) scathing denunciation of the movement from processual to “postprocessual” archaeology (from “science to séance,” as he calls it), he assumes as fundamental the ontological dichotomy between subject and object that post-Heideggerian hermeneutics calls into question. Thus he cannot regard hermeneutical approaches in archaeology as anything other than hopelessly subjectivist—the hermeneutical circle can only appear to be a vicious circle. Yet if one’s philosophical starting point is not the reified abstraction of the subject-object dichotomy that has been characteristic of modernist thought since the Enlightenment, but rather human lived experience, which empirical phenomenological analysis reveals to be ineradicably temporal and linguistic, then the positivist view of archaeology as a neutral science is difficult to sustain.

From the perspective of hermeneutics, language and interpretation are not merely tools of thought which an archaeologist may use or not as she likes in reconstructing some deeper social reality. On the contrary, Heidegger and Gadamer have questioned the notion that there is such a social reality before and behind language to which we have access other than through the medium of language itself. In Ricoeur’s hermeneutics, this ontology is developed into the concept of the basic textuality and narrativity of human existence, with various epistemological implications for the practice of the human sciences which I have outlined above.4

It did not help matters that in the early years of this debate, Hodder and his associates adopted a radically relativist poststructuralist position which gave little attention to the methods of validation needed to adjudicate conflicts of interpretation. More recently, however, Hodder (1992:155–200) has taken note of the differences between hermeneutics and poststructuralism, and he claims to have abandoned the latter in favor of the former (for avowedly poststructuralist approaches to archaeology, see Shanks and Tilley 1987; 1992; Bapty and Yates 1990; Tilley 1993; 1999). This permits him to argue for a “guarded objectivity of the past” (Hodder 1992:187ff.; 1999:159ff.), as opposed to the free proliferation of many equally valid “readings” of material culture, including readings which are racist, sexist, imperialist, or simply bizarre. Here, as he acknowledges, he is following Ricoeur rather than Derrida and Foucault.

But Hodder’s basic notion of “reading” material culture remains very doubtful, in my view, because it relies on structuralist and poststructuralist assumptions which are rejected in Ricoeur’s hermeneutical philosophy. One cannot simply add a dose of Ricoeur’s “guarded objectivity” to an essentially poststructuralist position, and Hodder’s attempt to do so betrays a lack of appreciation of the fundamental differences between hermeneutics and poststructuralist complete explanation of human behavior must take account of what human beings believe about their social and natural environment” (p. 10), and most would even agree that “culture has transformed human behavior to the point that the symbolic representations that human societies create mediate all forms of human behavior” (p. 11). But the real point of contention has to do with Trigger’s classically positivist and evolutionist assumption that “the primary purpose of symbolic representation is adaptive” (ibid.). This claim is meaningful only within an objectivist philosophical framework which simply assumes what it should prove, for the cultural-evolutionist paradigm falls apart when one begins to ask what meaning the biological evolutionary concepts of “selection,” “adaptation,” or “fitness” might have with respect to human society or culture (this is discussed further below in chapter 2.2). What is troubling is that in Trigger’s lengthy 34-page article there is not a single mention of Husserl and Heidegger—who are generally regarded as having some relevance for the philosophy of social science—not even of Wittgenstein, let alone the large body of secondary social theory derived from these philosophers’ insights (e.g., Giddens 1984). Instead, Trigger focuses on the outmoded debate between Collingwood’s idealism and Hempel’s positivism, viewing the currently influential phenomenological and hermeneutical critiques of both idealism and positivism merely as “various forms of idealist epistemology” (p. 7). Only by neglecting these important critiques could Trigger continue to applaud the evolutionism of the archaeologist V. Gordon Childe, which is rooted in what for many is a rather crude version of Marxist materialism.

4 Unfortunately, there has been little improvement to date in the philosophical breadth and sophistication displayed by leading practitioners of archaeological theory. For example, Bruce Trigger’s (1998) recent essay on “Archaeology and Epistemology” is characterized by a striking neglect of the twentieth-century philosophical discussion of ontology. Hence, in his view, the only epistemological alternatives are “idealism,” “positivism,” or “realism,” which he presents in pre-Heideggerian, Cartesian terms, and which he attempts to reconcile with one another through an appeal to a staunchly objectivist cultural-evolutionist perspective. But Trigger’s own brand of evolutionism hardly constitutes a new or convincing epistemological alternative. It does not succeed in taking into account the widespread criticisms of positivism, which have penetrated the consciousness of even the most processual archaeologists in “today’s postmodern intellectual environment” (p. 1). Many unreconstructed positivists (including the subspecies Trigger calls “realists,” with whom he identifies himself) would agree with him, and with those he calls “idealists,” that “a com-
ism. Indeed, Hodder continues to lump Ricoeur together with Foucault and Derrida as three proponents of a linguistic paradigm for the human sciences who adhere in one way or another to the principle of the autonomy of the “text,” be it linguistic or cultural. Yet the surface similarities between Ricoeur and these other French theorists are highly misleading, for Ricoeur’s view of the semantic autonomy of texts is very different from that of structuralism and post-structuralism, both of which downplay individual agency and conscious motivation in the name of collective unconscious structures of meaning, be they Lévi-Strauss’s semiotic codes, Derrida’s “absolute text,” or Foucault’s “discursive formations.”

As Hodder (1991:50) himself puts it: “In structuralism signifiers have meaning through their difference from other signifiers. But these other signifiers themselves only have meaning by being opposed to yet other signifiers in an endless chain of signification.” Poststructuralism thus develops the implications of structuralist logic and ends up by “deconstructing” structuralism itself on the basis of structuralist principles, challenging any claim to construct “a neat closed system of oppositions,” or indeed any “totality, a whole or an original meaning, a truth, because these ‘origins’ of meaning must always depend on other signifiers” (ibid.). The result is the postmodernist view of a chaotic and fragmented human world, in which there is no underlying reality but only the multivocal “texts” constituted by human actions and human theories. Indeed, the self-identity of the human subject as either actor (“author”) or interpreter (“reader”) is not fixed or determinate but is itself constituted by the interaction of signifiers. Systems of signification (“meanings”) are not linked to any external reality; hence, “all signs and symbols are inescapably indeterminate, embodying multiple and contradictory meanings and undergoing continual change” (Mingers 1995:214).

From Ricoeur’s perspective, however, both structuralism and poststructuralism err in suppressing the external “reference” of texts so as to focus solely on their immanent “sense” or internal correlations. It is important to remember that in structural linguistics, and in the semiotic paradigm in general, a “signifier” does not refer to a thing in the external world but to a “signified” lexical concept within a given semiotic system (Saussure 1959:65ff.). The link between the signer and signified is arbitrary, in that the signer has no natural connection with the signified. Together, the signer and signified constitute an arbitrary “sign.” Thus Ricoeur’s distinction between internal sense and external reference at the level of the sentence or text is not the same as the distinction between signer and signified at the level of the sign, which remains entirely within the semiotic system. By definition, as Ricoeur says, semiotic systems are “closed,” i.e., without relations to external, non-semiotic reality. The definition of the “sign” given by Saussure already implied this postulate: instead of being defined by the external relation between a sign and a thing, a relation that would make linguistics dependent upon a theory of extralinguistic entities, the sign is defined by an opposition between two aspects . . . . These two aspects are the “signifier”—for example, a sound, a written pattern, a gesture, or any physical medium—and the “signified”—the differential value in the lexical system. The fact that the signifier and the signified allow for two different kinds of analysis—phonological in the first case, semantic in the second—but only together constitute the sign, not only provides the criterion for linguistic signs, but also, by extension, that of the entities of every semiotic system. . . . Language no longer appears as a mediation between signs and things. It constitutes a world of its own, within which each item only refers to other items of the same system, thanks to the interplay of oppositions and differences constitutive of the system. . . . At this extreme point, language as discourse [i.e., as a temporal event] has disappeared. [Ricoeur 1976:6]

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5 Ricoeur is listed with Foucault, Barthes, and Derrida as a representative of French poststructuralism in an essay coauthored by Hodder and Robert Preucel (1996:299), and in Hodder’s (1999:182) reference to the “death of the subject” in poststructuralist writing. Although Robert Preucel’s (1995:153f.) more detailed typology of philosophers elsewhere distinguishes the “hermeneutic poststructuralism” of Gadamer and Ricoeur (who, it should be noted, were never structuralists in the first place) from the “critical poststructuralism” of Derrida and Foucault, the differences between hermeneutics and poststructuralism are so great that it is patently erroneous to link them as closely as Hodder and Preucel do. As witness to the distinction that is usually drawn between hermeneutics and poststructuralism in current philosophical discussion, see the essays “Hermeneutics: Gadamer and Ricoeur” and “Poststructuralism: Derrida and Foucault” in The Columbia History of Western Philosophy (Popkin 1999).

6 Hodder’s rather incoherent mixture of these approaches is reflected in his recent book on The Archaeological Process (1999), which is devoted to the methodological implications of his theoretical approach in the context of archaeological fieldwork. On the one hand, Hodder defends a run-of-the-mill hermeneutical position that emphasizes the dialectical (“recursive” and “reflexive”) nature of interpretation at all stages from initial description to final publication, a position with which few today would argue. On the other hand, his poststructuralist emphasis on “multivocality” and his rejection of linear “grand narratives,” eschewing the relative valuation of conflicting interpretations according to specified criteria, leans in the direction of the kind of radically relativist hermeneutics he had previously disowned.
In contrast to this “unidimensional approach to language,” Ricoeur follows the linguist Émile Benveniste (1971) in defending a “two-dimensional approach” in which language is seen as consisting of “two irreducible entities, signs and sentences” (Ricoeur 1976:6). The “sentence” (and, by extension, the configuration of sentences we call a text) is not simply a quantitative extension of the sign; instead, sign and sentence are qualitatively different phenomena. The whole cannot be reduced to the sum of its parts: “A sentence is made up of signs, but is not itself a sign.” This is because a sentence, unlike a sign, constitutes a predication—it says something about the world. “With the sentence . . . language is directed beyond itself” (ibid., p. 20).

By highlighting the referential function of language, which both structuralism and poststructuralism neglect, Ricoeur qualifies the concept of the semantic autonomy of the text, arguing that this autonomy “does not imply that the notion of authorial meaning has lost all significance. . . . If the ‘intentional fallacy’ [that the author’s intention is the criterion for any valid interpretation] overlooks the semantic autonomy of the text, the opposite fallacy [the ‘fallacy of the absolute text’ which hypostatizes the text as an authorless entity] forgets that a text remains a discourse told by somebody, said by someone to someone else about something” (ibid., p. 30).

Moreover, as was noted above, the referential dimension of the text ensures that it is not open to any number of equally valid readings, but is always “a limited field of possible constructions.” Interpretation is constrained by a probabilistic “logic of validation” that makes use of objective explanatory methods in order to follow a text from its sense to its reference, from what it says (e.g., by means of structural oppositions within a virtual semiotic system), to what it says it about.

Hodder (1992:190f.) accordingly appeals to Ricoeur in support of his notion of a “guarded objectivity” in interpreting “material culture as text.” It is not clear, however, that Hodder is justified in citing Ricoeur’s “model of the text” as the basis of his own approach. Key assumptions of Ricoeur’s model are ignored in Hodder’s application of it. It can be argued that Hodder continues to rely on an essentially structuralist model of the text which is antithetical to that of Ricoeur. To put it another way, Hodder does not actually read “material culture as text,” in Ricoeur’s sense of the word; rather, he decodes “material culture as sign.” The confusion arises because both Hodder and his more explicitly structuralist colleagues view linguistic texts as atemporal collections of signs rather than as qualitatively different phenomena which, by means of unique configurations of signs in the form of sentences (i.e., signs configured in temporal sequences that are written and read in a particular order), constitute determinate, temporally situated predications concerning a human world. Their kind of semiotic archaeology therefore slips into the fallacy of regarding material artifacts themselves as “symbols” or “metaphors,” neglecting the fact that a metaphor is always an event of discourse—a “semantic twist” or “absurd predication”—at the level of the sentence, as Ricoeur insists, not at the level of the sign (Ricoeur’s concepts of “metaphor” and “symbol” are described below in chapter 2.3; see Ricoeur 1976; 1977).

To be sure, material objects may be symbolic or metaphorical in the sense that they give rise to linguistic metaphors, but the metaphor itself inheres in linguistic expressions (i.e., sentences) uttered and understood by particular human beings at particular times. The metaphor does not inhere in the material object itself as an isolated signifier that points to a signified concept entirely within a semiotic system.

“Solid” Metaphors

The archaeological misuse of the linguistic and literary concept of metaphor is strikingly evident in Christopher Tilley’s recent book *Metaphor and Material Culture* (1999). One can agree with Tilley that metaphor is fundamental to and grounded in embodied human existence, so that it underlies all human thought and speech, without accepting his paradoxical notion of a nonlinguistic “solid” metaphor. Indeed, despite Tilley’s criticism of Ricoeur on this score, it is clear that Ricoeur gives metaphor an equally basic status in human lived experience by emphasizing the role of metaphor in the creation of meaning, as opposed to seeing it as merely a literary ornament.

What Tilley is trying to avoid here, however, is Ricoeur’s concept of metaphor as a temporal event of discourse at the level of intended meaning. Tilley therefore stresses the unreflective or unconscious use of metaphors, in order to say that “metaphor is more than a mere matter of language use. . . . Metaphor is not so much a matter of language in general, and literary use of language in particular, but a matter of thought. We do not just employ and construct metaphors but live through them” (p. 16).

But however habitual and unselfconscious our use of metaphor may be, can we go behind temporal events of linguistic usage to some underlying realm of meaningful “thought”? Tilley seems to suggest that human thought is fundamentally nonlinguistic; in other words, he views language as simply a secon-
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7 Palmer (1969:203) notes that, for Gadamer: “The nature of experience is not a non-linguistic datum for which one subsequently, through a reflective act, finds words; experience, thinking, and understanding are linguistic through and through . . .” In post-Heideggerian hermeneutics, language is central because human existence is fundamentally a matter of understanding the meaning of one’s situation, and a meaning is always “heard” before it is “seen” in symbolic images. Tilley (1999:267), however, contends that the “metaphorical significance” of a material object “is a complex unity of referential symbolic elements in itself working beyond and outside language. . . . Such images need to be witnessed and experienced to have their effect on the mind. They cannot be represented as they are in words.” While it is true that much that we experience is incommunicable via language, we should not regard this as a special weakness of language and expect it to be remedied by some mysterious mode of nonlinguistic mental representation. Here Tilley wrongly conflates direct perception or sensation with semantic meaning by speaking of the complex of “referential symbolic elements” that constitute an object’s “metaphorical significance” as an “image” that is directly witnessed and experienced. The ambiguity of the term “image” permits him to slide from the notion of an unanalyzed perceptual image to the notion of a metaphorical image or symbol as a mental representation and vehicle of thought. But the transformation of sensation into symbol is an operation of language; it does not happen outside of language, as Tilley claims. Perceptual sensations become meaningful—they become metaphorical “images”—only through integration into a linguistically mediated semantic network (see the essay “Imagination in Discourse and in Action” in Ricoeur 1991).

formative contradiction that arises when he can discuss supposedly nonlinguistic metaphors only through the medium of language itself, and only with reference to particular uses of language in temporal events of discourse.

This is not to deny that there is a nonlinguistic basis for metaphor in the prereflective embodied experiences of individual persons. We can agree that “all human beings think metaphorically with an embodied mind” (Tilley 1999:261). From a hermeneutical perspective, however, we must acknowledge that “to think is to think metaphorically” only because human thought itself is always a temporarily situated event in which lived experience is brought to reflective consciousness in language, which thus creates and recreates a repeatable, shareable meaning. Individual agency remains fundamental. Indeed, it is precisely because human being-in-the-world is embodied that it is characterized by a temporal, “engaged” agency (Taylor 1993), with all that this implies in terms of a methodological individualism that refuses to reify conceptual abstractions (e.g., signs and texts) as collective unconscious entities.

A “Universal Grammar” of Material Meanings

Hodder himself acknowledges the need to move from abstract and unconscious semiotic codes to actual discourse, from the oppositional systems of purely arbitrary signs that he and others have detected in various architectural and ceramic styles, to what Ricoeur calls the text’s reference (see Hodder 1991: 48f., 126–28; 1992:201–5; Preucel and Hodder 1996: 299ff.). But to deal with this problem, he invokes the pragmatic functions of material artifacts and the associated “universal grammar” by which he thinks such functions are related to meaning—what he calls the “very simple rules . . . underlying the ways in which homo sapiens sapiens at all times and in all places gives meaning to things” (Hodder 1991:126). Indeed, because “material culture language” is functionally constrained and thus is much simpler than written language, Hodder believes that the task of deciphering material culture “texts” is actually much easier than deciphering written documents whose grammar and lexicon we do not know in advance. Unlike linguistic signs, which are arbitrary and complex, “material culture meanings are largely influenced by technological, physical and functional considerations. The concrete and partly noncultural nature of such factors enables the ‘text’ of material culture to be read more easily than the arbitrary signs of language” (ibid., p. 128).


dary tool of thought, in opposition to Gadamer’s idea of language as the very medium of thought because of the basic linguisticity of human being-in-the-world. At the very least, Tilley hypostatizes meaning (and thus metaphor) as a collective unconscious entity that is ontologically prior to individual embodied uses of language.

Here the hidden objectivism and methodological holism of Tilley and other (post)structuralist archaeologists is revealed, despite their claim to be practicing an antiobjectivist phenomenology. Tilley’s notion of “solid” or “material” metaphor, as opposed to linguistic metaphor, can only be sustained if the metaphorical creation of meaning operates unconsciously at the level of the semiotic sign, consisting of a material signifier and what it signifies within a virtual semiotic system, rather than at the level of the sentence, in which someone intentionally says something to someone about the external world. But the ethnographic examples he cites in support of this notion invariably entail the use of metaphorical language by individual informants to explain to individual anthropologists, within particular historical contexts, what they understand to be the meanings of material things. Tilley’s approach is thus vitiated by the
It is questionable, however, whether pragmatic functional constraints do in fact operate in the way Hodder proposes to create a simple “universal grammar” of material meanings. It can be argued that the symbolic meanings of artifacts and of natural phenomena are as arbitrary and culture-specific as any language. In that case, the practical problem of semantic decipherment (in terms of external “reference” as opposed to internal semiotic correlation or “sense”) is insurmountable in the absence of written texts or native discourse that shed light on the symbolic meanings of material things for specific human beings in specific temporal contexts. We might be justified in speaking of “material culture as indecipherable text,” but of course this means abandoning any attempt to interpret material culture in itself as a vehicle of discursive reference, which Hodder is clearly unwilling to do.

But assuming for the moment with Hodder that universal functional limitations on the significations of artifacts do exist, the question is whether these natural constraints permit us to draw an analogy between material culture and linguistic texts (consisting of non-natural arbitrary signs) in the first place. As Hodder (1991:127) admits, “in many ways material culture is not a language at all—it is more clearly action and practice in the world.” The breakdown in the linguistic analogy is greatly exacerbated, moreover, when we consider the problem of reference, which operates at the level of the sentence, not the sign. While we might be able to posit some kind of self-contained signifying function for a given artifact in opposition to other artifacts within a particular unconscious semiotic system, it is more difficult to conceptualize a mute artifact or collection of artifacts as an intentionally organized event of discourse that makes a predication about the world, on the analogy of the sentence.

To be sure, many artifacts, both individually and collectively, can be assumed to be the product of a motivated pattern of social action, which itself is analogous to a text, according to Ricoeur. Presumably, this is what Hodder has in mind when he compares a material artifact such as a ceramic pot to a sentence:

Certainly, the pot is the result of a long sequence of activities or production “statements,” and this sequence is continued in the use and discard of the pot. . . . The purposeful and motivated nature of sentences contrasts with the more abstract nature of word meanings [signs]. The chaînes opératoires of material production are, like sentences, embedded in a real-world context and their meanings are yet more closely related to that context. [Hodder 1992:203]

But we are still left with a highly problematic interpretive gap between, on the one hand, a functionalist reconstruction of a sequence of externally observable activities involving the production, use, and discard of material artifacts, and, on the other hand, the subjective meaning of those activities to the social actors themselves. This is the crux of the difference between Ricoeur’s model of the text and Hodder’s notion of material culture as text. Hodder’s effortless movement from functional context to textual meaning—his ready equation of the material residue of practice with the social meaning of material artifacts—calls into question the degree to which he, as an “interpretive” archaeologist, actually endorses the hermeneutical idea of what constitutes the proper object of study in the human sciences. As I have noted in the previous chapter, this object of study, which for nearly a century has been characteristic of hermeneutics and of the related Weberian tradition of interpretive social theory, is not “social

Note that Saussure (1959:68) himself regarded material symbols as deviations from the semiological ideal of arbitrary signification, to the extent that there is a “natural [nonarbitrary] bond between the signifier and the signified.” For example, “the symbol of justice, a pair of scales, could not be replaced by just any other symbol, such as a chariot.”

8 See, e.g., Marshall Sahlins’s trenchant critique of anthropological functionalism and materialism in Culture and Practical Reason (1976a:206): “The unity of the cultural order is constituted by . . . meaning. And it is this meaningful system that defines all functionality. . . . It follows that no functional explanation is ever sufficient by itself; for functional value is always relative to the given cultural scheme. [Thus] no cultural form can ever be read from a set of ‘material forces,’ as if the cultural were the dependent variable of an inescapable practical logic . . . . It is not that the material forces and constraints are left out of account, or that they have no real effects on cultural order. It is that the nature of the effects cannot be read from the nature of the forces, for the material effects depend on their cultural encompassament.”

9 Note that Saussure (1959:68) himself regarded material symbols as deviations from the semiological ideal of arbitrary.

10 It is true that Hodder appears to have embraced a bewildering variety of quite disparate philosophical positions over the years, of which hermeneutics is only one. His work is thus typical of what Johnsen and Olsen (1992:420) have identified as “a worrying trend of ‘intellectual pot-latching’ in the current theoretical debate in archaeology. In this debate it often seems to be more important to cite as many (new) philosophical and theoretical works as possible, than to seriously discuss some of the fundamental problems raised in one or two of those works (not to say considering some of the contradictions between them).” But insofar as Hodder has drawn more careful distinctions among antipositivist philosophical approaches in his later writings, we must take note of his repeated invocation of Ricoeur’s hermeneutical theory, in particular.
behavior” as seen from the outside, but “meaningfully oriented behavior.”

In other words, the “text” for Ricoeur is not the residue of intentional social action, not the authorless “signs” that make up the world from a semiotic perspective, but meaningful social action itself, as the subtitle of his famous essay indicates: “The Model of the Text: Meaningful Action Considered as a Text.” What is exteriorized and “fixed” as the semantically autonomous meaning of social action are not material signifiers, but “objective” configurations of temporal social action (social structures), which must be understood in terms of ideal-typical motivations. Furthermore, the referential dimension of such a “text” consists not in its relating material signifiers to mental concepts, but in its disclosure of an intuitively understandable lifeworld of “existential perplexities, human predicaments, and deep-rooted conflicts” (Ricoeur 1991:166).

In practical terms, of course, our access to such “texts” of past social action is normally by means of ordinary written texts, although these need not be immediately contemporaneous with such action in cases where a plausible ethnohistorical analogy can be drawn (for example, when the temporal and cultural distance is not great or some sort of civilizational continuity can be demonstrated). Thus, despite the radical methodological claims made by leading postprocessual archaeologists, Ricoeur’s hermeneutical theory, at least, leads to a fairly traditional (“pre-processual”) view of archaeological interpretation.

It is true that from a strictly semiotic perspective, access to the subjective meaning of social action via contemporary written texts is irrelevant, because all that matters are the internal correlations among arbitrary signs, which are held to be indicative of an unconscious collective semiotic code. In this the legacy of Émile Durkheim’s methodological holism is evident, as mediated by Ferdinand de Saussure, who held that synchronic linguistic systems reside “in the collective mind [conscience collective] of speakers” (Saussure 1959:100). But if we reject full-blown structuralism and poststructuralism, as Hodder does, and wish to move from sense to reference, we must face the methodological implications of the linguistics and temporality of human being-in-the-world. Ricoeur’s hermeneutical model of the textuality and narrativity of social action does not eliminate the practical necessity of written documentation in the historical sciences. In the end, whatever our opinion of Hodder’s functionally constrained “universal grammar,” we are left with the traditional view of past material culture as sometimes symbolic but not “textual,” in Ricoeur’s nonstructuralist sense of the word. Furthermore, from a hermeneutical perspective, material artifacts are symbolic only with respect to a linguistically mediated lifeworld, to which we have access, in most cases, only via written texts.

It is rather ironic, therefore, that Hodder takes Ricoeur’s hermeneutical idea of “social action considered as a text” and interprets this in essentially positivist terms by appealing to objectifying analyses of externally observable behavior, in an attempt to relate mute remains to social-structural meanings on the basis of cross-cultural physical and functional exigencies. The resulting neglect of individual human agency and historically contingent subjective meanings is carried over into Hodder’s subsequent attempt to apply to archaeology Ricoeur’s concept of the basic narrativity of history (see Hodder 1993 on “The Narrative and Rhetoric of Material Culture Sequences” and his essay “Material Culture in Time” in Hodder et al. 1995:164–68). Unlike Ricoeur, Hodder does not see narratives as operating at the level of individual social agents, be they social actors of the past or present-day historians who are engaged in the “emplotment” of human action within a narrative framework. Instead, as Jonathan Last points out, Hodder sees narratives as Durkheimian collective representations or determining “structures” of the type that both Fernand Braudel and Claude Lévi-Strauss have invoked.11 It seems that Hodder, here as elsewhere, falls back into the very objectivism he sets out to refute.

Hodder’s vigorous critique of the positivist assumptions that underlie processual archaeology has been salutary, in my opinion (and long overdue); but

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11 Last criticizes this misuse of the notion of the narrativity of history (as defined by Ricoeur) as follows: “Hodder seems to make the error of confusing narrative as a means of ‘grasping together’ disparate factors to make sense of the world with narrative as a causal factor in history at the level of the langue durée, going beyond the arguments of White and Ricoeur. Hodder’s scheme replaces the Braudelian prison with the necessity of the story that the past reveals, and in a similar way denies agency” (Hodder et al. 1995:148). In a similar vein, Johnsen and Olsen (1992) point out the objectivism of Hodder’s earlier theoretical approach in the 1980s, as well, which was based on the Romanticist hermeneutics of Collingwood and thus, indirectly, of Dilthey (compare Dilthey’s “objektiver Geist” and Hodder’s “universal grammar” of material meanings). Johnsen and Olsen err, however, in citing Ricoeur alongside Gadamer in defense of their own dismissal of hermeneutics as a basis for sociohistorical method (p. 434). It is Ricoeur’s view of the necessary dialectic between explanation and understanding, not Gadamer’s rejection of method, which Hodder cites in support of what he calls the “guarded objectivity of the past.”
his view of “material culture as text” is highly questionable. A critique of “knowledge and human interests” might trace the philosophical inconsistencies in Hodder’s approach to an overriding concern for the interpretive independence of archaeology as a discipline. Indeed, Binford and Hodder are in harmonious agreement in their rather inflated view of the viability of purely archaeological reconstructions of social structure, which stems from a deeply rooted belief in what might be called the “semantic autonomy of the artifact.” That Binford as an ardent positivist downplays the need for language is understandable, but that Hodder does so is rather baffling. Here “interpretive” archaeology has inherited from positivist archaeology the belief that prehistory can somehow be transmuted into history.

2. The History of Symbols and the Prehistory of Facts

Hodder’s continuing entanglement with the objectivist and structuralist assumptions that bedevil so-called interpretive archaeology is symptomatic, in my opinion, of a fundamentally misconceived perception of archaeology per se as a human science, that is, a hermeneutical discipline. Over the years, it has been much debated whether “archaeology is history” (a particularistic historical discipline), or whether “archaeology is anthropology” (a branch of a generalizing “science of culture”), or whether “archaeology is archaeology” (a “science of material culture” with its own modes of explanation)—or now, whether “archaeology is the interpretation of material culture ‘texts’” (see Trigger 1989 for a survey of this debate). It seems to me that the best definition is that “archaeology is prehistory,” not in the sense that archaeological data are chronologically prior to written records (often they are not), but because archaeological work is logically prior to history as an enabling explanatory moment in an irreducible dialectic of explanation and understanding—what I have been calling the dialectic of “fact” and “symbol.”

From the perspective of history, archaeology is an auxiliary discipline, like demography or economics, which is called in to get historical understanding in motion again in cases where the direct comprehension of a contingent sequence of motivated actions (or a social structure understood as a repeated pattern of such actions) is impeded.12 From the perspective of any of these other disciplines, of course, there is no question of being dependent upon the problems and modes of investigation defined by historians. Each of these disciplines, including archaeology, is a nonhermeneutic “prehistoric” science with its own problems and its own generalizing and objectifying methods of explanation—methods that are appropriate to a purposely restricted domain of human behavior and to a carefully demarcated level of analysis.

Local Explanation and Global Understanding

So defined, “prehistory” and “history” are two different “language games” which have an equal right to exist. But we must avoid the easy eclecticism that accepts as equally valid the conflicting views of social structure and social change that these two paradigms will produce if prehistory is treated as somehow autonomous and absolute, and thus capable of providing global causal explanations of sociohistorical phenomena. The two distinct language games of prehistory and history—of causation and motivation, explanation and understanding, environment and lifeworld, fact and symbol—also intertwine and overlap, as Ricoeur has noted; indeed, they are frequently conjoined in the same researcher. This is so because the cross-cultural correlational and genetic models of the various branches of “prehistory” find their ultimate justification in hermeneutical understanding, while this understanding itself depends on the intervention of such models.

As Ricoeur (1991:142) puts it, historical understanding “envelops” prehistoric explanation, even as such explanation “develops” historical understanding analytically, a view he has summarized in the pithy motto that “we explain more in order to understand better.” On the one hand, history without prehistory is not a science; but on the other hand, prehistory without history is not a human science.

Thus it is not simply a matter of “multiple ways of knowing the past,” as Robert Preucel (1991; 1995:148) suggests, or of a simple “complementary” relationship between “positivist methods and humanistic forms of analysis that focus on subjectivity, agency, and the historical transmission of knowledge,” as Bruce Trigger (1998:1) proposes. Preucel and Trigger are representative of many theoretically oriented archaeologists today in advocating the integration of processual and hermeneutical approaches, but in my opinion they still grant too much autonomy to positivist attempts to determine, on a global socio-

12 This view is expressed in more concrete terms in the 1971 essay “Archaeology and History” by the eminent historian of ancient Greek and Roman society, Moses Finley (reprinted in Finley 1990); see further the more recent discussion of the relationship between the archaeology of Greece and both documentary history and anthropology by Ian Morris (1994).
historical level, the “cause-and-effect relationships that give rise to cultural process” (Preucel 1991:28), or to view the “cognitive realm” as something which “has evolved, both as a biological potential and in its culturally specific expressions, as the means by which people adapt to their social and natural environments” (Trigger 1998:34). The relationship between hermeneutical and causal analyses is not an equal partnership. By continuing to speak of “culture process” as something that can be “empirically observed by means of neo-positivist approaches” (p. 29), Preucel undercuts the hermeneutical paradigm to the point where “the meaning of cultural systems” may be readily placed within an unchallenged objectivist framework. Similarly, Trigger ignores the phenomenological critique of positivism and takes his stand on the old Cartesian dichotomy between subject and object, whence he falls into a familiar kind of mechanistic materialism. In contrast, although I acknowledge that causal explanation is in some ways independent of understanding, I agree with Ricoeur that hermeneutical understanding remains the encompassing horizon of the human sciences, enveloping all local causal models.

In my view, therefore, Lewis Binford’s nonhermeneutic definition of archaeology is more defensible than Ian Hodder’s model of material culture as text. Binford repeatedly cites Albert Spaulding’s (1960: 437) cogent and very precise description of archaeology in terms of “the fundamental operations of archaeology on its empirical data.” As Binford (1989: 3) puts it: “We do not study human behavior . . ., we do not study symbolic codes . . ., we do not study social systems . . ., we do not study ancient cultures, . . . nor do we study the past. We study artifacts.” Archaeology, pure and simple, is the study of “artifacts, in the broadest sense of the word, and relationships among those artifacts, as well as with associated indicators of the environment and other natural conditions that existed when the artifacts were produced, used, and discarded. Archaeologists study [these relationships] . . . in terms of the dimensions of form and space and the inferred dimension of time” (ibid., p. 28). So defined, archaeology ignores subjective meanings and confines itself to establishing the externally observable “relationships” or statistical correlations, synchronous and diachronic, among archaeological units of analysis (“artifacts and environmental indicators”).

In many ways, this is analogous to the structuralist analysis of myths and other texts, viewed as an internal correlational method, which is why Hodder can so readily invoke objectifying analyses in his attempt to determine the semiotic codes underlying artifacts. Ironically, Spaulding’s modest and rigorous definition of archaeology is more “structuralist” than that of many explicitly semiotic archaeologists. Spaulding himself notes the analogy with structural linguistics:

In some respects [artifact] attributes are analogous to linguistic phonemes. They represent minimal units of meaningful behavior; they are taken to be constant for comparative and descriptive purposes; and they are articulated to form the artifact, which can be regarded as the minimal independent unit of material culture. In the simplest terms, the problem is what to do with a collection of artifacts and a list of formal attributes which the artifacts exhibit [i.e., in terms of the formal correlation and ordering of these units of analysis]. [Spaulding 1960:442]

Structuralism is an objectifying method that ignores the subjective, temporal consciousness of the social actors who produce works of discourse. It focuses exclusively on objectively observable correlations among abstractly defined units of analysis, which are held to reflect deep structures of the human mind. Following Spaulding’s strict definition, we might say that archaeology explicates the unconscious structural “sense” of human artifacts, but does not comprehend their “reference.” But just as in the structuralist study of myths, most archaeologists are not content to stop with these internal correlations—this “algebra of constitutive units,” as Ricoeur calls it. Armed with the functionalist assumptions of objectivist social science, they try to reconstruct the patterns of social action which gave rise to the artifacts they observe, and to develop generalizing causal models to explain why those patterns changed.

Yet when archaeologists go beyond the “sense” of archaeological data (the synchronic and diachronic correlations among artifacts and other archaeological units of analysis) to their “reference” (the changing patterns of meaningful social action we call social structure), they are engaging in a hermeneutical enterprise—“history,” in the broadest sense of the word.14 When Binford moves from the analysis of artifact correlations to statements about “cultural

13 In keeping with his own methodological stance, of course, Spaulding should have stayed on the plane of formal abstraction and spoken of “minimal units of archaeological analysis” rather than “minimal units of meaningful behavior.” Here is a case which confirms his fear that “behavioral inferences may creep in, but they will be evidence of weak-mindedness” (p. 437).

14 This formulation sets aside the question of what counts as an “artifact.” As postprocessual archaeologists rightly point out, the delineation of archaeological units of analysis is not a neutral or objective enterprise, but depends on the background and problem-orientation of the investigator, and so is itself a hermeneutical operation.
processes,” he takes off his archaeologist’s hat and puts on the historian’s hat, because he is now creating a narrative “plot.”

From a hermeneutical perspective, the problem that he then faces, like Hodder, is how to validate his reconstructions of meaningful social action in the absence of relevant linguistic evidence. Again, it is not a question of simply reconstructing externally observable patterns of behavior, which archaeologists can often accomplish quite well. It is a question of relating such behavior to social “structure,” a task that must take account of linguistically mediated subjective meanings, unless one is prepared to defend an objectivist ontology and epistemology.

The Spaulding-Binford definition of archaeology, narrowly construed, is the easiest to reconcile with actual archaeological practice. By and large, in current usage the term “archaeology” describes a type of practical fieldwork, characterized by excavation or site-survey and various technical descriptions and analyses geared to the reconstruction of spatial and temporal patterns of distribution of artifacts (including architectural artifacts) and other materials affected by human activity. The fact that these fieldwork techniques, and thus the “lived experiences” of field archaeologists, are broadly similar across geographical regions leads to a sense of disciplinary solidarity which in turn motivates the search for a definition of archaeology as an independent interpretive discipline within the human sciences. Within the positivist paradigm, archaeology can indeed claim this sort of interpretive independence as a generalizing “science of culture.” But if one rejects positivism in philosophy and functionalism in social theory, then, properly speaking, it is history (broadly defined) and not archaeology which is the interpretive discipline having to do with past social action, because history deals with the linguistically mediated symbolizations that constitute the social order. Archaeology, on the other hand, belongs to “prehistory.”

Levels of Archaeological Inference
This raises the question of the precise mode of integration of the formal explanatory methods characteristic of archaeology and other “prehistoric” disciplines, on the one hand, and the enterprise of creating a “true” historical narrative that may be grasped by means of the faculty of subjective understanding, on the other. That is to say, how is the dialectical interrelationship between the prehistory of facts and the history of symbols to be implemented in practice? Here we must consider the frame of reference of the causal models that archaeologists develop. As Karl Butzer (1982; 1990) points out, we must distinguish between our lower-level inferences concerning local patterns of human behavior and higher-level inferences having to do with social organization and social change on broader spatial and temporal scales.

Butzer (1990:103) notes that: “At the micro-level, archaeologists deal with artifacts (sensu lato), artifact-patterning, and matters of basic intrasite or intrastructural activity-patterning. Perhaps 95% of the empirical and analytical work of archaeology is expended at this level, and this is what archaeologists do best.”15 At the “meso-level” of social organization, however, and most certainly at the “macro-level” of large-scale social change, socioeconomic reconstruction is extremely difficult in the absence of written records. This is demonstrated by cases (e.g., ancient Egypt) in which available historical documentation flatly contradicts what would have been inferred socioeconomically on purely archaeological grounds. Butzer sharply criticizes processual archaeology’s reliance on global deductive principles of “least effort,” cultural uniformitarianism, and ethnographic analogy. He concludes that:

Going beyond the level of linking artifacts and artifact-patterning with direct human activity . . . underestimates the critical matter of cognitive behavior, alternative choices, and multiple means to achieve a particular goal. . . . At the meso-level we are ultimately left with little but models or hypotheses that do not appear to be testable in prehistoric contexts, at least not without affirming the consequence. There is a deep chasm between a reasonable interpretation of micro-archaeological activity patterns, on the one hand, and socioeconomic reconstruction, on the other, one that is not bridged by any “robust” laws. Inferring degrees of social differentiation from archaeological data is one thing. Identifying a particular kind of social hierarchy is quite another. [Butzer 1990:104f.]

Despite Butzer’s rather pessimistic critique of the inadequacies of processualist reconstructions, his own approach remains strongly positivist.16 He con-

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15 Compare Binford’s advocacy of the development of empirically testable “middle range theories” as a solid base upon which to construct meso- and macro-level socioeconomic models (to use Butzer’s terms). In this regard, however, it is worth noting that Binford’s own ethnarchaeological research deals with very simple societies in which natural ecology has a direct and powerful influence on subsistence behavior.

16 The same can be said of Colin Renfrew’s “cognitive-processual archaeology,” which pays more attention to “cognitive and symbolic aspects” than did the earlier “functional-processual” archaeology, but is still fundamentally quite positivist. For Renfrew, the symbolic aspect is an independent factor in social life which must certainly be
continues to advocate the cultural-evolutionary “human ecosystem” paradigm as an encompassing framework for sociohistorical explanation, within which “cognitive systems” appear only as one factor among others—a wild card that disturbs the smooth functioning of ecological models. But from a hermeneutical perspective, there is a basic qualitative or logical difference, and not just a difference in quantitative scale, between Butzer’s micro-level, on the one hand, and his meso- and macro-level, on the other. Butzer’s micro-level should be viewed as the level of objectivist analyses of externally observable behavior occurring in an externally observable environmental setting. This is so if one considers the fact that all human behavior can be regarded as local or “intra-site” behavior to be studied at the micro-level, including not only activity-patterns in the household or subsistence sphere, but also economic, political-military, and religious behavior of all kinds, regardless of the quantitative scale of the behavior in terms of the number of persons acting together or the spatial and temporal scale of the subsequent impact of their behavior.

Butzer’s meso- and macro-level of analysis, on the other hand, cannot be addressed in an objectivist manner at all but must be tackled hermeneutically, because meso-level social organization (and hence macro-level change in social organization) is a matter of the shared understandings of social action which constitute whatever social organization there is. These shared understandings shape and enable individual social action, often independently of environmental constraints. It is true that objectivist ecosystemic analysis can and does operate quite well at Butzer’s micro-level, which, as he points out, is where most archaeological interpretation in fact takes place. But it breaks down at the meso- and macro-level of social organization and social change, because here the frame of reference must shift from the objective environment to the lifeworld, from the externally observable behavior of individuals to the virtual collective “structures” that are formed by patterns of meaningful social action. Moreover, these patterns must be analyzed in terms of their logical and symbolic “inner connections”; that is, in terms of ideal types of subjective motivation.

What micro-level ecosystemic causal models (including demographic and economic models) contribute to the meso- or macro-level—the level of hermeneutical understanding—is a narrowing of the range of interpretive possibilities in terms of functional and physical exigencies. These sorts of externally derived constraints are necessary for refining and testing ideal-typical or “motivational” models of social organization and social change, as has often been noted. But the encompassing conceptual framework for sociohistorical reconstruction is not the evolving “human ecosystem,” whose workings cultural evolutionists hold to be predictable in principle, like any natural ecosystem, as the result of recurrent causal processes (albeit unpredictable in practice because of the paucity of empirical data concerning initial conditions and the difficulty of specifying and measuring all of the relevant variables, especially “cognitive” variables). Rather, from a hermeneutical perspective, the encompassing conceptual framework is a singular, unpredictable narrative of motivated human action, within which causal explanations facilitate comprehension of patterns of action that might otherwise remain mysterious.

Motivational Models versus Causal Models

Even if contemporary textual evidence happens to be available in abundance, there are serious problems with an objectivist meso- or macro-level analysis of an hypostatized “social system.” It is questionable whether one can present a valid objective anatomy of global social organization viewed from above, or can invoke an overarching linear causal model of social change, without adopting a methodological holism that is difficult to defend both theoretically and empirically. This issue will be discussed further in chapter 3, entitled “Rethinking Social Systems.” It is worth noting here, however, that my critique of the ecological/systemic paradigm in archaeology can be expressed in terms of what has come to be called “complexity theory.” Hermeneutics and interpretive social theory call for ideal-typical generalizations which may be viewed as models of the “local rules” of social interaction, rather than as models of an inherently unpredictable “global order.”
To be sure, such models of recurring motivations and complexes of motivations (i.e., types of meaningfully oriented social action) must take account of “prehistoric” micro-level reconstructions of the empirical facts of human behavior and related material conditions, including those objective environmental constraints created by habitual economic and social behavior, which can be modeled on a limited scale in causal, ecosystemic terms. But this prehistory of facts is always incorporated within a history of symbols—a history of the many interrelated types of human motivations and their synchronic and diachronic configurations. Furthermore, these motivational models may be abstracted from past social action only to the extent that they can be related to “native” linguistic expressions of the meaning of social action, for the shared understandings that constitute social order inhère in structuring symbols that are always expressed in language.

This is not to deny that plausible motivational models may sometimes be constructed even in the absence of written texts, provided that subjective meanings can be inferred from plausible ethno-historical analogies. For example, in a widely cited article entitled “Anthropological Perspectives on Ancient Trade,” Robert Adams (1974) cites a variety of ethnographic and historical evidence in order to explicate the dialectical relationship between long-distance trade and political centralization (i.e., state formation, especially “secondary state formation”) by means of a retrodictive motivational model, as opposed to a predictive causal model, although he does not use these terms. Adams’s study can be interpreted as showing that, in general, the correlation between ancient trade and urbanization should be understood in terms of the symbolically mediated actions of individual goal-motivated social actors, whose goal is typically the prestige or higher social status achieved by accumulating either physical wealth or what Pierre Bourdieu (1977:36–41; 1991:72ff.) has called “symbolic capital.” This capital translates into increased authority over ever larger numbers of reciprocally obligated dependents or clients, not through an automatic or unconscious natural process, but thanks to the statistical preponderance of certain typical human motivations. Here a motivational model in terms of ideal types describes the recurring relationships between “entrepreneurs” (an ideal type), on the one hand, who by luck or initiative secure preferential access to trade with outsiders, and their economic dependents-cum-political followers, on the other hand, who wish to be associated with such a leader in order to increase their own status or material benefits.

The resulting dialectic of fact and symbol involves, on the factual level, externally observable changes in settlement pattern and in material culture as villages are abandoned and urban centers grow, together with changes in productive activities stimulated by increased trade in certain goods. On the symbolic level, it involves new ascriptions of status and new modes of legitimation of political authority, which are at some level culture-specific and discernible only through native linguistic expressions. This dialectic of fact and symbol can be self-reinforcing, leading to rapid and massive changes in the external facts studied by archaeologists and to a parallel transformation of the traditional political symbols that structure the social order—as, for example, when patriarchal authority is extended to larger social groups in what Weber called a “patrimonial” regime (see chapter 3.1).

Even quite impersonal causal models in prehistory can be recast in motivational terms, because subjectively meaningful individual motives are implicit in these models. For example, “population pressure” or “circumscription”—a favorite of ecosystemic explanations of social change—must be understood in terms of the dialectical relationship between the facts of demography and shared understandings of those facts with respect to perceptions of crowdedness, allowable forms of competition for space, ways of handling social conflict, and so on. Of course, such models remain highly speculative in the absence of texts that could validate them in particular historical settings. But the fact that the key ingredients in many overarching “causal” models can be rendered in motivational terms suggests that the ecosystemic paradigm must inevitably take account of subjective meanings, with all that this implies for its adequacy as a global explanatory paradigm.

This is evident in Butzer’s own essay on “Socio-political Discontinuity in the Near East c. 2200 B.C.E.” (1997), which bears some resemblance to Adams’s earlier study. Butzer rejects the environmentally deterministic hypothesis of a major climatic shift to account for sociopolitical devolution in both Palestine and Egypt at the end of the third millennium B.C. Instead, he invokes a dynamic, systemic model in which “the key elements of a societal system are energy flows (primary production, redistribution, trade) and information pathways (technology, structural nodes, institutions) that explicitly function in response to human cognition and decisions. As these flows and pathways expand, the system grows in complexity: when they retract, it declines and simplifies” (p. 280).
Butzer’s overarching system here consists of a global causal model which encompasses a local motivational model. The motivational model embedded in his system is necessary to account for the decision made by the Old Kingdom Egyptian pharaohs (according to Butzer’s hypothesis) to expand their political power by monopolizing trade between Egypt and Palestine. It is also needed to account for the parallel decision by primary producers in Palestine to increase production in order to meet the demand from Egypt. That decision was presumably likewise motivated by a desire to expand political power on the part of those who controlled the trade on the Palestinian side, whose monopoly led to secondary political centralization and the expansion and fortification of towns there. Butzer then speculates that the continued operation of the same basic motive—to expand political power—eventually led to the decline of the system: “Based on circumstantial evidence, Egypt seems to have decided to enforce its hegemony over Palestine at all costs [i.e., militarily], leading to catastrophic destruction of its trade, its towns, and its essential productivity . . . weakening the system . . .” (p. 282).

Now, Butzer would be the first to deny that this presumed pharaonic militarism was somehow automatic or determined by economic conditions. He insists that his systemic model “is neither mechanistic nor anonymous. It presumes a strong cognitive component, and accords a central role to ‘prime movers’ such as imperial ambition, war, invasion or pandemics that figure prominently in the historical record and will have been no less important in proto-historic times” (Butzer 1997:286). But his acknowledgment that “imperial ambition,” for example, is a historically contingent phenomenon is more damaging to the systemic perspective than Butzer admits. Historical and ethnographic sources provide ample warning of the dangerous anachronism entailed in flattening all historical actors into the same mold, as rational maximizers of what we think was their self-interest, without attention to the culturally specific “history of symbols” by which imperialist and militarist decisions and actions are mediated. In other words, it is an empirical question why a pharaoh might invade Palestine and destroy towns there; it is not a matter of deduction on the basis of oversimplified cross-cultural assumptions about his reasons for acting, for a variety of imperial motivations is possible, including motivations which have nothing to do with the control of trade or even the maintenance of domestic political power (see the discussion of Egyptian imperial ideology below in chapter 4.5, in the context of my discussion of “world-systems”).

Yet if we must deal with multiple motivational models, each of which has its own context and relationship to political and economic choices, then it is hard to see any systemic articulation among the various components of Butzer’s hypothesized system, because the factors that foster an increase in economic and political integration (“systemic complexity”) may be unrelated to the factors that foster economic and political devolution (“systemic simplification”). Warfare can be an unpredictable wild card in relation to a given economic system, rather than a cause or consequence of that system.

Indeed, the heretical thought arises that there might be no systemic connection even between political integration, on the one hand, and economic integration, on the other, because the legitimation of political authority and the desire for political authority may be based in such unpredictable factors as a leader’s personal charisma, or simply in the traditional sanctity of an inherited role, so that political integration relies on symbolically mediated patterns of action which have little to do with economic motivations and concerns. This violates the economistic assumptions of much modern archaeology and historiography, but again it should be an empirical question and not a matter of functionalist deduction whether there is a systemic link between political and economic phenomena in a given case.

The Human Ecosystem and Social Evolution

More fundamentally, in my view, little is gained by speaking in scientific terms of a global social network of hypostatized “structural nodes,” “energy flows,” and “information pathways” (which are mechanistic and anonymous, notwithstanding Butzer’s disclaimer), rather than making explicit the fact that these nodes and flows are secondary abstractions of contingent patterns of human behavior which must ultimately be understood in terms of the motivated, symbolically mediated actions of individual social actors. It is for this reason that a history of symbols encompasses all causal systemic models, limiting their scope and their pretensions to be global explanations of social phenomena.

In this regard, it is worth noting that the pleas for more nuanced “multicausal” or “multilinear” explanatory models which have been heard in processualist circles imply the abandonment of the original positivist ideal of discovering cross-cultural historical “covering laws” that could be the basis of historical deduction. This is because “multicausality” is much better expressed within the framework of an explicit rejection of causal explanation altogether, on a global level, in favor of dialectical motivational models of
local interaction. Explanatory models in prehistory which consist of multiple causes and multiple trajectories, apart from being highly speculative and difficult to falsify, are so vague as to call into question the very notion of linear chains of cause-and-effect. As I argue in more detail below in chapter 3.2, it is better to conceive of a multitude of nonlinear dialectical interactions between facts and symbols, occurring simultaneously on different levels and resulting in unpredictable global changes, while manifesting certain regularities that can be attributed to cultural “tradition”; that is, the ongoing transmission and transformation of symbolically mediated “local rules” of interaction within a given context.

My approach is thus superficially similar to that of Adams (1984) in his programmatic essay on “Mesopotamian Social Evolution” (see also Adams 1988). He notes the limitations of the systems paradigm in archaeology, in light of the fact that “the world generally consists of open, poorly articulated systems and loosely interacting subsystems whose relationships to one another and to their environments are ambiguous and indecisive” (p. 86). But despite his invocation of human “consciousness and purpose,” Adams, like Butzer, continues to adhere to objectivist cultural evolutionism, albeit in a qualified and attenuated form. He rejects “classical Darwinism,” noting that variations which affect the evolutionary “selection” or differential survival of “socio-culturally constituted groups” (which he views as the units of selection) are not necessarily random but may be the product of active and purposeful strategies. Nonetheless, Adams is clearly unwilling to reject evolutionaryism as an encompassing paradigm, asserting that “an eco-system framework (although hardly Darwininan) can be a common starting point” for historians, archaeologists, and anthropologists (p. 119). Although he qualifies considerably the notion of “laws” of cultural evolution and emphasizes the complexity of evolutionary processes, he still maintains that, “taken in sufficiently broad and flexible terms, an evolutionary approach continues to be fruitful” (p. 121).

But on what grounds? The evolutionist ecosytemic approach is possible only on the basis of a naturalist and positivist epistemology, as Adams himself recognizes: “As in the natural sciences more generally, those involved in tracing the evolution of culture tend to be aligned on the formally explanatory, ‘laws-and-instances,’ rather than ‘cases-and-interpretations,’ side of the diffuse polarity (or complementarity) in the social sciences” (p. 119). Yet the polarity in question is not a “complementarity” but an irreconcilable opposition on any global “evolutionary” level of analysis (i.e., on Butzer’s meso-level and macro-level of social organization and social change). It is not as easy to combine positivist and hermeneutical approaches as Adams and many other archaeologists appear to believe. Adams’s approach is thus another example of the overly casual theoretical eclecticism that is prevalent in archaeology today, in which explanation and understanding are treated as somehow compatible within what remains a basically positivist framework, and nothing of substance is conceded to the root-and-branch philosophical critique of objectivism that underlies current hermeneutical approaches. Yet there is no viable philosophical framework within which one may salvage the essentials of a positivist perspective and at the same time admit, with Adams, that cultural evolutionists “find it more and more necessary to acknowledge and learn from the indeterminacy and reversibility of short-term change and the not infrequent immunity to change of underlying structures of thought and action” (p. 120). From a nonpositivist hermeneutical perspective, the obvious empirical weakness of the cultural-evolutionist paradigm is rooted in its philosophical weakness. However much it may be qualified and redefined, cultural evolutionism defines “culture” in terms of externally observable patterns of behavior rather than taking as its fundamental starting point the subjective meaning of social action.17

Ultimately, then, the global claims of the systemic paradigm, in archaeology and elsewhere in the social sciences, must be addressed on a philosophical level. As was noted above, Ricoeur has treated the interrelation of causation and motivation in historical interpretation in terms of the general interpretive dialectic of explanation and understanding. In particular, Ricoeur cites the Finnish philosopher Georg von Wright (1971; 1980), whose work in action theory draws on general systems theory. Using a mode of

17 Jonathan Haas (1998) notes the new “bottom-up” emphasis (i.e., methodological individualism) in recent cultural-evolutionist approaches, e.g., the neo-Darwinian “selectionist” model of cultural evolution and the “agent-based modeling” approach using computer simulations of the behavior of individual social agents. These approaches try to avoid a theoretically indefensible reification of overarching social “systems” or “structures,” focusing instead on individual choices and their secondary collective effects. But what then is the unit of evolution and selection if collective social structures are not real, objectively bounded entities, but are simply patterns of individual action defined according to a given investigator’s particular criteria? The notion of social evolution is meaningless from a truly agent-oriented perspective, as I argue in more detail below. In practice, agent-oriented evolutionists continue to neglect the role of hermeneutical understanding in studying human agency, relying instead on functional constraints on human behavior as inferred from an external perspective.
logical argumentation derived from post-Wittgensteinian analytical philosophy, von Wright demonstrates that the underlying intellectual precondition for both causal explanation and motivational understanding is the notion of “intentional intervention” in the world. He leads up to this by noting that we cannot conceptualize an unbounded causal system, unless it be the entire universe viewed as the system of all systems. Thus in explaining human action we inevitably deal with closed, partial systems. (This is in contrast to Butzer and other processual archaeologists, who attempt to salvage the “human ecosystem” as an encompassing paradigm by a vague appeal to an “open-system perspective” on cultural evolution.) Von Wright then argues that the closed systems required by the explanatory causal paradigm, most famously articulated in Carl Hempel’s “covering law model,” can be put in motion only by an intervention or “interference” in the course of events that produces the initial state of the system. But such an intervention cannot be conceived of apart from the idea of the human power to act; that is, the ability to intervene in a situation and produce an intended effect. Ricoeur summarizes von Wright’s rigorous logical analysis as follows:

First, we are obviously turning our backs once and for all on the dichotomy between explanation and understanding. For, if explanation belongs to the domain of system theory and understanding to that of motivation (of intentional and motivated human action), we perceive that these two elements—the course of things and human action—are intertwined in the notion of intervention in the course of things. . . . What is more, the notion of interference puts an end to the untenable state of opposition between a mentalist order of understanding and a physicalist order of explanation. On the one hand, there is no system without an initial state, no initial state without intervention, and no intervention without the exercise of a power. Acting is always doing something so that something else happens in the world. On the other hand, there is no action without the [motivational] relation between knowing how to do something . . . and that which the latter brings about. Causal explanation applied to a fragment of world history goes hand in hand with recognizing and identifying a power that belongs to the repertoire of our own capacities for action. . . . Human action and physical causation are too interconnected in this entirely basic experience of the intervention of an agent in the course of things for one to abstract from the first term and raise the second to an absolute. [Ricoeur 1991:136f.]

In practical terms, this philosophical argument means that sociohistorical reconstruction may indeed proceed by means of the explication of local, partial systems (“causal explanation applied to a fragment of world history”). But “intentional intervention in the world” nonetheless remains the fundamental experiential ground of the systems paradigm itself; hence the necessity for Weberian motivational models, framed in terms of subjective meaning, which organize and encompass the partial systemic models.

In the language of Heideggerian phenomenology, the externally observable environment as the field of operation of objectifying scientific analysis always presupposes a more basic lifeworld that is characterized by understanding—by an active “living-ahead” or anticipation of what one’s actions will produce. For Ricoeur, as was discussed above, such understanding is realized in historical research by the employment of past human action in an unpredictable narrative. Here deterministic causal explanations do not constitute a total, enveloping system, but are always at the service of narrative understanding, because these explanations themselves depend upon “an originary capacity of apprehension as regards the meaning of human action” (Ricoeur 1984:132).

Neo-Darwinian “Selectionism”

It should be noted, however, that despite the sustained philosophical critique of positivism over the past few decades from diverse directions, including both the Heideggerian Continental tradition and the Wittgensteinian Anglophone tradition, and despite the parochial archaeological version of this critique in the form of “postprocessualism,” positivist archaeology is still alive and well. This is evident, for example, in the unquenchable biologist that underlies the resurgence of interest in “evolutionary archaeology” in the guise of neo-Darwinian “selectionism” (see O’Brien 1996). Yet as critics such as Anthony Giddens point out, the problem with any form of social evolutionism—however abstracted from biology it may be—is the difficulty of specifying what is meant by evolutionary “survival” and “extinction” in the case of human society, for “there is no real analogue to these circumstances in the social world” (Giddens 1984:272; see also pp. 236ff.). Social evolutionists face a perennial problem in answering the question “what evolves?”—a problem not faced by biologists, who deal with objectively bounded species. When we consider the units of social or cultural evolution, we are thrown back to the prior question of what constitutes “society” or “culture,” and thence to hermeneutical critiques of the tendency of all functionalist or organismic approaches to reify social phenomena (patterns of meaningful action) whose boundaries are not fixed or objectively given, but fluctuate greatly, both temporally and spatially,
depending on the background and problem-orientation of the observer. The problem of defining units of social evolution is not solved by invoking the vague concept of “cultural transmission” as an analogue of genetic transmission (e.g., Shennan 1996). From a nonpositivist hermeneutical perspective, the heritable “cultural traits” that are transmitted over time can never be observed archaeologically, or by any mode of external observation. Insofar as they are cultural, these traits are not simply external social behavior that is somehow slavishly imitated from generation to generation in an unconscious manner. Rather, they depend on the subjectively meaningful symbols that mediate patterns of social action and are learned and transformed in the course of socialization. Such organizing symbols persist and recombine, and are perhaps even resurrected from purely textual sources, in so many different ways, independently of the empirically observed fortunes of particular human communities, that it is impossible to say whether they have “survived” or become “extinct” in terms that are at all analogous to Darwinian natural selection. Indeed, the definition of cultural “survival” is bound up with the research problem and perspective of the investigator who is asking the question. Of course, one can characterize any historical change as “evolution” in the loosest sense of the word, and this is what is done in effect by selectionist archaeologists who define evolution very broadly as “the differential persistence of discrete variants” (O’Brien et al. 1998:487). But this vacuous formulation adds nothing to social theory, and it certainly does not justify the use of the intellectual apparatus of biological evolutionary theory. The fact that “the greatest weakness of evolutionary archaeology to date is determining how to measure [cultural] transmission” (ibid., p. 494), as leading selectionist archaeologists admit, is thus a symptom of evolutionaryism’s failure in principle, and not merely a practical problem that will someday be overcome. Moreover, the symbolic or cognitive aspects of human society cannot be so readily domesticated within an evolutionist framework as many selectionists and processualists believe, simply by speaking of organizing symbols themselves as “discrete variants” whose “differential persistence” over time can be measured scientifically like any other genetic trait. The “evolution” of cultural symbols must be studied in terms of their inner logical connections or “elective affinities,” to use Weber’s term; that is, by an “exegesis” that takes account of their subjective meanings in the context of particular material conditions, and not simply in terms of the statistical correlation of their occurrence as observed from the outside. Thus, despite their claim to an innovative “methodological individualism” (innovative in the context of positivist evolutionism) and their embrace of historical contingency, present-day cultural selectionists remain poles apart from the agent-oriented tradition of interpretive social theory. This is because selectionists continue to rely on materialist or functionalist definitions of the cultural rules of interaction that produce individual behavior—rules which they see as differentially selected for survival in the course of social evolution. What they have failed to provide, however, like the preceding generation of cultural evolutionists and processual archaeologists, is a cogent philosophical defense of the positivist ontology and epistemology that underlie their approach. With respect to the ancient Near Eastern “house of the father,” it should be recognized that the patriarchal household is not simply an externally observable socioeconomic fact that is explicable by evolutionary or ecosystemic models. It is also a structuring symbol of social order that is comprehensible only in terms of the subjectively meaningful motivations that give rise to patterns of social action. Furthermore, by virtue of this symbolic dimension, the “house of the father” may be abstracted into a conceptual unit of analysis, an ideal type, which Weber called “patrimonialism.” In this volume and a planned second volume, I will make extensive use of this ideal type in order to explore the dialectic of motivational understanding and ecosystemic causation in the singular history of the Canaanite and Israelite “house of the father” throughout the Bronze and Iron Ages. In particular, I will rely on formal demographic models and on ecological models of agrarian subsistence behavior in order to understand ancient household and kinship relationships, and the symbolic projection of these relationships to the entire divine-human social order. But I will not view the “house of the father” as a timeless cross-cultural structure that is entirely explicable in terms of these causal models. It has its own culture-specific history, for it is not a recurrent natural phenomenon but rather a contingent pattern of motivated social action. This pattern established itself in the ancient Near East as a very durable, but by no means changeless, civilizational structure, which (speaking metaphorically) experienced temporal growth and decay. Furthermore, the “house of the father” must be understood in terms of both its “sense” and its “reference.” In other words, the explication of the internal correlations among various motivational models related to the patriarchal household (described in terms
of interlocking ideal types and subtypes and supported by causal models) points to the ultimate existential referent of the “house of the father” itself as a social structure, namely, the age-old perplexities (or “aporias”) surrounding human social life. These perplexities are vividly portrayed in traditional narratives from ancient Canaan and Israel; thus our preliminary theoretical discussion of “the sense and reference of human works” cannot stop with issues of archaeological and historical interpretation, but must now turn to the interpretation of myths.

3. Structure and History in Traditional Narratives

I will take it as axiomatic that stories about anthropomorphic deities, in ancient Canaan and Israel as elsewhere, reflect in some way—however distorted—the types of social action familiar to their authors and hearers. Ordinary types of behavior may be highly exaggerated or even inverted, violating normal social conventions; but vivid depictions of the gods as larger-than-life human beings nonetheless originate in mundane experience. We will not progress very far beyond this rather general thesis, however, without a theoretically and empirically defensible method of analysis that can put the narration of mythical events into some coherent relation to what we know of actual social structure (understood as patterns of meaningfully oriented behavior). This requires that we explore the relationship between traditional narratives and the linguistically mediated understandings of social reality that constitute the social order.

In so doing, material factors can by no means be ignored, because new symbolizations of social relationships that effect social change themselves arise through the dialectical interplay of existing traditional symbols, on the one hand, and concrete lived experience in specific material circumstances, on the other. In interpretive social theory, the irreducible dialectic of the “material” and the “ideal,” of the facts of social life and the symbols by which they are understood, is seen as essential to the creation, transmission, and transformation of organizing symbols. This is so because such symbols themselves originate in concrete experience before they enter language and take on a life of their own. As Ricoeur emphasizes in his essay on “Metaphor and Symbol” in Interpretation Theory (1976), the double meaning of a symbol inheres in the correlation of some aspect of prelinguistic experience to what is expressed in language—be it a symbol of the sacred, as studied in the history of religions; or a symbol of hidden psychic conflicts, as seen by psychoanalysis; or a symbolic vision of the world or of a possible world, from the point of view of literary criticism; or, indeed, a symbol of social order, of the kind we are concerned with here.

Dead Metaphors and Rooted Symbols

The “house of the father” is just such a symbol. The patriarchal household was an organizing symbol of ancient Near Eastern society precisely because it was rooted in mundane experience, and this is reflected in Canaanite mythology and in the Bible.18 Now, this rootedness in concrete experience of a symbol like the “house of the father” is what distinguishes it from mere metaphor. Here I am following Ricoeur’s discussion of the relationship between metaphor and symbol, in which he first establishes that a metaphor involves the creation of meaning (“semantic innovation”) through a metaphorical twist or absurd predication (“semantic impertinence”) which is accomplished by juxtaposing a literal and a metaphorical meaning. A metaphor operates at the level of the sentence, not of the word, as classical rhetoric would lead us to believe; it involves a predication, not merely a verbal substitution based on resemblance. A metaphor is thus not merely an ornament of discourse but actually achieves the creation and extension of meaning. As Ricoeur (1976:53) puts it: “A metaphor, in short, tells us something new about reality.”19

The question arises, then, of the connection between metaphors and symbols. Ricoeur notes that symbols, unlike metaphors, partake of two dimensions, one linguistic and the other nonlinguistic. A symbol is accordingly more opaque than a metaphor, more resistant to semantic analysis, because it is rooted in and therefore bound to nonlinguistic experience. As he says: “This bound character of symbols makes all the difference between a symbol and a metaphor. The latter is a free invention of discourse; the former is bound to the cosmos” (p. 61).

Ricoeur goes on to say that precisely because a symbol is bound, or rooted, in this way, it can endure, unlike a metaphor. A metaphor remains an “event of discourse,” a “transitory aspect of language,” a semantic innovation that “exists only in the moment of invention” (p. 63f.). Thus when a metaphor is adopted by a linguistic community, it quickly becomes a “dead

18Our chief source for “Canaanite” mythology (broadly defined) are the Ugaritic myths and epics. These will be discussed in more detail below in chapter 14. Note also the discussion of the scope of application of the term “Canaanite” in chapter 10.1. Biblical narratives will be discussed in a second volume, currently in preparation.

19This point is developed in considerable detail by Ricoeur in his book The Rule of Metaphor (1977).
that certain metaphors

are deprived of its original power to create new mean-
ing as part of the ordinary polysemy of words, but it

is futile to search for an underlying social reality
before and behind the world of symbols to which that
world can be reduced. There is no social “structure”
to which we have access apart from the symbols
themselves.

This is not to say that our experience of reality as
such is exhausted in what we are able to express in
language. The “nonsemantic moment” of linguistic
symbols reminds us that this is not the case. As Ri-
coeur observes, drawing on the phenomenology of
religion, the linguistic expression of religious and
literary insight, in the form of symbols which also
have a nonlinguistic dimension, in itself points to a
fundamental ontology of the indescribable experience
of “power”:

What asks to be brought to language in symbols, but
which never passes over completely into language, is
always something powerful, efficacious, forceful.
Man, it seems, is here designated as a power to exist,
indirectly discerned from above, below, and laterally.
The power of impulses which haunt our phantasies,
of imaginary modes of being which ignite the poetic
word, and of the all-embracing, that most powerful
something which menaces us so long as we feel
unloved, in all these registers and perhaps in others as
well, the dialectic of power and form takes place,
which insures that language only captures the foam
on the surface of life. [Ricoeur 1976:63]

Thus,

there is more in the symbol than in the metaphor.
Metaphor is just the linguistic procedure—that bi-
zarre form of predication—within which the sym-

bolic power is deposited. . . . The symbol is bound in

a way that the metaphor is not. Symbols have roots.
Symbols plunge us into the shadowy experience of
power. Metaphors are just the linguistic surface of
symbols, and they owe their power to relate the se-

mantic surface to the presemantic surface in the

depths of human experience to the two-dimensional
structure [linguistic and nonlinguistic] of the symbol.

[p. 69]

Symbol and Narrative

This brings us to a consideration of how a linguistic
symbol such as the “house of the father” actually
functions within a traditional narrative. Here I will
argue that the formal analysis of myths and other
traditional tales reveals that they derive their basic
plot structures from shared programs of social action.
In this I am following Walter Burkert’s approach in
Structure and History in Greek Mythology and Ritual
(1979). Burkert notes that Vladimir Propp’s (1968)
pioneering study of Russian fairy tales in his 1928
book Morphology of the Folktale has spawned a vari-
ety of “dechronologized” formalist and structuralist methods for the analysis of narratives. Propp himself retained some notion of temporal sequence, however, despite the abstractness of his method, arguing that tales of the type he analyzed could all be reduced to a recurrent series of thirty-one plot “functions”—meaningful units of action which later authors have called “motifemes.” Within any genre, the number of motifemes is limited and their sequence is fixed, even though the characters of the story are interchangeable. Subsequent formal analyses by structuralist theorists (beginning with Lévi-Strauss) have tried more and more to dispense with chronological sequence, aiming to develop a timeless algebra of narrative variables and functions that might reveal the innate unconscious structure of myths and other narratives.

As was noted above, Ricoeur, among others, has attacked the philosophical and empirical weaknesses of structuralism in general and of structuralist analyses of narratives in particular. Ricoeur’s basic point is that human existence is inherently temporal, hence time is an irreducible element in human narratives, and the understanding of narratives (including both fictional and historical narratives) depends on our prior experience of action in time. Time is humanized to the extent of its expression in narrative; narrative, in turn, is meaningful to the extent that it portrays temporal experience.

Fictional narratives in particular, including myths, involve the mimēsis or imaginative redescription of reality via a kind of suspended or “split” reference, in order to disclose something about the world of human action, including new possibilities for action. The interpretation of such narratives, like all textual interpretation, must therefore take account not simply of their immanent “sense” or timeless structure, but also of their external “reference” to a temporal way of being-in-the-world. For Ricoeur, the notions of plot and emplotment provide a corrective to structuralist approaches to narrative which neglect the relationship between individual agency and the action of which the narrative is an account.20

Burkert (1979) makes a similar argument against atemporal structuralist analyses of myth, although the original formalist method of Propp continues to exert an attraction for him, provided that proper attention is paid to the temporal sequence inherent in the plot of a

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21 Burkert gives as examples the Greek stories that recount in many different ways “the girl’s tragedy” of rape and rescue; and the Greek and Hittite myths involving the combat of the storm god and the dragon, which, despite their differences, have the same basic sequence of actions (see Burkert 1992 for a historical explanation of the structural similarities between Near Eastern and Greek myths).
perplexities or “aporias” of human social existence. Furthermore, I would not emulate Burkert’s stress on biologically as opposed to culturally transmitted programs of action, although he does obtain interesting results with his sociobiological approach in the study of Greek myths and rituals (see Burkert 1996; cf. Sahlins’ [1976b] critique of sociobiology).

With respect to ancient Near Eastern myths, it is clear that they are open to a number of different natural and psychosocial interpretations, although we should not look for direct references to empirical historical events. Interpretations in terms of natural phenomena have been especially common for the West Semitic or “Canaanite” mythology known from ancient Ugarit (discussed below in chapter 14). In the famous “Baal Cycle,” in particular, the conflict between Môtu, “Death,” the god of drought and sterility, and Ba’lu, the “Lord,” who is the god of wind and rain, is obviously related to the natural cycle of sterility and fertility (the name Ba’lu is an epithet of the weather god known more generally in West Semitic as Haddu/Hadad; see Greenfield 1995). Similarly, the conflict between the storm god Ba’lu and Yammu, the “Sea,” also reflects natural events.

This has frequently been noted; but it has not been remarked so often that the impersonal natural cycle and awesome natural phenomena like storm and sea are humanized in Ugaritic myth by means of narrative emplotment in terms of temporally sequenced programs of action—programs of action which, I would argue, are evoked by the perplexities of social life in extended patriarchal households (Schloen 1993). In other words, these myths invoke natural phenomena that elicit numinous awe, and at the same time they “redescribe” aporetic human situations involving kinship, inheritance, sibling rivalry, sexual attraction, and the like. Indeed, in Bronze Age Near Eastern mythology in general, with the partial exception of Egyptian mythology, intrahousehold conflict and its resolution are the principal means of symbolizing cosmogonic and theogonic events, thereby expressing and validating human social experiences by placing them on a cosmic scale and relating them to divine origins.

Many of the same programs of action stemming from the “house of the father” can also be found in later biblical narratives; for example, in the patriarchal narratives of the book of Genesis and in the stories about the Israelite kings. Such plot structures include “the quest for an heir” (with subplots concerning “the lack/death of an heir” and “the search for a suitable wife”), “the combat among potential heirs” (with subplots involving “the avenging of a brother” and “the exile of disinherited kin”), “the perils of parricide” (reflecting intergenerational rivalries), and “the ascendance of the rightful heir” (with subplots concerning “the building of a house” and “the giving of a feast”).

It is worth noting, however, that “mythological” narratives which were composed and transmitted even later, in the latter part of the first millennium B.C., reveal quite different plot structures. In Jewish apocalyptic literature, for example, the highly personalized quest for an heir and the single combat among potential heirs that are characteristic of Canaanite and Israelite narratives are replaced by a more impersonal and bureaucratized warfare between the massive armies of light, led by a delegate of a remote monarch, and the armies of darkness, led by a rebellious former subordinate. In my opinion, this is indicative of an epoch-making rationalization and transformation of the “house of the father,” which nonetheless remained a powerful political and religious symbol well into the Roman era and beyond.

Traditional narratives from Canaan and Israel will be discussed in more detail elsewhere in this book, with a view to discerning in them programs of social action that reflect the fundamental structuring symbol of the “house of the father.” These plot structures point to common existential perplexities inherent in ancient Near Eastern social structure. Moreover, this is what ensured that such narratives became traditional, because those who told and retold them were potential actors in the same plots themselves. In these narratives we can see how the internal structural “sense” in terms of such plots leads to an external historical “reference” to social structures (recurrent patterns of meaningful action) that embody a possible mode of being-in-the-world.

But to go beyond the interpretation of traditional narratives to objectifying historical explanations of social structures, and especially to explanations of why these structures change, remains a perennial problem. In the next chapter I will consider the complex relationship between social structure and individual agency, in light of recent reformulations of the notion of social “systems.” Then I will introduce the question of the “rationalization” of traditional symbols, providing a theoretical basis for my argument (to appear in a second volume) concerning the transformation of the “house of the father” as both fact and symbol during the first millennium B.C.
II. THE INTERPRETIVE PARADIGM AND THE ANCIENT NEAR EAST

Chapter 3. Rethinking Social Systems

It should be clear from the preceding discussion that I regard social action as symbolically mediated and constrained, as has been argued by Max Weber and his followers in the interpretive (verstehende) tradition in twentieth-century social theory. Empirical observation indicates that human behavior which can be called “social action” is motivated and guided by the shared subjective meanings that are attributed to actual behavior, both by the actor and by others to whom the behavior is directed implicitly or explicitly.

The implication of this view for sociohistorical research is that functionalist and materialist modes of explanation are inadequate because they unduly limit the explanatory significance of subjective understandings of social relationships and of the symbols by which these are expressed. Moreover, these objectivist modes of explanation can be quite misleading because they suggest that broad predictive generalizations are possible concerning the historical development of various kinds of social organization—in particular, by correlating economic factors with social structures.

In positivist functionalism, which was classically articulated in the late nineteenth century by the French sociologist Émile Durkheim, the primacy of subjective meaning is lost because sociohistorical generalizations are couched in terms of an hypostatized entity called “society” that somehow exists externally to individual social actors. On the model of biological organisms and other physical systems, this social entity is regarded as maintaining its equilibrium and evolving over time through the interrelated working of various reified suprapersonal social “functions,” analogous to the specialized cells or organs of multicellular organisms. Political and religious ideology (what later authors have called the “symbolic subsystem”) is only one such function, rather than what constitutes social order itself.

According to Durkheim (1984), the “division of labor in society,” by which different groups of persons take on specialized tasks, leads to “organic solidarity,” because each social organ depends for its survival on all of the others. As a society grows in numerical size, its division of labor necessarily becomes more and more refined, as a matter of (assumed) functional necessity; hence its structural complexity and level of organic solidarity also increase. Social structure and social functioning are therefore directly correlated with externally observed quantitative measurements, on the analogy of biological organisms.

Similarly, in orthodox Marxism or “historical materialism,” especially as expressed by Friedrich Engels, subjective understandings of social relationships and associated religious and political symbols are not fundamental or autonomous but are part of the epiphenomenal “superstructure” that is secondarily generated by “real” economic factors, in particular by the factual disparity between those who own the means of production and those who do not. Despite their differences, Durkheimian functionalism and Marxian materialism are both species of nineteenth-century positivist objectivism, which (like the otherwise very different Romanticist subjectivism it opposed) entails the “search for a reality before and behind the cultural world to which that world can be reduced” (Rabinow and Sullivan 1987:9).

In reaction to the reductionism of these positivist approaches, Weber and others in the interpretive tradition (including, of course, a good many neo-Marxists) insist that shared understandings of social

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2 This sort of objectivism also characterizes the “structural-functionalism” of Talcott Parsons (1951) that was long dominant in English-speaking circles. Parsons attempted to combine Durkheim’s approach with that of Weber. Like Weber, he viewed the concept of motivated “action” as fundamental, but he related “unit-acts” to one another in a functionalist manner in terms of “action systems.” These are components of what Parsons schematized as the separate major subsystems of the social system as a whole, which maintains a state of equilibrium by adapting to changing conditions and evolving into new forms. Despite his references to Weber, therefore, Parsons’s theory is dominated in the end by the positivist notion of the functional equilibrium of social systems, and hence diverges radically from Weber’s more hermeneutical action theory, as many critics have noted (e.g., Zaret 1980).
reality, and the symbols that express them, are not reducible to economic or political interests, nor are they to be explained merely as functional aids to the smooth operation of a reified object of study called “society.” On the contrary, in an important sense such symbols are society, because they are constitutive of social action and thus of the regular patterns of behavior that comprise social institutions. In other words, what is objectively given to human experience and to historical research as external society is the byproduct of innumerable individual actions that have been motivated and constrained by an array of overlapping symbolizations of social reality. As a result, sociohistorical generalizations about any society, ancient or modern, must begin with an analysis of these organizing symbols.3

Furthermore, changes in social structure should not be conceptualized as the process by which the reified organism or system called “society” maintains its equilibrium or seeks a new state of equilibrium by adapting itself in response to external shocks. Nor is social change a matter of the internal dynamics of impersonal economic “forces” inexorably leading from one “mode of production” to the next. This is not to say that economic or material factors are unimportant, or that the emergence and reproduction of stable patterns of behavior is irrelevant to historical research; but sociohistorical changes in those patterns of behavior we call social institutions must be studied first of all in terms of the emergence of new understandings and organizing symbols that motivate and constrain individual human action. There is a striking difference, therefore, between Weber’s methodological individualism, which focuses on the subjective meanings of social action, and Durkheim’s methodological holism, which leads naturally to the positivist “biologism” or “organicism” of the systems paradigm.

1. The Patrimonial Household Model

The functionalist systems paradigm has been repeatedly critiqued on both philosophical and social-theoretical levels over the past century. During the past few decades, this critique has had an impact even in traditionally positivist Anglo-American social science.4 The functionalist paradigm is still influential, however, in many branches of ancient studies, including archaeology. This is no doubt a reflection of the fragmentary and ambiguous nature of the available data. The great practical advantage of functionalism is that it permits one to fill in many of the blanks through sheer deduction, if one assumes that a social group of a given size with a given level of technology in a given environment “must” have certain structural features on the basis of an inescapable functional-adaptationist logic. Furthermore, because certain features of modern society can be explained in functionalist terms, being themselves the products of a functionally oriented instrumentalist logic, it is tempting to use modern analogies (e.g., “bureaucracy” or the “market economy”) to interpret ancient evidence.

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3 In a well-known passage in Economy and Society, Weber (1978:341) observes that “groups that are not somehow economically determined are extremely rare. However, the degree of this influence varies widely and, above all, the economic determination of social action is ambiguous—contrary to the assumption of so-called historical materialism. Phenomena that must be treated as constants in economic analysis are very often compatible with significant structural variations—from a sociological viewpoint—among the groups that comprise them or coexist with them. . . . Even the assertion that social structures and the economy are ‘functionally’ related is a biased view, which cannot be justified as an historical generalization, if an unambiguous interdependence is assumed. For the forms of social action follow ‘laws of their own,’ as we shall see time and again, and even apart from this fact, they may in a given case always be co-determined by other than economic causes. However, at some point economic conditions tend to become causally important, and often decisive, for almost all social groups . . . conversely, the economy is usually also influenced by the autonomous structure of social action within which it exists. No significant generalization can be made as to when and how this will occur. However, we can generalize about the degree of elective affinity between concrete structures of social action and concrete forms of economic organization . . .”

4 For an overview of the philosophical critique of social-scientific functionalism, see Lessnoff 1974. See also Charles Taylor’s widely cited 1971 essay, “Interpretation and the Sciences of Man,” reprinted in Taylor 1985b. In anthropology, Clifford Geertz (1973) has been an influential proponent of “interpretive” approaches to the study of culture. For a systematic exposition of a nonfunctionalist, “hermeneutically informed” social theory, see Anthony Giddens, The Constitution of Society (1984), and also his essay “Hermeneutics and Social Theory” in Shapiro and Sica 1984. In what follows, I will take an approach similar to that of Giddens, whose key concept of dynamic “structuration” expresses the view that a social “structure” should not be thought of as a reified system, but rather as “rules and resources, recursively implicated in the reproduction of social systems. Structure exists only as memory traces, the organic basis of human knowledgeability, and as instantiated in action” (Giddens 1984:377). A social system itself is “the patterning of social relations across time-space, understood as reproduced practices. Social systems should be regarded as widely variable in terms of the degree of ‘systemness’ they display and rarely have the sort of internal unity which may be found in physical and biological systems” (ibid.). A rather different but equally nonfunctionalist theory has been developed by Habermas (1984).
But this practical advantage is nullified when doubts are raised concerning functionalist assumptions and the related use of modern analogies, as empirical research in history and anthropology gives us reason to do. In ancient Near Eastern studies, ethnographic and historical analogies have often been selected quite casually, or, what is worse, they remain unacknowledged, even though they are implicit in every aspect of historical reconstruction, from the broadest socioeconomic studies to detailed lexicography. To be sure, the authors of particularistic studies frequently note in passing the necessity of avoiding anachronistic analogies for specific social phenomena. But little attention is given to the problem of identifying, on a broader level of analysis, which of the familiar analogies or automatic preunderstandings of social phenomena that are current today lack validity when applied to the alien world of the ancient Near East.

For example, the formally “rationalized” (in Max Weber’s sense) form of government that we call “bureaucracy,” which is so widespread in the modern world, is usually assumed to be characteristic of complex urban societies wherever they are found—so much so that all governmental administration operating on a large scale is termed “bureaucratic.” Near East specialists purposely use this term rather loosely, of course, and it is not my intention simply to challenge the etymological adequacy of what many no doubt view merely as a convenient term for describing complex administration of any kind. But in what follows I do intend to challenge what I believe to be the faulty understanding of ancient Near Eastern society that lies behind the use of the modern term “bureaucracy” (for example) for social phenomena that, strictly speaking, were not bureaucratic at all.

Challenging faulty preunderstandings and choosing adequate analogies requires a new approach to sociological and historical perspective, which I have undertaken to provide in this work. In my opinion, given the interpretive problem we face, the conscious and explicitly defended choice of appropriate ethnographic analogies should form a major part of any attempt to describe an extinct society like that of ancient Canaan or Israel. Furthermore, in substantive terms, I am in basic agreement with Weber’s own view of ancient Near Eastern society, which is based on his ideal type of “patrimonialism,” or what I will call the “patrimonial household model” (PHM). Patrimonialism is the antithesis of rationalized bureaucracy. In a patrimonial regime, the entire social order is viewed as an extension of the ruler’s household—and ultimately of the god’s household. The social order consists of a hierarchy of subhouseholds linked by personal ties at each level between individual “masters” and “slaves” or “fathers” and “sons.” There is no global distinction between the “private” and “public” sectors of society because governmental administration is effected through personal relationships on the household model rather than through an impersonal bureaucracy. Likewise, there is no fundamental structural difference between the “urban” and “rural” components of society, because political authority and economic dependency are everywhere patterned according to the household model, so that the entire social order is vertically integrated through dyadic relationships that link the ruling elite in the sociocultural “center” to their subordinates in the “periphery.”

Patrimonialism as an ideal type is therefore the analytical distillation of the native symbol of the “house of the father”—that fertile “root metaphor” which I have singled out as having special importance in the ancient Near East, and in Canaan and Israel in particular. Weber applied the concept of patrimonialism very broadly, citing many different instances of it around the world, including the feudal regimes of medieval Europe, which he called “a marginal case of patrimonialism” (Weber 1978:1070). Indeed, almost all premodern states can be viewed as patrimonial, to some extent, because of the prevalence within them of personal ties of patronage and dependency in place of impersonal bureaucracy. Many of the best-attested examples, however, are found among the Islamic states of the Near East, some of which exemplify a subtype of patrimonialism that Weber called “pure” or “patriarchal patrimonialism” (ibid., p. 1107).

perhaps because relatively little was known about it at the time he was writing. Nevertheless, it is clear that he regarded the various states of the ancient Near East, including pharaonic Egypt, as patrimonial rather than bureaucratic (see, e.g., Weber 1978:1013, 1015, 1030, 1044–47; cf. Mark Lehner’s essay “The Fractal House of Pharaoh” in Kohler and Gumerman 2000).

6 For the special meaning given here to “center” and “periphery,” see Shils 1975 (cf. Eisenstadt 1993:iii–liiv). Note that these terms do not necessarily describe geographic zones, although they do often correspond to physical centers and their hinterlands. The “center” in question is not a particular place but rather “the center of the order of symbols, of values and beliefs, which govern the society” (Shils 1975:3).
Unlike Weber, I will restrict my definition of patrimonialism to this Near Eastern subtype rather than including European feudalism under the same rubric. As Reinhard Bendix (1977:382) has pointed out, Weber erred in linking feudalism with patrimonialism on the grounds that both types originate in patriarchal household government, because a feudal vassal was legally a free man and thus was not a member of his lord’s household. Feudalism and patrimonialism have different conceptual and symbolic underpinnings and should therefore be treated as separate types; hence patrimonialism, properly speaking, has a narrower range of application than Weber himself gave it. This is true chronologically as well as geographically, because Weber did not always distinguish the more purely patrimonial regimes of the pre-Hellenistic Near Eastern and Mediterranean world from the later Hellenistic and Roman empires, whose patrimonial elements he emphasized at the expense of their nonpatrimonial, bureaucratic features.

In recent decades, however, a chronologically more restricted application of pure patrimonialism has been espoused by the historical sociologist S. N. Eisenstadt (1971; 1979), who distinguishes the “archaic” patrimonial regimes of the pre-Hellenistic Near East from the more rationalized “patrimonial bureaucracies” that emerged later. This distinction is not evident in Eisenstadt’s 1963 treatise on The Political Systems of Empires, in which he included all of the ancient Near Eastern empires in the category of “historical bureaucratic empires,” without treating them in detail. In his earlier work, Eisenstadt was heavily influenced by structural-functionalism, as he later admitted (1993:xvi). Accordingly, he had adopted an oversimplified “top-down” approach, merely cataloguing the external features of various historical social groups without regard to the differing local understandings of social reality by which these groups were integrated. Eisenstadt’s later work is more Weberian, however. This can be seen in his appreciation of the special nature of what he calls “archaic” patrimonial regimes, which grows out of his recognition that their defining feature was a symbolic homology between the social center and periphery, a homology that most subsequent states and empires did not possess.

Following Karl Jaspers (1953), Eisenstadt uses the term “Axial Age” (Achsenzeit) for the period of the emergence of more rationalized regimes in the first millennium B.C. that were characterized, as he puts it, by the symbolic autonomy of the cultural center over against the periphery; that is, by “autonomous” as opposed to “embedded” elites (Eisenstadt 1979; 1986; cf. B. Schwartz 1975). Like Eisenstadt, I will limit the application of pure patrimonialism to archaic or “pre-Axial” societies in my use of Weber’s ideal type. Accordingly, I prefer to emphasize household membership as the defining feature of truly patrimonial regimes and to speak of the “patrimonial household model” (PHM). This usage conforms better to Weber’s own definition of patrimonialism, although he himself departed from it, to some extent, in his rather wide-ranging application of the concept.

The term “patrimonial household model,” as opposed to “patrimonialism” in general, thus serves to distinguish societies that exhibit the “household” understanding of the social order in a relatively pure form from more rationalized societies that retain patrimonial elements—in some cases to a high degree—but whose internal structure is based on a greater symbolic differentiation between center and periphery. This sort of “pure” patrimonialism should therefore be separated analytically from both feudalism and “patrimonial bureaucracy.” According to Eisenstadt, “pre-Axial” patrimonial regimes are characterized by a structural and symbolic homology between center and periphery, while the more rationalized “Axial” regimes that succeed them exhibit cultural centers that possess structural and symbolic autonomy over against their peripheries, even though patrimonial features may persist in them (see Machinist 1986).

To be sure, the emergence of a new kind of Near Eastern “world empire” on an unprecedented scale during the Neo-Assyrian, Neo-Babylonian, and Iran-based Achaemenid regimes may reflect the development of certain forms of practically rationalized administration within a patrimonial framework. But bureaucratic government is truly evident only later, in the Greek political institutions and related philosophical and constitutional theorizing that were carried throughout the Mediterranean area and the Near East during the Hellenistic period. It is true that subse-

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7 Weber himself recognized this difference between feudalism and patrimonialism (1978:1081), and often treated feudalism as a quite separate type because it had diverged so far from its (presumed) patrimonial origins. See the more detailed discussion of feudalism in chapter 9.1 below.

8 The concept of an “Axial Age” will be treated more fully below in chapter 5.2, after the particular meaning of the term “rational” in Weberian historical sociology has been discussed in chapter 4.3. Note that “more rationalized” here refers to an increase in “formal” rationality as opposed to “substantive” rationality. Earlier regimes were not simply “irrational.”
quent states and empires, including the Roman empire, retained many patrimonial features—sometimes to a high degree, as in medieval feudalism and certain Islamic regimes. But Greek rationalization left its mark on the social institutions and political thinking of the Near East and Europe, and it set the stage for the emergence of more fully bureaucratic states in modern times. Of course, patrimonial elements still crop up today in many nominally bureaucratic administrations, in the West and elsewhere, and they are seen, from a legal-rational point of view, as evidence of bribery or nepotism. The point to be made about the ancient Near East, however, is that we should not assume a priori that bureaucracy, in anything like the modern sense, was present at all, or that governmental administration was anywhere conducted on anything but a purely patrimonial basis.

I will treat the distinction between patrimonialism and bureaucracy in more detail in the next chapter, in the context of Weber’s general sociology of domination. It is worth stating at the outset, however, that the present work defends the validity of the patrimonial model as a description of the mode of socioeconomic organization in Bronze Age Syria and Palestine (i.e., “Canaan,” broadly defined), as this is currently known, in particular, from the textual and archaeological evidence found at the site of ancient Ugarit on the north Syrian coast. More generally, I defend the proposition that many other sociopolitical entities of the ancient Near East, in spite of their undeniable diversity, can also be understood in terms of this model. The alternative is to see the basis for administration and social integration either in a rationalized bureaucracy (“patrimonial” or not), or in a form of feudalism like that of medieval Europe, both of which I reject as suitable analogies for interpreting the data available from the ancient Near East.

The best historical and ethnographic analogies are found, rather, in the Islamic society of the premodern Middle East, although even here it must be said that patrimonialism often does not appear in a pure form but crops up within a more-or-less patrimonial bureaucracy (see Mottihaedeh 1980 and 1981 on early Islamic states in Iran and Iraq; also Findley 1980; 1989 on Ottoman society). This is because most of the Islamic states and empires were heirs of a long tradition of Hellenistic or Roman rule, and because Islam as a monotheistic faith itself promotes a rationalized and egalitarian approach to social life. Thus the patrimonial characteristics of premodern Islamic society, however prominent, were embedded in a more differentiated symbolic framework than had existed earlier in the region. Nevertheless, to the extent that they testify to a patrimonial understanding of social relationships, Islamic Near Eastern analogies promise to provide a more accurate picture of ancient Near Eastern society than do feudal or bureaucratic analogies derived from medieval and modern Europe.

This claim for a rather wide-ranging breadth of application of the PHM across the ancient Near East may elicit some skepticism. The phenomenon of patrimonialism and its appearance throughout the ancient Near East will be documented in some detail below in chapter 12, so I will simply note here that, at the broadest level of analysis, we may include the many diverse social groups known from the ancient Near East within a single type of social organization because of numerous lines of evidence which indicate that these societies were based on symbolically expressed ordering principles that were shared by all. Of course, it is necessary to take account of the variation occurring within this basic structure, and I attempt to do so as I place Bronze Age Canaan, and the kingdom of Ugarit, in particular, within the context provided by its Near Eastern neighbors and antecedents. Nevertheless, the recognition of the fundamental similarity between Ugarit and other ancient Near Eastern regimes permits us to apply insights derived from these external sources to the data from Ugarit, and vice versa.

2. Local Rules and Global Order

From a hermeneutical perspective, it is not as surprising as one might think that we can detect the same principles of social order in many different times and places, permitting us to describe the diverse social groups found within the vast temporal and spatial span of the ancient Near East in terms of a single, fairly simple, sociohistorical model. To understand why this is so, we must turn from the substance of Weber’s analysis of ancient Near Eastern society to his sociological method. It is impossible to overemphasize the point that, within the interpretive paradigm of historical sociology, the native understanding of the social order assumes central importance. If the native symbolization of social relationships is rather simple and undifferentiated, as in patrimonial regimes, then societies that are based on this symbolization will possess a simple underlying structure, even though there may be complex elaborations on
this common structure as individual persons and groups experiment with the different behavioral strategies and the different interpretations of the common symbol that are available within it. Moreover, if a certain understanding of society is widely shared over a broad geographic region and persists for a long time, then we can expect to observe structural similarities between social groups that are quite distant in time and space.

For the Bronze and Iron Age Near East, therefore, I argue that the basic structure of social interaction exhibits considerable simplicity and interregional uniformity, both of which are derived from the widely shared conception of society described by the PHM. From an external perspective, of course, social complexity and interregional variability did exist in the ancient Near East, but they resulted from the replication and adaptation of the simple household model in innumerable social situations, encompassing political and economic relationships at all levels. The familiar patriarchal household served as the universal paradigm for all social relationships, whether economic, political, or religious. This view of ancient Near Eastern society is in keeping with the interpretive method of sociohistorical analysis, which seeks analytical generalizations, not at the level of the superficial sociopolitical behaviors and institutions that can be catalogued according to external traits, but at the level of the ordering symbolizations that give form to the unpredictable flux of human social interaction.

Now, even though the “house of the father” was manifestly a central organizing symbol or “root metaphor” in ancient Near Eastern society, as I have argued, this symbol has its own history and contextual particularity, being interpreted somewhat differently in different contexts—and even interpreted in different ways by different persons within the same context. Furthermore, it was not the only symbol at work: other symbols of social order (e.g., the pastoral symbol of shepherd and sheep, or the military symbol of commander and troops) could on occasion cross-cut or color the “house of the father,” not to mention the varying degrees of emphasis in different times and places within the network of subsidiary symbols pertaining to household life and property (e.g., son, brother, wife, daughter; steward, client, slave; adoption and inheritance) which were engendered by the root metaphor of the father’s house. Nonetheless, the “house of the father” is plainly primary in most cases, in view of its well-documented ubiquity throughout the ancient Near East, which is attributable to its metaphorical fertility and broad applicability.

It is worth noting that the simplicity of the social model derived from this equally simple native symbol is not due to the historian’s illogical reduction of a more complex social reality, thanks to the arrogant naïveté of the outsider’s perspective. Indeed, the simplicity of this model abandons what too often amounts to the arrogant cynicism of the outsider’s perspective, which attempts to penetrate beneath the symbol to what is “really” going on, thereby failing to credit the power of a symbol whose very simplicity gives it the flexibility and extensibility to integrate disparate persons and groups within a durable social framework.

By emphasizing the native self-understanding of society in this way, I do not mean to deny the importance for sociohistorical analysis of the objective environmental or economic conditions of social life. Local variation in material conditions can lead to striking social differences, whether across regions or over time, and much of the diversity observable in the ancient Near East is no doubt the result of such variation. But it is questionable whether the scientific analysis of external causes can provide adequate explanations of social phenomena. Indeed, what can be observed from an external perspective in terms of material conditions and mute human behavior may have a quite limited bearing on the question of social organization and social change, in cases where the same conditions and behavior coincide with very different symbolizations of social order. The interpretive method consequently emphasizes subjective “ideal” factors, not to the exclusion of material factors, but under the assumption that shared conceptions of society are not secondary phenomena but are what ultimately constitutes social reality itself, being themselves experienced through language as something “objective” or given (for a particularly lucid discussion of society as both a subjective and an objective reality, based on Alfred Schutz’s phenomenological sociology, see Berger and Luckmann 1966).

Subjective conceptions of society therefore channel and constrain social behavior just as objective material conditions do—and even more powerfully, in many cases. It is true that these conceptions are themselves rooted in concrete lived experience or embodied practices under particular material conditions, but it must be remembered that the material conditions of social life (both environmental and economic) are themselves reciprocally affected by, or even created by, social action itself—action that is motivated by a particular symbolization of the world. Interpretive social theorists have therefore attempted to avoid the oversimplifications that result from reliance on either excessively materialist or excessively
idealist explanations of social phenomena, preferring to view social change instead as the product of an exceedingly complex and unpredictable dialectical interaction between material and ideal factors in the context of meaningful lifeworlds.

Interpretive sociohistorical treatments of ancient Near Eastern data are still quite rare, however. Not surprisingly, most of the explicitly theoretical work on the ancient Near East has been done by Marxist historians such as Igor Diakonoff and Mario Liverani, whose reductionist “two-sector” model, based on Marx’s concept of the “Asiatic mode of production,” will be discussed in some detail in Part Two of this book. Liverani, in particular, has written extensively on Ugarit and on Bronze Age Canaan as a whole. The functionalist “ecosystemic” approach to Bronze Age Canaan, on the other hand, is best represented in the work of the archaeologist William Dever. Dever explicitly rejects economic or environmental determinism, but this disclaimer is contradicted by his definition of culture as “a patterned adaptation to the environment” (1989:235). Although he would like to maintain a balance between material and ideal factors, Dever, like many other processual archaeologists, attempts to do so merely by inserting a “symbolic subsystem” (together with various ecological, demographic, and technological subsystems) into his overarching model of culture as an adaptive system.10

Like other processual archaeologists, Dever advocates the use of “general systems theory” to construct models that describe and also (it is hoped) explain the rise and collapse of complex societies.11 Accordingly, he has proposed ecosystemic models to account for social-structural changes in Early, Middle, and Late Bronze Age Syria and Palestine (Dever 1981; 1983; 1987b; 1989; 1992; 1995a). Note that the social systems and subsystems in view here tend toward homeostatic equilibrium, as in the older view of biological ecosystems, an equilibrium from which they depart only when they experience some kind of perturbation, either external in origin or because of an individual innovation or random “mutation” within the system. Such perturbations can be counteracted by “negative feedback,” which restores the system to its former state; or, under the right conditions, they can lead to “positive feedback,” which amplifies the deviation from the former state and produces an overall change in the system and, eventually, a new equilibrium. In this view, therefore, positive feedback accounts for systemic social changes that include, at the highest level, the rise and collapse of societies as a whole.

In spite of their acknowledgment of the effects of positive feedback, however, most processual archaeologists, operating under the influence of functionalist social theory, have tended to think of homeostatic equilibrium as a defining characteristic of social systems. But in recent years scientific work on nonlinear or “chaotic” systems in nature (as opposed to the linear systems which Durkheim and Parsons had in mind) has prompted a fundamental reconsideration of the nature of social systems. Research on nonlinear dynamical systems has shifted attention away from homeostasis via negative feedback to the unpredictable effects of positive feedback, including the intriguing phenomenon of “self-organization.” Rather than attempting to predict the extremely complex behavior of the total system, attention is focused on “local rules” of interaction, whose recursive operation can produce unpredictable patterns of emergent “global order.”

Here we have a striking convergence with the longstanding interpretive paradigm in social theory. Indeed, the intellectual links go as far back as the Romantic period, to the rejection of the reductionism of Newtonian science found in Goethe’s holistic approach to natural phenomena, which has influenced leading complexity researchers of the past few decades (see Gleick 1987:163–66; 197ff.).12

Syro-Palestinian archaeology is illustrated by the fact that the “human ecosystem” paradigm is adopted, in one form or another, by several of the contributors to The Archaeology of Society in the Holy Land (Levy 1995), which is intended to represent the current state of the discipline.

10 Other influential specialists on the ancient Near East who have employed an explicitly systemic-functionalist model of society include the archaeologist Robert McC. Adams (especially in his earlier work, e.g., Adams 1966; cf. Adams 1984) and the Assyriologist Giorgio Buccellati (1977; 1996), whose research encompasses both philology and archaeology. The functionalist approach that was so characteristic of the 1960s and 1970s (and continues in certain circles up to the present) may be contrasted with the more phenomenological approach of an earlier generation, exemplified by Henri Frankfort (1948; 1956). Among historians and archaeologists of ancient Canaan and Israel, the pioneering American scholar William F. Albright (1957) claimed to avoid reductionism, but he accepted the basic assumptions of Durkheimian functionalism and biologism (p. 118), and his own “organismic philosophy of history” remained strongly positivist and evolutionist (see Miles 1976; Long 1997; Schloen, in press). More recently, however, some scholars in the tradition of American biblical archaeology, such as Lawrence Stager (1985a; 1988), have adopted a much less functionalist sociohistorical approach.


12 Similarly, the antipositivist phenomenology of Husserl and Heidegger is echoed by the leading theorists of “auto-
Theorists do not conceptualize society as a reified global system whose components can be neatly defined and labeled by an outside observer according to externally observed functions. Instead, they view society as the unpredictable product of innumerable ongoing interactions among individual social actors who relate to one another in terms of “local rules” based on the meanings they typically attribute to the actions of others and to their own responses. This methodological individualism rests on the empirical observation that human beings do not experience society as an abstract system but as a sequence of concrete, subjectively meaningful social actions. Indeed, “society” itself does not exist apart from individual social experiences.

Sociohistorical generalization is not thereby rendered impossible, however, for individual social actions are not random but are constrained by intersubjectively shared meanings, and it is possible to construct “ideal types” or motivational models that summarize the meanings individuals typically impute to social behavior. This is precisely what Weber did in the areas of religion, law, government, and economy; and these analytical types (e.g., the patrimonial household model) form the basis for further investigation into the complex and inherently unpredictable interplay between symbolically mediated behavior and the factual conditions of social life. This method entails what the sociologist Anthony Giddens (1984:374) calls the “double hermeneutic” that always characterizes sociohistorical research; that is, “the intersection of two frames of meaning as a logically necessary part of social science: the meaningful social world as constituted by lay actors and the meta-languages invented by social scientists” (on Weber’s methodological individualism and the long tradition of “structural sociology” that it opposes, see Giddens 1984:207–26).

The agent-oriented interpretive method therefore avoids the danger of oversimplification inherent in functionalist and materialist approaches. It is worth repeating, however, that the interpretive method is not thereby ineluctably particularist and purely descriptive. Analytical generalizations are still possible—not, it is true, at the level of “society” as a whole, whose external features are too variable to anatomize, but at the level of shared understandings of social relationships. Moreover, these local shared meanings, which are open to analysis, can be shown through empirical historical study to have generated characteristic patterns of social action (“social structures”), even though the particular features of a given society at any one time are unpredictable in principle. Observable historical configurations of such patterns of action can be explained after the fact by showing how they are meaningfully related to one another in terms of their inner logical connections. This is what Weber means when he speaks of the nondeterministic but still retrodictively explicable “elective affinity” (Wahlverwandtschaft) that is discernible among certain ideal types, his most famous example being the relationship between the “Protestant ethic” and modern capitalism.13

Working at the level of subjective meanings and motivations, Weber developed a nonpositivist method of “singular causal analysis” of historical events in terms of ideal types and their affinities.14 This “exegetical” method entails the identification of typical or statistically probable (albeit nondeterministic) relationships between motives and actions in given contexts. As Fritz Ringer (1997:80) puts it, this mode of “causal interpretation in terms of motives” involves “trying to link alterations in the contexts of choice to changes in the courses of action pursued by typical agents.” Furthermore, with respect to Ricoeur’s concept of the narrativity of history, Weber’s method of singular causal analysis “is the explanatory procedure that accomplishes the transition between narrative causality [in terms of reasons] . . . and explanatory causality [in terms of laws]” (Ricoeur 1984:182). This is because “causal imputation is related at every stage to scientific explanation,” for scientific explanation is required both to analyze the objective contextual factors that affect the choice of a particular action, and to formulate general rules that summarize the ways in which human beings typically react in given situations. In other words, what in the preceding chapter I have called “prehistoric” scien-

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13 On the concept of “elective affinity,” see Kalberg 1994:102–47. As Kalberg notes, there are “logical interactions” or “analytic relationships” among empirical patterns of social action that Weber modeled in terms of either elective affinity or antagonism: “A compatible intermingling of two or more ideal types indicates the existence of an elective affinity model. A nondeterministic though typical and reciprocal interaction of regular action is hypothesized. . . . Each such interaction model involves an inner [logical] affinity between two or more separate ideal types. Antagonistic relationships, on the other hand, indicate hypotheses of ‘inadequacy’ and a clash, a hindering, and even an excluding of the patterned action-orientations implied by each ideal type” (Kalberg 1994:103).

tific explanation in terms of objective causal models is needed to permit the construction of a historical narrative in terms of subjective motivational models and their interconnections. As Ricoeur (1991:142) says, the aim is not “to place a case under a law but to interpose a law in a narrative, in an effort to set understanding in motion again.”

Weber’s agent-oriented approach to sociohistorical generalization is thus well illustrated by analogy with recent mathematical and scientific work on nonlinear dynamical systems, and on the subset of such nonlinear systems that have been called “complex adaptive systems.”

Over the past few decades, the characteristics of unstable or “chaotic” systems have been studied intensively, and it is now recognized that such systems can often be modeled quite simply in terms of a few “local rules” (i.e., local to the individual components of the system) even though the future state of the system as a whole is inherently unpredictable. This is because these systems are nonlinear; that is, very small changes in initial conditions will produce large changes in the future state of such a system because of the operation of positive feedback, which amplifies tiny localized perturbations (i.e., “mutations” or innovations) in such a way that the future state of the entire system quickly becomes unpredictable, even though the local behavior of its individual components is predictable and can be modeled with a few simple rules.

Although a few processual archaeologists have long since attempted to include positive feedback effects in their systemic models, they have done so at cross-purposes to their functionalist view of society.

That view was originally derived (as far back as Comte and Adam Smith) from linear or homeostatic systems in nature in which positive feedback is negligible and negative feedback operates to maintain a single equilibrium state in the face of external perturbations. It is true that systems with positive feedback need not remain permanently chaotic and may also return to a stable equilibrium, but this will be a new and different state of equilibrium; moreover, it is impossible to predict which new state will arise from among the multiple equilibrium-states that are possible in such a system.

The analogy of positive feedback in nonlinear systems therefore does not suit a view of society that stresses the functional value of social institutions for maintaining stability or for adapting to changes in the external environment, on the model of stable, linear systems in nature. For that matter, stable systems are relatively rare even in the physical world. Many natural phenomena are themselves inherently “chaotic” rather than homeostatic. A favorite example is the weather, which is a deterministic system, in the sense that it can be described in terms of a relatively simple set of local relationships or “rules” involving temperature, pressure, and humidity, but is impossible to forecast accurately more than a few days in advance. This gives rise to the so-called Butterfly Effect—that the flapping of a butterfly’s wings in Beijing, for example, can cause a hurricane in New York.

In spite of their unpredictability, however, a striking feature of nonlinear chaotic systems is the replication of certain “fractal” patterns that exhibit the same structure across all scales of observation, from the smallest to the largest, and persist through time even though the individual components that comprise them may change. This kind of self-similarity indicates that “order” is hidden in “chaos”; thus general statements can be made, after all, about otherwise unpredictable phenomena. Furthermore, in certain kinds of nonlinear systems, the algorithm or local rules that determine the interactions between the individual components of the system can produce not only chaos but also global order or self-organization.


16 Flannery (1968), for example, tried to take positive feedback into account within what remained, nonetheless, a functionalist model. His well-known hypothesis concerning the origins of Mesoamerican agriculture highlights the role of positive feedback in producing unintended (and unpredictable) social change. It has been pointed out, however, that systemic models (like Flannery’s) in which positive feedback is taken seriously are necessarily descriptive, not explanatory in a predictive sense, and consequently it is hard to see how they suit the processualist aim of producing causal explanations of culture change based on the assumption of functional adaptation to the environment. As Trigger (1989:308) observes in his discussion of archaeological applications of systems theory: “Archaeologists soon began to move further away from rather than nearer to a consen-
as an emergent property of the system itself, without plan or external intervention. These systems are called “complex adaptive systems” because their unique characteristic is that their components are not inanimate particles but adaptive agents which change their behavior in response to surrounding conditions. Examples of complex adaptive systems have been sought among living organisms at various levels, from cells to entire ecosystems, and the emergence of self-organization solely from individual interactions proceeding according to local rules has been offered as an explanation for phenomena such as biological evolution, in which many different organisms and species compete for resources, and human consciousness itself, which emerges from the local interactions of billions of neurons. It has also been argued that human society can be regarded as a complex adaptive system, and the emergence of various kinds of social order from the operation of local rules of social interaction has been investigated in recent years, especially by economists (e.g., Arthur 1990), the modern market economy being a prime example.

In light of this approach to complex systems, it is clear that the chief defect of the functionalist model does not lie in the notion that human society can be pictured as a system of interrelated components. The defect arises, rather, from two false assumptions: first, that the components of a social system must consist of a small, manageable number of reified “social structures” or “subsystems,” rather than consisting of the totality of individual social actors themselves; and, second, that social systems must be linear or homeostatic, and therefore behave predictably if their initial conditions and rules of interaction are known. Interpretive social theorists have long challenged both of these assumptions, arguing that social structures (i.e., stable patterns of meaningful social action) emerge from individual social interactions and the shared meanings attributed to them—meanings that are themselves generated and maintained in the process of interaction—and that social change is therefore historically contingent and unpredictable because individual actions and the subjective meanings attached to them are not entirely determined by other factors.

On the analogy of biological evolution, of which cultural evolutionists have been so fond, innovative types of social action and shared meaning (initiated, for example, by “charismatic” leaders) may be viewed as random “mutations” that produce internal perturbations in the system, and, when amplified by positive feedback, result in massive social change. Such changes can be understood in a nonfunctionalist sense, without assuming systemic homeostasis, because the scientific view of biological evolution and ecological balance as the deterministic products of adaptation to the environment has itself given way in recent years to a new appreciation of the historical contingency of evolutionary development and of the “chaos” inherent in ecological systems (see Pahl-Wostl 1995). The interpretive paradigm in social theory therefore agrees remarkably well with the new scientific understanding of complex systems in nature; and this is no accident if, as its proponents claim, modern complexity theory describes any system in which many individual agents interact according to a relatively simple set of local rules.

In terms of the patrimonial household model, in particular, it can be argued that, in the ancient Near East, familiar household relationships, born of personal ties of kinship and master-slave association, provided the local rules for all social interaction—rules that themselves emerged out of the social interactions generated within the smallest viable social unit, namely, the household. It is not surprising, then, that these local rules are expressed in narrative form as readily understood “programs of action” underlying the plots of traditional Canaanite myths and legends (see chapter 2.3 above and chapter 14). The application of patrimonial rules to social settings beyond the individual household served to integrate many disparate households into a social whole consisting of a “fractal” or recursive hierarchy of households within households, not through the imposition of an overall structure from above, but through the ongoing operation of a simple set of local rules for social interaction.

Thus global order, in the form of social complexity, emerges from local rules, as in other complex

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17 Note the difference between this view of a social group as a system consisting of individual persons and Niklas Luhmann’s (1995) neofunctionalist theory of social systems whose components are “communications,” not persons. Luhmann’s theory is based on Maturana and Varela’s biological theory of “autopoeisis” (self-reproduction) in living systems, which parallels in many respects what is elsewhere called complexity theory (see Mingers 1995). It is tempting to apply the notion of self-producing living systems to social systems that also seem to be characterized by recursive self-reproduction. Yet according to Luhmann, the components of a social system are not human beings as individual adaptive agents but rather communicative events, because these, and not social actors, are what is reproduced over time through the functioning of the system. Other autopoeisis theorists have criticized Luhmann, however, for his neglect of individual actions and intentions, without which there could be no communication in the first place (see Mingers 1995:148–42; cf. the philosophical critique of Luhmann’s approach in Habermas 1987: 368–85).
adaptive systems, and the household model that provides the local rules can be seen as a fractal pattern that is replicated on every scale throughout the whole system. Social collapse occurs when the system breaks down into its component parts (e.g., into smaller “households” such as tribes or chiefdoms) and lapses into “chaos”—itself an unpredictable event. But the analog of complex adaptive systems suggests that even in its most complex form a social system is far from experiencing static equilibrium. In such systems, ordered complexity is maximized on the “edge of chaos”—the zone of precarious balance between frozen homeostasis and random chaos—rather than in the highly ordered but “dead” state of complete equilibrium.

It is important to remember that, in a complex adaptive system, particular manifestations of global order (or chaos) cannot themselves be modeled, for they are states of an inherently unpredictable nonlinear system. What can be modeled, however, are the local rules that define the behavior of the individual components of the system; thus scientific generalization is possible, even though the overall characteristics of the system will change unpredictably through time. In terms of sociohistorical analysis, this is the exact counterpart of Weber’s insistence on developing detailed typologies of social phenomena based on the objectively shared subjective meanings of individual social actions, rather than on the external features of particular societies viewed as abstract wholes in functionalist fashion. A Weberian sociohistorical model, such as the PHM, can therefore be regarded as the local rules of a complex adaptive system; and this simple set of rules could have been common to the different societies of the ancient Near East, as I shall argue in detail below, even though global order manifested itself in different ways in each case. The mathematical and scientific metaphor of “system” thus remains useful in sociohistorical research, provided that the right kind of (nonlinear dynamical) systems are in view.

3. Complex Systems: Model or Metaphor?

A word of caution is in order at this point, however. In using the language of complex systems to describe the ideal type of patrimonialism, nothing has been added to interpretive social theory. Moreover, it is very difficult to give a rigorous demonstration of the validity of complexity theory for this kind of sociohistorical analysis because we lack the detailed, quantifiable data available to natural scientists, or even to social scientists (especially economists) who study present-day social behavior within narrowly defined domains. But there is a heuristic value in viewing ancient societies in terms of such systems, for much the same reason that processual archaeologists have been fond of systemic metaphors in the past. Furthermore, by borrowing in this way from the natural sciences I do not believe that I am guilty of a new kind of naïve scientism. Older systemic social models derived from the natural sciences were excessively deterministic and ahistorical because they selected unthinkingly from impressive and easily grasped examples of stable, linear systems, like the motion of the planets, for which historical contingency may be safely ignored. But many natural phenomena—not to mention social phenomena—are not produced by such systems, as the foregoing discussion has emphasized. Complexity theory therefore provides a unified perspective on complex adaptive systems wherever they are found, an approach that is as relevant for sociohistorical research as for natural science, because such systems possess the same formal properties regardless of the adaptive agents of which they are composed.

But there is an important caveat here for ancient studies, in particular. Without texts or other native expressions of the meaning of social action it is very difficult to apply Weberian motivational models, whether or not these are dressed up in the jargon of complexity theory as the “local rules” of social interaction. More fundamentally, from the point of view of philosophical hermeneutics, the objectifying formalism of a systems approach, as valuable as it may be within carefully circumscribed domains, can never provide an encompassing sociohistorical paradigm, as I have noted above in chapter 2. That this is true even with respect to complex nonlinear systems is demonstrated by the weaknesses of recent archaeological attempts at “agent-based modeling” by way of computer simulations of complex adaptive systems (see Epstein 1999; Kohler and Gumerman 2000). In such simulations, the local rules of interaction that determine the behavior of individual agents are usually specified on the basis of functionalist or materialist assumptions concerning what is “rational” (using the principle of “least effort,” for example), because no native informants or textual sources are available to divulge the subjective meaning of social action. But then the local rules are simply positivist fictions invented by the researcher, who is playing a computer game that may have little relation to historical reality as it was experienced by the human beings involved. Agent-based modeling thus has many of the same weaknesses as neo-Darwinian “selectionism” (see chapter 2.2), to the extent that it relies on dubious assumptions concerning the local
rules that determine the behavior of individual agents.\textsuperscript{18}

To compound the problem, there is usually no attempt in agent-based modeling to formalize the ways in which the local rules of interaction might themselves change reflexively as a result of the recursive operation of the system. Such changes may result in a situation in which different agents employ different rules of action at a given time, which increases the difficulty of performing an adequate simulation. Indeed, it is hard to see how one could ever formalize adequately what is involved in such diachronic changes and in the resulting synchronic intrasystemic variations in the local rules of social interaction. If one posited new rules at random or on the basis of external functional considerations, one would thereby ignore the inner logical connections that normally exist between successive organizing symbols, by virtue of the fact that they are subjectively meaningful to the social actors. On the other hand, if one specified the changes in local rules in advance on the basis of their presumed inner connections, what is the point of running the simulation? One would already have done all the modeling that could be done, remembering that only local rules are susceptible to modeling, not the unpredictable global configurations that they produce.\textsuperscript{19}

\textsuperscript{18} We can contrast the problem faced by prehistorians in this regard with our ability to understand local rules, and the symbols and rituals by which they are implemented, in more recent ethnographic situations in which native informants provide indispensable assistance. For example, Stephen Lansing’s (1991) study of traditional Balinese rice-irrigation practices shows in a striking way how a complex and quite efficient interregional irrigation system emerged in Bali through local interaction and communication among rice farmers, centered on the institution of the “water temple,” without the intervention of a centralized bureaucratic authority (I am grateful to Gil Stein for drawing my attention to this work).

\textsuperscript{19} Note, however, that the theoretical chemist Walter Fontana (1991) has formalized the “learning” of new rules of interaction by agents within a complex system in terms of systems of mathematical functions (represented as combinable “algorithmic strings”). Each such function is both an operator and an operand, and the interaction of these functions in a nonlinear dynamical system can result in an “algebraic extension” that produces new functions, i.e., new agents operating according to new local rules that conform to the existing rules but also extend them. Fontana points out that this mirrors the way in which molecules combine to produce specific new molecules in the course of chemical reactions, in such a way that “the formation of the product object is \textit{instructed} by the interacting reactants,” and is not merely the result of chance events, as in the case of genetic mutations (p. 408). Fontana’s nonrandom model of the generation of new rules is therefore similar in many ways to Weber’s notion of the elective affinity among ideal-typical patterns of social action, which interact and combine on the basis of inner logical connections in ways that produce new patterns of action (indeed, it is an interesting coincidence that Weber borrowed the term “elective affinity” from chemistry, by way of Goethe—see Howe 1978). Having said that, I do not see how Fontana’s approach would justify creating agent-based computer simulations of actual social systems of the past, because such systems are not amenable to predictive modeling but can only be studied retrospectively on the basis of historical data. In other words, the new rules generated by a computer simulation would have no necessary relevance to any particular historical case, and thus the simulation would add nothing to traditional historical methods.

Most damaging to this line of research, however, is the following consideration: even if abundant ethnohistorical evidence were available, permitting one to specify the local rules of subjectively meaningful social action in great detail, one’s computerized simulation of the workings of the resultant complex adaptive system would serve no purpose. Such a system is “complex” precisely because it is characterized by “sensitive dependence upon initial conditions”; thus the measurement error inherent in specifying the initial state of the system guarantees that the system will quickly diverge in an unpredictable manner from any representation of an actual historical starting point. The results may be intriguing as an experiment in alternative (albeit unreal) social worlds, but their relevance to actual historical phenomena will be uncertain. The relationship between local rules and global order in a nonlinear system can only be explained retrodictively; it cannot serve as a basis for prediction of any particular historical outcome. Despite the allure of a rigorously formal method of computerized analysis, we are thus thrown back upon the conventional sociohistorical method of Weber and his successors, in which logical connections are traced retrodictively among humanly meaningful ideal types—motivational models that represent the symbolically mediated local rules of action by which social actors interact with one another and with material conditions in particular historical settings.

In my view, therefore, complex systems provide an interesting heuristic metaphor that is of some value in conceptualizing sociohistorical phenomena, but detailed computer-based models of such systems have little practical research value in historical studies, either for prediction or for simulation. (In a similar vein, the status of complex adaptive systems as merely useful metaphors versus models of actual natural phenomena has been debated in scientific circles; see Cowan et al. 1994.) Ricoeur’s argument for the limited role of systemic models in historical
research still holds (see chapter 2.2). The notion of a social system necessarily implies the more basic notion of the human power to act in an intentional manner. As a result, all systemic models, whether linear or nonlinear, are ultimately at the service of a human narrative understanding that cannot be simulated by any kind of “artificial intelligence,” because such understanding depends on embodied existence, as Heidegger has shown. What even the most complex systemic models fail to capture are the meaningful inner connections among sequential patterns of motivated action, for these historically and culturally contingent inner connections cannot themselves be modeled formally and abstractly in a disembodied way, but can only be “exegected” after the fact by hermeneutical means, in terms of what Weber calls elective affinities among ideal types.  

If we return now to William Dever’s ecosystemic models of Bronze Age Canaan, in particular, it is clear that merely inserting a “symbolic subsystem” into such a model does not lessen its reductionism. By speaking of social reality in this way, processual archaeologists have plainly not abandoned the external orientation of the functionalist paradigm. The symbolic subsystem is treated as an external structure or pattern of behavior that is visible in the form of “art, ideology, and religion” (Dever 1989:235; cf. Renfrew and Bahn 1996:457). Individual social actors’ subjective understandings of the social order are therefore reduced to being only one component of the total system. But native understandings of social relationships cannot be treated as merely one externally observable feature among others, for shared symbolizations uniquely define social behavior, circumscribing the types of behavioral strategies available to individual agents much more narrowly than does the environment, technology, or demography of a social group.

Furthermore, complexity theory suggests that the actual components of a social system (if we wish to use that metaphor) are not the hypostatized patterns of behavior that we label “structures” or “subsystems,” but are the individual social actors themselves, whose complex interaction generates those patterns of behavior. The symbolic, ecological, technological, and demographic subsystems proposed by processual archaeologists are thus misleading reifications of secondary patterns of social behavior, for these patterns are created by the meaningfully defined, symbolically mediated interaction of social actors with one another and with their environment and do not operate independently of the individual agents who comprise the fundamental components of any social system.

But this shift toward a more agent-oriented perspective also serves to highlight the basic limitation in the very notion of a “social system,” however it be conceived. On the one hand, it is true that we can salvage the system metaphor by appealing to complex adaptive systems, which help us to see that symbols or shared meanings are central to human society because they uniquely define the local rules of interaction among individual adaptive agents. On the other hand, the actual behavior of such systems cannot be predicted or simulated but can only be interpreted retrodictively in terms of (in the case of human society) intersubjectively meaningful motivations, and this calls upon the faculty of understanding.

20 On the limitations of computer simulations of intentional human action, see Hubert Dreyfus’s (1992) Heideggerian philosophical critique of attempts to develop generalized artificial intelligence (AI). The same fundamental criticism applies to computer simulations of social phenomena as applies to rule-based AI, for both are based on what Dreyfus (p. x) calls “an old rationalist dream,” “the Cartesian idea that all understanding consists in forming and using appropriate symbolic representations. . . . Given the nature of computers as possible formal symbol processors, AI turned this rationalist vision into a research program and took up the search for the primitives and formal rules that captured everyday knowledge. . . . As it turned out, though, it was much harder than anyone expected to formulate, let alone formalize, the required theory of common sense.” The problem is not solved by the introduction of “neural network” or “connectionist” computer models, although current AI research using such techniques is much closer in spirit to the phenomenological approach that Dreyfus advocates, since neural networks emulate the situation-specific “reinforcement learning” that seems to characterize the human brain. The fact remains that “in all applications of reinforcement learning the programmer must use his or her knowledge of the problem to formulate a rule that specifies the immediate reinforcement received at each step. For path problems and games the objective nature of the problem dictates the rule. If, however, the problem involves human coping, there is no simple objective answer as to what constitutes immediate reinforcement. Even if we assume the simplistic view that human beings behave so as to maximize their total sense of satisfaction, a reinforcement-learning approach to producing such behavior would require a rule for determining the immediate satisfaction derived from each possible action in each possible situation. But human beings do not have or need any such rule. Our needs, desires, and emotions provide us directly with a sense of the appropriateness of our behavior. If these needs, desires, and emotions in turn depend on the abilities and vulnerabilities of a biological body socialized into a culture, even reinforcement-learning devices still have a very long way to go” (Dreyfus 1992:xlv). For a general hermeneutical critique of mechanistic reductionism in the cogni-
possessed by the embodied human agent who is writing or reading a historical account—a mode of understanding that cannot be replaced by an objectifying systemic model, but encompasses and envelops any such model. In the end, the metaphor of complex nonlinear systems, which is increasingly popular among archaeologists, not only fails to add anything new to interpretive social theory, but despite its heuristic value, it may in fact be misleading if it is used, like the older natural-science metaphor of linear homeostatic systems, to sustain a nonhermeneutical objectivist approach to past social action.
Chapter 4. The Patrimonial Ethic and the Spirit of Antiquity

In promoting the virtues of the interpretive method of sociohistorical analysis, I do not mean to derogate the important insights achieved by scholars working with materialist or functionalist models of society in ancient Near Eastern studies and elsewhere. But I believe that the textual and archaeological data from ancient Canaan and Israel and from other ancient Near Eastern societies can best be explained in terms of Weberian typologies of the meaning of social action; that is, in terms of analytical generalizations which proceed from the interpretive method’s emphasis on individual understandings of social behavior rather than on the external anatomy of a society viewed from above as an abstract whole. With this in mind, it is clear that the patrimonial household model (PHM) itself is not a model of this or that “society,” seen as a reified entity, as if the historical particularity of different social groups could be reduced to a simple scheme. Rather, it is a model of the ordering principles that underlie certain recurrent social phenomena, or stable patterns of action, which may assume quite different forms in different times and places.

It is true that, through elective affinity, such principles of social order often produce strikingly similar social institutions wherever they are found; thus we may speak loosely of the “patrimonial state” as a type of society, even though patrimonialism, strictly speaking, refers only to a type of understanding of social relationships, which in turn tends to generate characteristic types of social behavior (compare the notion of a “basin of attraction” in complexity theory; see Gleick 1987:233ff.). But the occasional adoption of a global perspective should not prevent us from recognizing, from the agent-oriented perspective of the interpretive method, the fundamental structural similarity (in terms of ordering principles or “local rules”) among superficially diverse social groups of various sizes existing at different levels of social complexity. A “top-down” view of ancient Near Eastern society might tempt us to categorize various groups as “tribes,” or “chiefdoms,” or “states,” as if their differing sizes and technological abilities proved that the underlying social structure at each level of size and complexity were somehow necessarily different. This sort of functionalist approach implies that the problem for social history is to explain the linear processes of structural transformation that led from one qualitatively different type of society to another. But the radical implication of the interpretive method, and of the PHM in particular (radical in terms of the sociohistorical interpretation of ancient Near Eastern evidence as it is usually practiced), is that the same principles of social order may be found among social groups at all points along the continuum from political decentralization to centralization. The concepts of “urbanization” and “tribalization” are therefore devoid of meaning in the ancient Near Eastern context if they are taken to refer to anything more than different historical states of a complex adaptive system in which the local rules of social interaction remain constant.

This is not to say that the social differences that these terms describe are illusory, or that the explanation of the change from simple to complex societies is no longer a central problem; however, according to the interpretive method, sociohistorical analysis of the ancient Near East should not proceed by defining types of “society” based on external features and then attempting to bridge the historical gaps between the occurrences of these types. It should proceed by analyzing the interaction of ideal and material factors—in the context of local economic exchange and long-distance trade relations, for example—that generate changes in social complexity (i.e., the rise and fall of “urbanized states”) among groups whose basic principles of social interaction may remain unchanged over long periods. Furthermore, sociohistorical models should attempt to describe these ordering principles rather than the fluctuating, historically contingent products of the social interactions that they produce.

It is an empirical fact, of course, that the ordering principles of society, the local rules of social interaction, have themselves changed over time. They, too, are subject to historical contingency because they do not exist in a timeless realm of ideas but are generated and maintained in the process of social interaction among individual human beings, who live in changing material conditions and constantly attempt to make sense of their world. In the language of complexity theory, the individual agents who make up the system are adaptive, not simply because the existing set of rules that they follow permits different responses to different conditions, but because they can learn to change their rules of behavior in the course of interaction with one another and with their environment.
My application of the “pure” patrimonial model will accordingly be restricted to the period before the seventh century B.C. because of the undeniable evidence that, in the course of the first millennium, especially after the formation of the Neo-Assyrian empire, Near Eastern and Mediterranean societies experienced a fundamental shift in human conceptions of social order. This shift is evident in the religious and philosophical literature of the period, especially that of ancient Greece and Israel; but it was also reflected in and dialectically influenced by changing material conditions, in the form of new economic relationships (including a monetary economy) and the physical reorganization of social interaction that we can detect archaeologically in changing settlement patterns and in new forms of architecture and urban plan.

This new understanding of the social order was characterized by what Weber called the “rationalization” of social life, a phenomenon that was ultimately rooted in a new awareness of the gulf between the transcendent and mundane spheres of reality. The older patrimonial pattern did not disappear completely; indeed, it is found in the region to this day. The thoroughgoing patrimonialism of the earlier period was no longer present, however, and it is necessary to introduce the ideal type of “bureaucracy” (or the hybrid “patrimonial bureaucracy”) in analyzing the Mediterranean and Near Eastern societies of the late first millennium B.C. and thereafter.

I will discuss the rationalization of patrimonial society in more detail below, with particular reference to the work of S. N. Eisenstadt. In introducing my methodology and the scope of this study, however, it has been necessary to draw attention to the persistence of the PHM in the Near East for thousands of years, and to outline the relationship between a sociohistorical model such as this and observable patterns of social behavior. Important social changes accompanied the transition from patrimonialism to a more rationalized conception of the social order, and modern scholars stand on the other side of that divide, with the result that patrimonialism is not an intuitive concept or preunderstanding that we might naturally apply in our historical reconstructions of the ancient Near East. But philosophical hermeneutics and the related interpretive method in sociohistorical research require that special attention be paid to the native terminology of political relationship and to native mythological symbolizations of the social world, because social actors’ understanding of the social order is reflected more-or-less directly therein. For the Bronze and Iron Age Near East, these types of evidence point in the direction of the patrimonial model. Other evidence is provided by administrative documents and material remains (especially domestic architecture, the concrete locus of the individual household), which reveal external patterns of social behavior and economic activity that have been constrained by a particular understanding of the social order. All of these types of data will be adduced below to defend the proposition that the Late Bronze Age kingdom of Ugarit, in particular, is best understood in terms of Weber’s concept of patrimonialism.

For the Bronze Age Near East, in general, the PHM has important implications for textual and archaeological interpretation because it calls into question a number of widely held assumptions about social and economic organization. It predicts a lack of distinction in political and economic administration between “public” and “private” sectors, corresponding to the absence of any notion of rationalized bureaucracy, abstract constitutionalism, or the impersonal state. It expects law to exhibit “substantive” rather than “formal” rationality (to use Weber’s terms), and to consist of the application of traditional precedents or the unquestioned decisions of traditionally legitimated rulers rather than a formally rationalized legal code. It sees no structural or conceptual difference between governmental fiscal systems (“taxation”) and smaller-scale reciprocal and redistributive economic exchanges that are rooted in personal social relationships. It rejects any kind of structural or symbolic dichotomy between “urban” and “rural” spheres, which are not horizontally stratified but are vertically integrated with one another by means of dyadic personal ties of authority and subordination. It denies structural and symbolic autonomy to the ruling elite, whose status is ascribed to them by virtue of their positions within traditional social networks and whose authority is structurally similar to that of subordinate patriarchal masters of every rank, with the result that the basis of social solidarity and integration is “mechanical” or segmentary rather than “organic” (to use Durkheim’s terms). It does not expect depersonalized market exchange to be the primary engine of economic production and distribution, which are characterized instead by reciprocal and redistributive transfers of goods—social actions that are embedded in traditional social relationships. Finally, it sees no fundamental structural difference between simpler and more complex forms of society, be they “tribes” or “chiefdoms” or “states.” Textually, the patrimonial understanding of society and the organization of socioeconomic relationships according to this conception should be manifested in administrative, legal, epistolary, and mythological documents. Archaeologically, the patrimonial structure of
society should be visible in patterns of urban residence and economic activity that are congruent with those found in more rural settings, indicating the vertical integration of center and periphery by means of a hierarchy of households linked by personal relationships on the household model.

All of these predictions and correlates of the PHM can be tested in light of available textual and archaeological data. But it must be emphasized again that such data do not speak for themselves. Competing “feudal” and “bureaucratic” interpretations of the evidence from Ugarit and from elsewhere in the ancient Near East have proliferated over the years, with little systematic discussion of their basis in theory and in fact. The existence of these antithetical reconstructions shows that the data are always interpreted in light of some model, explicit or implicit, which is applied to ancient Near Eastern evidence. Moreover, such models ultimately reflect a broader theory of human society. The virtue of theoretical reflection in this context, therefore, is to call attention to the dangers of relying on inadequate sociological assumptions and anachronistic historical analogies, and to underscore the necessity of refining one’s models in light of comparative sociohistorical research.

1. Traditional and Rational Legitimations of Authority

The patrimonial household model that I am proposing for the Bronze Age Near East pictures a society that was at once more centralized and less centralized than what adherents of bureaucratic models have envisioned. In a patrimonial regime the ruler stands at the apex of an integrated socioeconomic system that encompasses all of the land and personnel in his domain. Everyone is ultimately a member of the ruler’s household—there is no “free” sector of independent proprietors who enjoy a separate conceptual and legal status from that of “nonfree” palace dependents. Viewed from the top down, such a society appears highly centralized and the ruler is theoretically omnipotent. But the other essential element of this model is hierarchy, a feature it shares with the feudal model that has also been applied to the Bronze Age Near East (see chapter 9.1 below concerning the differences between the patrimonial model and the feudal model). The effective power of the ruler is luted by his need to exercise authority through subordinates (and their subordinates), whose “household” domains are smaller in scale but similar in structure to his own. As a result, all kinds of private economic activity and jockeying for political and social advantage can take place beyond the ruler’s direct supervision. What looks at first glance like an all-encompassing royal household reveals itself, when viewed from another angle, to be a complex and decentralized hierarchy of households nested one within another and held together by dyadic “vertical” ties between the many different masters and servants who are found at each level of the hierarchy. Instead of a few “horizontal” social classes or a simple dichotomy between “free” and “nonfree” persons (i.e., “villagers” versus “palace dependents”), the patrimonial model envisions a more complex and dynamic social order—one that is open to individual initiative within the context of changing personal relationships between masters and servants, but is also highly structured because it makes use of a simple set of concepts derived from everyday life and applied to many different situations.

Thus, unlike the Marxist “two-sector model”—a version of the bureaucratic model that many scholars have applied to the Bronze Age Near East in recent years (see chapter 9.2)—in which there is very little integration between the “state” or “slave” sector and the “free” sector (Diakonoff 1982), the household concept provides an integrating principle that binds together all members of the society and serves to legitimate authority at all levels. Royal authority is certainly the most inclusive form of such authority, but it is structurally the same as the patriarchal authority exercised by the master of the humblest household. In contrast, an integrating principle is lacking in the two-sector model, which offers no explanation of how the king’s rule was legitimated within the free sector.

The issue of legitimacy also distinguishes the PHM from the feudal model. In chapter 9.1 below I will note certain similarities between these two approaches, but the key difference between them lies in the patrimonial model’s special concern for the particular cognitive mode by which power was legitimated in ancient Near Eastern polities. In other words, the patrimonial model takes account of the native understanding of social reality in a way that an alien model derived from medieval European feudalism cannot. Of course, if by “feudalism” we mean relationships of domination, whereas the patrimonial model is constructed in terms of personal, household-type relationships.

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1 It is true that administrative hierarchy is also a feature of bureaucratic models, but this is understood in terms of a rationalized system in which “public” and “private” affairs are distinguished and officials enjoy well-defined spheres of competence. As I shall argue below, bureaucratic analogies are even less appropriate than feudal analogies for the Bronze Age Near East. Bureaucracy involves impersonal
simply “manorialism,” then we need not be distracted by the particular ideological component of fully developed European feudalism. But in that case, the term “feudalism” is misleading and we should turn instead to Max Weber’s carefully defined concept of patrimonialism, which is based on comparative study of numerous premodern states that were characterized by dependent land tenure of the manorial type.

At this point, then, it is worth introducing Weber’s ideal type in some detail, together with its theoretical underpinnings in what he called the “sociology of domination.” Weber began by observing that all stable political arrangements depend on the perceived legitimacy of the ruler’s exercise of power. With respect to the subject of this book, this observation prompts a fundamental question: How was power legitimated in the Bronze Age Near East? This question has not received the attention it deserves in studies of the ancient Near East and of the kingdom of Ugarit, in particular (which will serve as a primary case study in Part Two of this book). Another way of putting it is to ask how persons of various ranks viewed the relationships of domination in which they took part, and why they acquiesced to them. I will attempt to answer this question in terms of the PHM, but first it is necessary to explain why so much emphasis is placed here on the concept of legitimacy.

As I have noted above, historians and sociologists working in the interpretive tradition have long attempted to understand sociohistorical change as the result of complex interactions among both ideal and material factors, rejecting one-sided approaches that emphasize one kind of factor to the exclusion of the other. This sociohistorical paradigm is based on the belief, buttressed by empirical observation, that cultural symbols and the social institutions that embody them cannot be reduced to mere epiphenomena that are derivative of deeper material causes rooted in environmental conditions or in the structure of economic production in a society. Such symbols are indeed rooted in concrete lived experience, but, as expressed in language, they mediate and constrain social action in an autonomous manner that cannot be reduced to the status of an effect of underlying material causes.

Given this approach to society, the concept of legitimate domination assumes special significance in Weber’s work, in contrast to sociohistorical writings of an orthodox Marxist orientation (as seen, for example, in the two-sector model of ancient Near Eastern society), in which phenomena such as the belief in a ruler’s legitimacy are not treated as independent causal factors but are regarded as part of the ideological “superstructure” of a society. In classical historical materialism, only the economic or material substructure is ultimately relevant for sociohistorical analysis. In reaction to Marx and Engels, therefore, Weber argued that in analyzing social phenomena one must look beyond external features of social organization and seek the meaning that actors attach to social relationships and social behavior. More generally, one must avoid monocausal materialist explanations that ignore historical contingency, and one should reject the resultant oversimplified schematizations of human history that posit a unilinear sequence of universal stages in human social evolution.

Useful sociohistorical generalizations are still possible, but they will not concern inevitable historical processes. Instead, they will consist of careful typologies of the meaning of social relationships. Such ideal types can be distilled from comparative historical study, remembering that they do not aim to provide full descriptions of particular historical cases (in the language of complexity theory, they describe the “local rules” of social interaction, not the “global order” such rules might produce). These analytical models may then be applied in various combinations to particular cases in the hope of improving our understanding of specific historical developments over variously defined spans of time and space, both large and small.

It is as a basis for constructing a typology of political relationships, in particular, that the concept of legitimacy assumes prime importance in Weber’s discussion of the “sociology of domination” in his great treatise on Economy and Society (1978:941–55 [orig. German 1922]; see Bendix 1977:290–97). For Weber, the simple “power” (Macht) to impose one’s will is too varied and amorphous a phenomenon to be used as an analytical concept. In its place he defined “domination” or “rulership” (Herrschaft) as power that depends on a recognized legitimacy of authority and a corresponding measure of voluntary compliance (p. 53f.). From his wide ranging historical studies Weber then distilled three ideal types of legitimate rulership (p. 215): (1) legal-rational, “resting on a belief in the legality of enacted rules and the right of those elevated to authority under such rules to issue commands”; (2) traditional, “resting on an established belief in the sanctity of immemorial traditions and the legitimacy of those exercising authority under them”; and (3) charismatic, “resting on devotion to the exceptional sanctity, heroism or exemplary character of an individual person, and of the normative patterns or order revealed or ordained by him.”

In its purest form legal-rational domination is exhibited in the “bureaucratic” regimes characteristic of modern Western states (pp. 217–26, 956–1005; cf.
Bureaucracy operates within a system of abstract rules in which persons in authority are subject to an impersonal order and enjoy specified spheres of competence in a hierarchical organization composed of bureaucratic offices, for which officeholders are qualified by specialized training. Obedience is owed not to officials as individuals but to the impersonal order that they represent. Moreover, bureaucratic officials are personally free and assume their positions by free contract; thus their public affairs and even their places of work are kept separate from their private lives. Ideally, these officials discharge their duties impersonally and dispassionately.

Traditional domination is in many ways the antithesis of legal-rational domination. As Weber puts it: “Obedience is owed not to enacted rules but to the person who occupies a position of authority by tradition or who has been chosen for it by the traditional master” (p. 227; in general see Weber 1978:226–41; 1006–69; and Bendix 1977:329–84). The ruler’s administrative staff consists of personal retainers who do not have clearly defined spheres of competence in a rationally constructed hierarchy and who do not distinguish between their “public” and “private” affairs. The commands of those in authority (and “law” itself) derive from a mixture of traditional norms and personal discretion; moreover, the master’s discretionary power of command is not bound by any formally rational principle but is limited only by existing tradition. In its simplest form, traditional domination is found in the patriarchal household. Here the belief in authority stems from the commensal relationship, “the close and permanent living together of all dependents of the household” (p. 1007). Patriarchal domination is experienced as a natural personal relationship that is based not only on biological kinship but also on the patriarch’s actual power over his entire household, including his wives, children, servants, and animals.

More generally, Weber defined “patrimonialism” as the most common political expression of traditional domination and he considered it to be characteristic, in one form or another, of many premodern polities. In the “patrimonial state,” the ruler “organizes his political power over extrapatrimonial areas and political subjects . . . just like the exercise of his patriarchal power” (p. 1013). The household model is replicated throughout the social hierarchy because each political subject is himself a patriarchal ruler whose domain differs in scale but not in structure from that of his master. Political and economic relations at all levels therefore reflect the pattern that is basic to the whole social order: patriarchal domination in the household.

Patrimonialism is similar to feudalism in some respects, but these are two distinct types, although Weber regarded both of them as forms of traditional domination (pp. 1070–1110). In the course of his discussion of Weber’s sociology of domination, Bendix summarizes the difference:

Patrimonial government is an extension of the ruler’s household in which the relation between the ruler and his officials remains on the basis of paternal authority and filial dependence. Feudal government replaces the paternal relationship by a contractually fixed fealty on the basis of knightly militarism. [1977:360]

Unlike Weber, Bendix emphasizes the fact that feudalism “always involves a contract between free men” (p. 361); thus, the vassal “does not become a personal dependent like the patrimonial retainer” (cf. Bloch 1961:450ff.). It is true that Weber somewhat inconsistently derived both feudalism and patrimonialism from the household model involving traditional domination, but patriarchal household government is not essential to feudalism, as Bendix points out. European feudalism was much more impersonal and legal-rational than true patrimonialism, even though feudal fealty was expressed in personal terms. Whatever the similarities between feudalism and patrimonialism in their use of land grants and nonbureaucratic personal loyalty, their conceptual underpinnings are quite unlike. Furthermore, patrimonialism is found in many different times and places while feudalism, in the strict sense, is found only in medieval Western Europe and perhaps also (independently and somewhat later) in Japan.

Charismatic domination, although extremely important as a source of social change, differs from legal-rational and traditional domination in that it cannot be sustained. According to Weber (1978 [1922]: 241–54), charismatic domination can establish new norms but it is unstable and is soon “routinized” into one of the other types of domination.2 For example, a charismatic leader may found a dynasty whose rule is subsequently legitimated on traditional grounds, or charisma might become institutionalized within a legal-rational system and attach itself to the current holder of a bureaucratic office, as in the Roman Catholic church. Weberian models of stable, ongoing political relationships will therefore usually be

2 Edward Shils (1975) suggests a modification of this view in his essay “Charisma, Order, and Status,” arguing that charisma is a distinct and perduring quality of both traditional and legal-rational authority because it is rooted in respect for power, whatever its origin.
framed in terms of either traditional or legal-rational domination, or some combination of the two; that is, in terms of patrimonialism or bureaucracy.

Over the years various elements of Weber’s typology of domination have been fruitfully applied to many different societies. This is true of ancient Israel, for example, to which Weber himself devoted a separate study. His *Ancient Judaism* (1952) has generated considerable discussion and controversy. Indeed, Weberian ideas about the Israelite tribal confederacy, about covenant as a social institution, and about the role of charismatic domination have become deeply rooted in the literature of biblical scholarship. Strangely, however, Weber’s concepts of traditional domination and patrimonialism have been underused by biblical scholars and by students of the ancient Near East in general. This mirrors a wider neglect of the patrimonial model among historians and sociologists, who have often reduced Weber’s tripartite typology of domination to the dichotomy of bureaucracy and charisma (see G. Roth 1968:194).

In recent decades, however, there has been a revival of interest in patrimonialism among scholars studying various non-Western societies. Variations on the patrimonial model have been applied to post-colonial states in Africa and Asia (G. Roth 1968; Eisenstadt 1973), to precolonial Southeast Asia (Tambiah 1985), colonial Java (Bakker 1988), India (Jacobs 1989; cf. Rösel 1986), to the early Islamic empires in Iran and Iraq (Mottahedeh 1980; 1981), and to the late Ottoman empire (Findley 1980; 1989).

Among ancient Near East specialists, Jean-Pierre Grégoire (1976) and Johannes Renger (1990) have been the most explicit in their use of Weber’s concept of patrimonialism, which they have applied to Mesopotamia and Syria in the third and second millennia B.C. (see chapter 12.2–4 below). In his widely cited article on “The Archaeology of the Family in Ancient Israel,” Lawrance Stager (1985a:25) described biblical Israel as a “patrimonial state,” citing Weber’s definition of patrimonialism. More generally, in the last few decades a new appreciation has emerged of the importance of patron-client relationships and quasi-kinship structures in complex societies, both ancient and modern (e.g., Wolf 1966a; Fox 1971; Eisenstadt and Roniger 1980; 1984).

Carter Findley’s (1989) study of Ottoman administration is particularly noteworthy for students of the ancient Near East, because he shows that it was quite feasible for a large and complex Middle Eastern state to be organized according to a basically patrimonial, as opposed to bureaucratic, political model. Findley observes that:

> The patrimonial household . . . was the basic structural unit—with variations in different settings—for all of Ottoman society. At the lowest level, it characterized the extended families and other kinship networks in which the lives of individuals were submerged. At a higher level, this model structured the household-centered factionalism that characterized political life in the ruling class. At the highest level, this model provided the means, in metaphorically extended form, for integrating the entire state into a single “household.” [Findley 1989:46]

Findley is especially interested in the transition within the later Ottoman empire from an originally quite patrimonial regime to a more rationalized system under Western influence. Patrimonial and bu-

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3 See Hahn 1966:159–73; Petersen 1979; and Rodd 1979 for reviews of the literature. Several studies evaluating various aspects of Weber’s treatment of Israel and Judaism are presented in Schluchter (ed.) 1981. Christa Schäfer-Lichtenberger (1983; 1996) discusses at length Weber’s view of the ancient city and the “oathbound confederacy” as ideal types. Overall, Weber’s work on Israel and Judaism has not been immune from criticism (see, e.g., Talmon 1987), especially on account of his errors of fact and his occasionally fanciful exegesis. Many of his specific conclusions in *Ancient Judaism* rely on outdated anthropological or biblical scholarship. There is a general appreciation, however, of his stimulating influence on the study of Israelite history (although biblical scholars have sometimes misunderstood or selectively applied his terms and concepts).

4 Weber himself makes little explicit use of the ideal type of the “patrimonial state” in *Ancient Judaism* and, in fact, he sees in Judaism one of the wellspringss of Western rationalization. But it is clear from his general statements in *Economy and Society* (1978:1013–20) that preexilic Israel, at least, along with most other ancient Near Eastern states, falls within the patrimonial category. Indeed, Weber refers in passing to David as a patrimonial ruler (Weber 1978:1018). In addition, there are scattered comments in *Ancient Judaism* that indicate Weber’s view of preexilic Israel as a fairly simple and nonbureaucratic patrimonial state (see, e.g., Weber 1952:254, 259).

5 See also the detailed study by Jane Hathaway of *The Politics of Households in Ottoman Egypt* (1997), in which she shows how a disparate group of Anatolian and Balkan soldiers in a Janissary corps came to dominate Egypt in the eighteenth century by means of a household-based system of patronage. As she says, “The household was, in fact, the fundamental assimilative structure of Ottoman elite society. . . . As applied to Egypt’s military echelons, the household can transcend the historiographical tension between beylicate and soldiery, mamlik and freeborn, by including all these elements in a common organizational strategy. For the military household in its various incarnations, from barracks gang to fully developed residential complex, was precisely that: a strategy designed to enable a group with common interests to succeed in the never-ending competition for revenues, influence, and imperial favor” (p. 167).
The Patrimonial Ethic and the Spirit of Antiquity

S. N. Eisenstadt (1979; 1986) has explored the sociological ramifications of the emergence of more rationalized conceptions of society in the first millennium B.C. (Note that “more rationalized” refers here to an increase in “formal” rationality as opposed to “substantive” rationality—see the discussion of types of rationality below; it is not a matter of a totally “irrational” patrimonial society.) This rationalization is related to a new awareness of the tension between the transcendent and the mundane aspects of reality, an awareness of what Weber called “the problem of salvation” (see chapter 5.2 below). Like Weber, Eisenstadt regards ancient Near Eastern societies, which predate this trend toward rationalization, as patrimonial rather than bureaucratic. He points out that a feature of such societies is the presence of “embedded” as opposed to “autonomous” elites, and a resultant symbolic homology between the sociocultural center and its periphery. As Eisenstadt puts it:

The centers that tend to develop in such “patrimonial” societies were ecologically and organizationally but not symbolically distinct from the periphery, and these centers crystallized around elites which were embedded in various types of ascriptive units, even if often broad and restructured ones. [Eisenstadt 1986: 21]

The reason for this “congruence” between center and periphery is that social order in patrimonial societies is not predicated on an appeal to abstract transcendent principles (i.e., legal-rational legitimation) but rests on traditional legitimation, which is usually expressed in terms of kinship, even though local kinship relationships must be metaphorically expanded and reconstructed to encompass larger groups and territories. The ruling elite remains embedded, therefore, in an ascriptive social framework. In other words, all those who exercise authority do so by virtue of their roles within preexisting networks of traditional personal relationships, and not because they occupy offices in a constitutionally ordered bureaucracy that reflects and enforces an ideal vision of society. Eisenstadt (1986:12) notes that social integration in patrimonial societies is accordingly based on “mechanical solidarity” rather than “organic solidarity” (to use Durkheim’s terms). Society is not held together by the “organic” economic interdependence of different households, each engaged in specialized labor, but by “mechanical” or automatic bonds of attachment resulting from the similarity of lifestyle and behavior among people who practice a minimal division of labor.

6 More generally, the sociologist Bryan Turner, in his study of Weber and Islam (1974), accepts Weber’s view of the patrimonial character of traditional Islamic society. Turner emphasizes the similarity, as he perceives it, between Weber’s patrimonial model and Marx’s concept of the Asiatic mode of production (pp. 78f., 173f.). It is true that both models have been applied to the same “Asiatic” social phenomena, but Turner neglects the fundamental opposition between Weber’s approach, which is based on the analysis of legitimacy (i.e., subjective understandings of social relationships), and Marx’s focus on the external features of purely economic relationships. In a more recent study of Weber, however, Turner (writing with Robert Holton) has applauded the “post-Marxist” revival of liberal social thought and the need for a positive appreciation of Weber’s continuing relevance to current political, economic, and social issues. Indeed, Holton and Turner have now “found it necessary to defend Weber against many artificial and misguided criticisms from Marxism and Marxist sociology. . . . These interests have emerged both from our sense of the exhaustion and collapse of the intellectual and moral credentials of Marxism and state socialism, and from the interesting revival of interest in liberalism and libertarianism” (Holton and Turner 1989:11f.).

reauratic elements coexisted in the Ottoman empire, as in most of the other relatively recent historical situations to which the concept of patrimonialism has been applied. Roy Mottahedeh (1980; 1981), for example, characterizes the governments of the early Islamic empires in Iran and Iraq as bureaucracies with strongly patrimonial elements in certain periods. As Weber himself insisted, it will often be the case that the society under consideration must be modeled by some combination of the ideal types that he defined. As I have argued above, however, the PHM applies quite directly, without modification or admixture, to all known sociopolitical groups of the third and second millennia B.C. in the Near East. This is possible, in spite of the undoubted diversity among groups so widely separated in time and space, because the PHM permits great variation within a single basic structure. Different patrimonial regimes may have been more or less centralized, with deeper or shallower hierarchies and varying degrees of effective control on the part of their rulers, and this alone gave rise to striking differences. But their underlying similarity was due to the fact that they all had the same conceptual and symbolic basis, which entailed traditional, as opposed to legal-rational, domination. As I argue in chapter 12, the available evidence suggests that the PHM provides a faithful depiction of the types of social relationships that existed in the Near East before the emergence of a new kind of rationalized social order in the first millennium B.C.
In terms of the PHM, center and periphery are homologous because both are structured, symbolically and organizationally, around the “household,” which is not only a self-sufficient economic unit but also provides the template for social interaction at all levels. Furthermore, in the absence of a socially effective trend of rationalization that produces autonomous elites, the quantitative enlargement of a social group does not entail a qualitative transformation from a personalized kin-based society to an impersonal “state,” as functionalist researchers assume. This functionalist view was for a long time a fixture of historical and anthropological thought, but it is fallacious because it rests on the comparison of small-scale “tribal” societies of recent times to larger scale (and conceptually quite different) bureaucratic societies, rather than comparing them to the large-scale patrimonial regimes of the Bronze Age Near East.

If patrimonial regimes are headed by embedded elites, how are we to understand the role of kinship in ancient Near Eastern society? In recent decades a number of researchers have observed—in opposition to the view held by an earlier generation of anthropologists and historians—that kinship does not disappear as an effective force with the advent of complex urban society. It is now widely recognized that kinship networks have remained important in the Near East up to the present day, not only in rural villages but also in urban neighborhoods, where patterns of residence and of economic cooperation reflect extended-family and “clan” ties (real or fictional).

The view persists, however, that kin-based social organization in the ancient Near East was, at some level, antithetical to state administration (see, e.g., Yoffee 1993:69ff.). The state is assumed to have been organized on different principles (i.e., impersonal “bureaucracy”), and so either bypassed kinship altogether or, at best, co-opted those whose local authority was ascribed to them by virtue of their position within a network of traditional kinship relationships. But if Bronze Age Near Eastern society was fundamentally patrimonial rather than bureaucratic, as I suggest (following Weber and Eisenstadt), then traditional “kinship” relationships alone provided the organizing principles of the entire society. To be sure, in the process of the emergence of urbanized states, the numerical and territorial enlargement of the social group would have necessitated the reformulation of original kinship relationships that are suitable only for small-scale society. But important elements of the kinship model remain—indeed, this is the symbolic basis of the household model of society that is characteristic of patrimonial regimes.

In other words, ancient Near Eastern kinship is not a stubborn foreign body encapsulated and controlled within a quite different political structure. Instead, the metaphorical extension of kinship itself provides the administrative structure of the patrimonial state. Evidence for the “persistence of kinship” in urban society therefore does not indicate the anachronistic survival of archaic practices; it reflects the adaptation of the still-vital traditional mode of legitimation, so transparently displayed in small-scale societies, to meet the needs of more complex social groups. As Eisenstadt puts it:

Within congruent [symbolically homologous] societies . . . new cultural-political frameworks were based on the reformulation of the preexisting—kin and territorial—criteria. Such reformulation took place above all through extension of kin units into a combination of kin-territorial entities, based on more diversified and even more encompassing subunits still designated, nevertheless, almost entirely in such newly reconstructed kin terms. [Eisenstadt 1986:20]

The foregoing discussion implies that “kinship” should be subsumed within what I have been calling

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8 Anthropologists now emphasize the degree to which “kinship” is not a rigid external reality but is pragmatically constructed to meet current needs (see, e.g. Eickelman 1989:126–78 [on Middle Eastern kinship in general]; cf. Fried 1975 [on “tribes” as the invention of the state]). Henry Rosenfeld (1983), for example, argues that the kinship structures of the Near East (ancient and modern) are not vestiges of primordial prestate tribalism, as is usually thought, but directly reflect the imposition of particular forms of state administration upon rural cultivators—a process in which the government co-opts the leading members of local kin groups and uses them to control their dependents. Using examples from Ottoman Palestine, he explains how these kin-group leaders became an urban elite with dependent kin in the villages, and he suggests that similar relationships existed in ancient times. Rosenfeld provides an illuminating picture of how kinship is constructed to meet current needs; however, his structural opposition between “state” and “kinship,” although it may be valid for recent times, has not been demonstrated for the ancient Near East. Moreover, his argument that kinship is a vehicle of domination, masking class inequalities, fails to recognize the positive value for the “lower classes” of their “vertical” kinship ties to patrons among the elite; and it creates a false opposition between the reconstructed kinship of urbanized states and the mythical “egalitarian” kinship structures of prestate society.
“household” relationships, of which patrimonial policies are constituted. This is because a realization of the lack of opposition—indeed, the identity—between so-called kinship and state modes of social organization in the ancient Near East prompts the adoption of another term that encompasses both, namely, Weber’s “patrimonial household.” As I am using it here, the latter term includes kinship relationships; however, the word “household” is preferable to “kinship” because it encompasses master-servant relationships as well as blood ties, and therefore expresses more accurately the composition of the patriarchal domain. Moreover, broader political and economic relationships can in many cases be construed more easily in household terms, as opposed to narrow kinship. In a patrimonial regime, subordinates are either “sons” or “servants” of the person in authority, superiors are “fathers” or “masters,” and social equals are “brothers.” This household-derived terminology is ubiquitous in ancient Near Eastern texts, where the word “house” itself (Sumerian ḫ, Akkadian bittu(m), West Semitic *baytu, Egyptian pr, etc.) is used to describe social groups of widely varying degrees of size and complexity. Furthermore, the extension of the social group through time—be it a “lineage” or a “tribe” or a “state”—is often expressed as the endurance of an eponymous ancestor’s household. This is reflected in reverence for deceased ancestors, who were thought to have some kind of ongoing participation in the life of their household (a point I develop further in chapter 13.4).

It is important to note that the reconstruction of kin-cum-household relationships in a large-scale patrimonial society—with its proliferation of fictive, as opposed to actual, blood relationships, and its extensive use of master-servant ties—does not entail a qualitative break with the mode of social organization in simpler societies; it simply continues a process already at work within smaller “tribal” groups. As many ethnographers have noted, actual blood relationships, in the Middle East and elsewhere, is less important than the substantive relationships of friendship or dependence that exist among persons who construe their closeness in terms of kinship. On the broader levels of corporate social action (the “clan” or “tribe”), in particular, the facts of genealogy are beside the point—common descent is assumed to exist among those who cooperate.\footnote{To emphasize the degree to which Middle Eastern kinship relationships are not rigid but are creatively constructed, Abner Cohen (1965; 1970) has coined the term “patronymic association” (in place of terms like “lineage” or “clan”) to describe groups whose members claim descent from a common ancestor. Very often the genealogical links cannot be demonstrated, but informants believe that the close personal relationships that arise among neighbors must originate in actual kinship. The patronymic association of households that constitutes a residential neighborhood is often organized around a leading household, whose patriarch acts as the patron of the poorer households nearby. For a discussion of Middle Eastern “practical kinship,” with references to the anthropological literature, see Eickelman 1989:151–78.}

The kin/household idiom is extremely powerful in traditional patrimonial societies. It compactly expresses diverse social relationships that, in legal-rational society, are described by a much more differentiated terminology. Accordingly, it can be argued that ancient Near Eastern social groups, both large and small, were based on traditionally legitimated domination expressed in the language of the household. Rather than dichotomizing “tribe” and “state,” therefore, we should recognize their underlying conceptual unity within an overarching patrimonial social order. The rise and collapse of complex society in the ancient Near East thus appears, not as a series of qualitative structural transformations, but as an oscillation between decentralized and centralized forms of the patrimonial mode of social organization, whose extreme poles are represented historically by small-scale “tribes” and “chiefdoms,” on the one hand, and by political behemoths such as Ur III Mesopotamia and pharaonic Egypt, on the other. In all cases, as Eisenstadt has argued, the ruling elites were not autonomous but were embedded within traditionally ascribed social roles; thus the social center and its periphery remained structurally and symbolically homologous.

The old opposition between “egalitarian” tribes and “stratified” states is therefore misleading for the
ancient Near East, because all social groups were organized on the same patrimonial principle. Lloyd Fallers (1973:3–29) argues that we should abandon the term “social stratification” in favor of the more culturally neutral term “inequality.” An exclusive focus on “objective” or material inequalities leads to externally defined “classes” or horizontal social “strata,” which are presumed to possess social solidarity; but attention to the subjective meaning of social inequality for the social actors themselves often reveals the importance of dyadic “vertical” relationships between masters and servants or patrons and clients.

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12 Southall (1988:74ff.) himself adopts a Marxist perspective, arguing that his segmentary-state model describes a stage intermediate between the communal-kinship mode of production and the Asiatic mode of production with its bureaucratic state sector.
must first distinguish between traditional domination and legal-rational domination; that is, between patrimonial (or segmentary) polities of any size and degree of centralization, over against the symbolically quite different bureaucratic polities that first emerged during the first millennium B.C.

3. Types of Rationality and Processes of Rationalization

Closely related to Weber’s concept of legitimate domination or authority (Herrschaft) is his concept of rationality. Indeed, Stephen Kalberg (1980:1145) notes that “rationality and its diverse manifestations in historical rationalization processes have been universally acknowledged as a major, perhaps the major, theme in Max Weber’s corpus.” But as Kalberg also points out, few researchers have fully appreciated “Weber’s vision of a multiplicity of rationalization processes that variously conflict and coalesce with one another at all societal and civilizational levels” (ibid.). This barrier to comprehension arises because Weber himself nowhere provides a systematic exposition of his typology of rationality. Still, as Kalberg shows, it is possible to discern the careful conceptual distinctions concerning rationality and rationalization that underlie Weber’s own comparative-historical work on legal and economic phenomena and on the distinctive ethos of each of the major world religions (see also the essays in Lash and Whimster 1987, which treat Weber’s concept of rationalization from various perspectives).

Kalberg relates Weber’s well-known and explicit typology of social action to the typology of rationality that is implicit in his work. Weber (1978:24ff.) identifies four basic types of social action, two rational and two nonrational, which are defined in terms of subjectively meaningful motivations for action, as one might expect in a verstehende sociology. These four ideal types of action include: (1) “means-end” or instrumentally rational (zweckrational) action, which is “determined by expectations as to the behavior of objects in the environment and of other human beings; these expectations are used as the ‘conditions’ or ‘means’ for the attainment of the actor’s own rationally pursued and calculated ends”; (2) value-rational (wertrational) action, which is “determined by a conscious belief in the value, for its own sake, of some ethical, aesthetic, religious, or other form of behavior, independently of its prospects of success”; (3) affectual or purely emotional action, which borders on nonmeaningful automatic behavior; and (4) traditional action, which is “determined by ingrained habituation” and thus also borders on automatic, nonmeaningful behavior, or, if the attachment to habitual modes of action is upheld self-consciously, merges with value-rational action.

Empirical historical and ethnographic evidence indicates that each of these four basic types of social action, including the two rational types, is within the capacity of human beings in all times and places. Hence it is not a question for Weber of a historically acquired rationality of the Western world that may be contrasted with “primitive” irrationality elsewhere. Weber quite consciously attempts to avoid a crude ethnocentrism here, while recognizing that by conceptualizing ideal types of social action we can identify different historical configurations of the various types of action in terms of their relative preponderance among social groups in various periods and regions. It is true that Weber’s own research focused on the distinctive features of modern Western civilization, in relation to the striking predominance therein of instrumentally rational action, in particular. But he by no means asserted that this type of action is restricted to the modern West, or that it is somehow superior to other types of action in an absolute sense (for evidence of this in Weber’s corpus, see the citations in Kalberg 1980:1148ff.).

Kalberg notes that the two types of rational action (instrumentally rational and value-rational), themselves reflect distinct types of rationality, either singly or in combination. Weber cites four types of rationality here and there throughout his voluminous writings, namely, theoretical, practical, formal, and substantive rationality. Theoretical rationality differs from the other three types in that it is not directly related to social action. It is motivated instead by a more transcendent need to understand the world as a meaningful cosmos, and thus entails abstract cognitive processes by which magicians, priests, theologians, philosophers, and (in the modern world) scientists construct conceptual or symbolic systems. Theoretical rationality is sociologically significant, however, because under certain circumstances purely cognitive rationalizations can have a profound effect on everyday social action (e.g., the Hindu doctrine of karma or the Calvinist belief in predestination).

In contrast to theoretical rationality, practical and formal rationality are both directly associated with social action in the form of instrumentally rational action, which involves the calculation of appropriate means to achieve a given end. The difference between practical and formal rationality is that practical rationality operates “in reference to pragmatic self-interest, [whereas] formal rationality ultimately legitimizes a similar means-end rational calculation by reference back to universally applied rules, laws,
with the difficulties they present and calculates the most expedient means of dealing of daily life in behalf of an absolute value system, a for example, actively manipulate the given routines involved. “Instead of implying patterns of action that, for example, actively manipulate the given routines of daily life in behalf of an absolute value system, a practical rational way of life accepts given realities and calculates the most expedient means of dealing with the difficulties they present” (ibid., p. 1151f.). This is not to say that egocentric practical rationality is absent from bureaucratic forms of administration, but it is not their dominant or defining feature.

In traditional modes of domination, furthermore, there will also be a strong element of substantive rationality. Substantive rationality is oriented to substantive values rather than to a formal or practical means-end calculation. Thus it gives rise to value-rational, as opposed to instrumentally rational, social action. Value-rational action is based in a belief in the unquestionable value of some kind of behavior, purely for its own sake (e.g., obedience to patriarchal authority). Substantive rationality thus “orders action into patterns not on the basis of a purely means-end calculation of solutions to routine problems but in relation to a past, present, or potential ‘value postulate’ . . . [which] implies entire clusters of values that vary in comprehensiveness, internal consistency, and content” (ibid., p. 1155).

Historical research shows that human value postulates are highly variable, which raises the question of whether values themselves can be considered “rational” or “irrational.” For Weber, as Kalberg says, no absolute array of “rational” values exists as a set of perennial “standards” for “the rational” and for rationalization processes. Instead, a radical perspectivism prevails in which the existence of a rationalization process depends on an individual’s implied or stated, unconscious or conscious, preference for certain ultimate values and the systematization of his or her action to conform to these values. These values acquire “rationality” merely from their status as consistent value postulates. [Kalberg 1980:1155]

This is an extremely important point, for it means that “rationality” and any process of rationalization are ultimately defined from the perspective of a given set of substantive values—including the value placed on impersonal formal rationality itself in the modern world.13 For example, in describing bureaucratic forms of government as “more rational” than patrimonial regimes, as Weber and Eisenstadt do, it must be remembered that rationalization is here understood specifically as an increase in formal rationality. But formal rationality is perceived as more rational than substantive or practical rationality only from a certain historical perspective, namely, from the point of view of the modern West, in which formal rationality is deeply embedded in a cluster of atomistic egalitarian and democratic values. Hence we can acknowledge the extent to which we belong to this Western tradition—a tradition that points us toward a certain research program and definition of rationalization—without claiming that our own values are the absolute yardstick by which rationality as such is to be measured.

Thus it is not ethnocentric to note that “constellations of historical and sociological factors determine . . . whether a particular type of rationality in fact found clear expression as a mental process alone or also as regularities of action that became established as sociocultural processes, whether at the level of groups, organizations, societies, or civilizations as a whole” (Kalberg 1980:1160). In other words, regardless of whose substantive scale of value is used to measure rationalization, it is still an empirical question and not a matter of inescapable instrumentally-fundamentalist logic as to when and where formal rationality (defined by Weber in a value-free manner) became socially effective on a wide scale, to the point where we may speak of a fundamental structural change in patterns of social organization. In this

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13 Nietzsche’s influence is evident here; note the comparable perspectivism inherent in post-Heideggerian philosophical hermeneutics, especially as elaborated in Gadamer’s hermeneutics of tradition. It is worth noting also in light of this perspectivism that Weber’s famous endorsement of a “value-free” social science should not be understood in a sociologically positivist sense (i.e., in terms of an objectivist “neutral” scientism that implicitly absolutizes the value of instrumental reason), but in the “legal positivist” sense that the historically conditioned values which any researcher will necessarily hold are not themselves subject to formally rational scientific demonstration.

14 As Kalberg (1980:1160) notes, all four types of rationality are seen by Weber as fundamentally alike in the sense that they all involve mental processes by which human beings everywhere attempt to make sense of their world, or “consciously strive to master reality”; that is, “to banish particularized perceptions by ordering them into comprehensible and ‘meaningful’ regularities.”
book I attempt to chart the history of the ancient Near Eastern “house of the father” as fact and symbol in terms of an archaeologically and textually documented process of rationalization. In so doing, I have employed a Weberian analytical construct drawn from the native symbol of the “house of the father,” a construct I have called the “patrimonial household model.” It should be noted, however, that the kind of rationalization I am concerned with here involves the increasing influence of formal rationality at the expense of substantive rationality in certain important domains, as manifested by the emergence of patterns of instrumentally rational social action in political, legal, and economic spheres. In the political sphere, in particular, this rationalization is evident in the shift from strongly patrimonial regimes based on a traditional and substantively rational legitimation of authority to more bureaucratic regimes characterized to some extent by legal-rational legitimation.

The question is under what conditions formal rationality becomes socially effective on a wide scale and also endures over the long term. Although I have characterized the societies of the Bronze Age Near East as generally patrimonial rather than bureaucratic, it is likely that there were occasional episodes of nascent rationalization sponsored by political elites (e.g., Ur III Mesopotamia and late Middle Kingdom Egypt) that did not last long, with a subsequent reversion to the “archaic” patrimonial pattern. In light of the oscillations between political centralization and decentralization in pharaonic Egypt, for example, when the central government was reconstituted after each “intermediate period” from an enduring local pattern of disparate patrimonial households of various sizes, it may be that royal attempts to rationalize administration (perhaps originally motivated by purely practical concerns), which thereby undercut traditional social networks, actually destabilized the regime to the point that central political authority collapsed and was only resurrected on the basis of a patrimonial reamalgamation of households under the next great household or “dynasty” that was successful in reunifying the country (see Lehner 2000).15

In this case, as elsewhere, the interaction of various types of rationality and social action is exceedingly complex, such that the objectifying analytical procedure I am employing can never hope to render an exhaustive account of historical reality, which is amenable to analysis from a variety of historical and methodological perspectives. Nonetheless, I believe that the evidence from ancient Canaan and Israel, in particular, can be illuminated with the aid of Weber’s ideal types and their elective affinities. Moreover, within the interpretive sociohistorical paradigm it is possible to capture much of the complexity of historical processes of rationalization. This is demonstrated by Weber’s subtle notion that rationalization does not occur in a unilinear or comprehensive fashion, but affects different spheres of life in different ways. As Kalberg puts it:

Weber does not employ the concepts of “rationality” and “rationalization” in a global manner to refer merely to a general unfolding of civilizations. Instead, qualitatively different rationalization processes that potentially advance at their own indigenous rates take place at various sociocultural levels and in different life-spheres, both in those relating to the “external organization of the world,” such as the realms of law, politics, economics, domination (Herrschaft), and knowledge, and in the “internal” spheres of religion and ethics. [Kalberg 1980:1150]

For the ancient Near East, in particular, it is important to recognize that even though socially effective manifestations of formal rationality appeared and endured in political and economic spheres during the first millennium B.C.E., this was not a simple dispossession of substantive rationality and of value-rational social action as significant phenomena. Rather, new and powerful forms of substantive rationality took root at the religious and cosmological level, interacting in a dialectical fashion with the process of formal rationalization in government and economy. Indeed, Weber argues that any socially effective process of formal rationalization is ultimately dependent upon a parallel theoretical rationalization of the prevailing substantive rationality, in terms of the systematization of the values to which this substantive rationality is oriented.

This is so because, as Kalberg emphasizes, the four types of rationality, although they all “master reality” by making sense of the world, are not all capable of generating what Weber calls “methodical rational ways of life,” in which socially significant modes of formal rationality and instrumentally rational action may emerge:

Only action oriented to substantive rationality has the potential to introduce methodical ways of life . . . This development occurs most effectively after the values of a given substantive rationality of delimited magnitude have been rationalized, through theoretical
rationalization processes, into internally unified value constellations that comprehensively address and order all aspects of life. . . . Most important for Weber in the introduction of methodical rational ways of life is the fact that only substantive rationalities place “psychological premiums” on ethical action in the world. [ibid., p. 1165]

Only substantive rationality possesses the analytical potential to master—or rationalize—reality comprehensively. It does so by consciously and methodically organizing action into patterns that are consistent with explicit value constellations. [ibid., p. 1169]

The implication of this is that the Axial Age of the first millennium B.C. marks a new historical epoch in the Near East because it was only then that a socially effective theoretical rationalization of substantive values occurred in conjunction with formal rationalization, and not because attempts at sociopolitical rationalization were not made or could not have been made many times beforehand—as if formal rationalization were impossible until some inevitable evolutionary “stage” had been reached. Thus formal rationality became an important ingredient in a more-or-less stable and enduring sociopolitical order for the first time in the Near East within the hybrid “patrimonial bureaucracies” ushered in by the Axial Age, when a particular, historically contingent combination of symbolic and material factors arose that permitted this. In earlier periods, it seems that rationalization was highly destabilizing (contrary to modern perceptions) because it involved a delicate dance between the maintenance of the traditional “embedded” social relationships that underpinned royal authority, on the one hand, and various rationalizing innovations that served to disembody persons from these relationships, on the other (especially in military or other administrative contexts, e.g., when men and women were recruited and organized for service by age and other personal characteristics rather than by clan or family). The result was a precarious balance that was easily disrupted because it produced centrifugal tendencies that led to political decentralization and a return to the patrimonial pattern.

Formal rationalization, which is characterized by the increasing importance of universal rules applied without regard to persons, depends on a comprehensive and unified worldview that provides the transcendent principles by which individual and social life may be methodically ordered. But such a worldview takes shape only with respect to an ordered set of values which, by a purely intellectual process of abstract theoretical rationalization, have “come to stand in a relation of consistency not only to one another but also hierarchically under an ultimate value” (ibid., p. 1166); for example, the universal sovereignty of the one God of Israel. The question is what stimulates this sort of systematizing of substantive values, carried on by priests, prophets, philosophers, and other intellectuals—and, more importantly, what makes it “stick.” In other words, what inspires and sustains the methodical action patterns of the social groups who are the “carriers” of rationalization, and who ensure that it will be socially effective on a broad scale?

Weber himself wrote detailed studies of the major world religions, including Judaism, in order to trace just these interactions among formal, substantive, and theoretical rationality, noting their impact on economic and political patterns of action. It is important to remember, however, that specific historical outcomes are, for Weber, ultimately “irrational” in the sense of being the unpredictable consequences of chance configurations of factors. Particular historical patterns of action may be interpreted or “exeged” retrodictively in terms of motivational models and their meaningful internal connections, but history as a whole cannot be encompassed within a single predictable framework, for human history is not “subject to a transcendent meaning, the inexorable dialectical advance of ‘Reason,’ evolutionary laws, or even the centrality of the economic sphere as a general rule” (ibid., p. 1172). This is why it is necessary to speak of multiple rationalization processes occurring on many different levels and interacting in unpredictable ways, rather than imagining a single inexorable process of progressive rationalization leading from antiquity to the present, as did Hegel, Marx, and many others since the Enlightenment.

4. Substantive Rationality in Ancient Law and Economy

Weber’s typology of rationality has implications for empirical historical research in many different areas. With respect to premodern law, in particular, Bendix (1977:398) notes that: “Patriarchal and theocratic powers are primarily interested in substantive rationality. They approach all legal questions from the viewpoint of political expediency [practical rationality] or substantive [value-rational] justice and hence disregard any limitations on their actions that might arise from requirements of formal procedure or logical consistency” (cf. Weber 1978:641ff.; on Weber’s sociology of law in general, see Kronman 1983). In the ancient Near East, for example, the famous laws of Hammurapi and other Bronze Age law collections exhibit substantive rationality as opposed to formal rationality. Substantively rational law may well be generalized or standardized as normative “case law”
throughout a social group, but unlike formally rational law, it has not yet been systematized in terms of logical consistency or formal procedure.

Moreover, legal systematization of this sort is a relatively late occurrence—Roman law (especially in its medieval and later incarnations) is the earliest known example in the Mediterranean and Near Eastern region. Indeed, Weber argues that fully developed formal legal rationality is peculiar to the continental European law of recent centuries, whose gapless and logically coherent character is derived from the tendencies of Roman and canon law. This is in contrast to the lack of formal rationality of the English common law tradition, which retains many formally irrational elements (e.g., its reliance on case law and jury trials). Weber points out, however, that extralegal factors related to the social position and apprentice-style training of legal professionals in England paradoxically facilitated the common law’s successful adaptation, in spite of itself, to the needs of modern capitalism and bureaucratic government, which themselves are formally rational (and note that many of the great British theorists of modern capitalism and democratic government were Scottish rather than English, and thus heirs of the Roman law tradition). Allowing for this exception, Weber’s argument is that the dominance of highly abstract formal rationality in legal, political, and economic domains is manifestly a modern European phenomenon. It explains the acceptance in Europe (and in European colonies in America) of political theories based on the abstract—and in cross-cultural terms, rather odd—conception of the impersonal “state” as the representative and embodiment of the collectivity of individual “citizens,” viewed as interchangeable legal-political-economic atoms in abstraction from their substantive socioeconomic positions and personal kinship ties.

The substantively rational legal and political thought of the ancient Near East was quite different. It was guided by the value-rational principle of the patriarch’s concern for the welfare of his people, which is the basis of his traditional authority. That the king is the father and shepherd of his people was a common refrain in the ancient Near East. As the patriarch, the ruler is expected to care for his “household” and to adjudicate disputes. In patrimonial Near Eastern society, therefore, law is rooted in the traditional value placed on obedience to the patriarch and his reciprocal obligation to care for his people. Hence it is reactive and restorative, aiming to maintain the harmony of the “household” that is the state; it is not a proactive instrument by which the lawgiver systematically attempts to organize social life according to a transcendental vision of the good society. Furthermore, in terms of the broad scheme of historical development adopted in the present work, formal rationality (and legal-rational legitimation in general) becomes socially effective on a large scale only with the widespread inculcation of a symbolically differentiated awareness of the gulf between the mundane and transcendent levels of reality, and the corresponding impulse to solve the “problem of salvation” through the conscious reordering of individual and social life (see chapter 5.2).

Few specialists in ancient Near Eastern law have made explicit reference to Weber’s typology of law, but his distinction between substantive and formal rationality is mirrored in current approaches to the literature of Mesopotamian “science,” to which cuneiform law collections have been compared. Mesopotamian medical and divinatory treatises, for example, consist of lists of examples presented as hypothetical situations and their consequences. In contrast to Greek science, there is no attempt to develop comprehensive categories and to work formally from general principles to particular cases, although underlying substantive principles of organization can sometimes be detected. In the same way, Mesopotamian laws are presented as more-or-less ad hoc collections of hypothetical cases, and there is no overarching logic that explains their arrangement or their content (although the motivation for preparing the

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16 Although it is beyond the scope of this work to treat Islamic law and culture in any detail, it should be noted that medieval Islamic jurisprudence, which built on the earlier Roman and Hellenistic legal traditions of the eastern empire (Crone 1987b), in certain respects strove for the kind of universal consistency of legal principle that characterizes later Continental European law. This is not surprising, in view of the conceptual basis of Islamic law in an emphatically transcendent monotheism with corresponding egalitarian tendencies. I am grateful to Fred Donner for making this clear to me.

17 Compare the much later Ottoman idea of the state as a household, in which the sultan “was the head of the household, the dynasty was the family proper, the ruling class comprised the slaves who served in the household, the subject classes were the ‘flocks’ (reaya) entrusted by God to the care of the family head, and the territory of the state . . . was the dynastic patrimony” (Findley 1980:7; see also Findley 1989:42).

18 Weber (1978:1107) notes that “patrimonialism must legitimize itself as guardian of the subjects’ welfare in its own and in their eyes. The ‘welfare state’ is the legend of patrimonialism, deriving not from the free cameraderie of solemnly promised fealty, but from the authoritarian relationship of father and children. The ‘father of the people’ (Landesvater) is the ideal of the patrimonial states.”
collections in the first place may be attributed to what Weber calls the “theoretical” rationality of intellectuals everywhere; in this case, scribes and priests).  

One scholar who does address Weber’s sociology of law directly with respect to ancient Mesopotamian law is Norman Yoffee. Yoffee (1988a:96) criticizes as ethnocentric what he calls “Weber’s discussion of the progressive rationalization of legal authority through time”; but he fails to take account of Weber’s distinction between formal and substantive rationality, and he appears to miss Weber’s point that it is the dominance of formal as opposed to substantive rationality that is an unusual historical occurrence and is a defining characteristic of the modern West. Yoffee argues, for example, that:

When a judge’s hands are effectively tied against incorporating an understanding of the social context of a dispute by the imposition of mandatory sentencing, legal institutions become less, not more, rational. Rationality is similarly sacrificed when zero-sum game decisions instead of compromise dominate civil procedure. [Yoffee 1988a:96]

But Weber in fact was greatly concerned to stress that strict adherence to formally rationalized legal procedures can lead to substantive injustice or “irrationality,” and that this is the basic dilemma of law in the modern West. It is therefore incorrect to suppose that Weber’s emphasis on “progressive rationalization” entails an ethnocentric value judgment about the superior overall rationality, in terms of ethics and efficiency, of modern Western society. In my opinion, Weber’s distinction between the substantive rationality of most ancient Near Eastern regimes and the formal rationality of modern society is preferable to the model of legal and social evolution that Yoffee proposes in its stead. Yoffee suggests that:

the evolution of formal legal institutions, at least in a quantitative sense, must be considered dependent on the development of political institutions. That is, in the evolution of early civilizations, corporate groups are progressively drawn into a complete matrix, in which an emergent center achieves unprecedented power to mediate conflicts that are generated from new and more intensive kinds of social and economic interactions. . . . The evolution of specialized and bureaucratized legal systems, therefore, is inextricably linked to the nature of expanding political authority. [Yoffee 1988a:96ff.]

Yoffee seems to be saying that the role of the ancient Mesopotamian ruler as universal lawgiver and judge was not embedded in preurban traditional society, but was an innovative functional response to increasing social complexity. “Formal legal institutions,” of which the king was the representative, provided the necessary legitimation that permitted the state, as Yoffee puts it, “regularly to disembowel goods and labor from their social matrices” (ibid., p. 97). Yoffee’s functionalist argument is therefore parallel to that of Giorgio Buccellati (1977; 1996), discussed below in chapter 9.3, concerning the emergence of impersonal bureaucracy in ancient Mesopotamia—that the quantitative enlargement of the social group necessitated “specialized and bureaucratized” social roles that were different in kind from what had existed in earlier times. The implication is that formal rationality (in law and in administration) is a necessary feature of an urban “state” society in any period, and so cannot be distinctive of the modern West, as Weber maintains.

Yoffee (1988a:106ff.) argues, furthermore, that the self-presentation of the lawgiver Hammurapi, for example, who claims to uphold traditional patriarchal values, serves as political propaganda to justify his control and expropriation of resources to serve his own ends. Buccellati (1977:34) makes the same point about the propagandistic function of the personalized, fatherly image of the king. There is a sense in which this view is correct, of course, because it is a fair presumption that all rulers hide their own ambitions, to some extent, behind a mask of disinterested concern for the welfare of their people. Moreover, Yoffee does not deny that this rhetoric was sincere, on one level. But by contrasting Hammurapi’s “propagandistic” appeal to traditional values with the reality of his coercive regime, Yoffee implies that the traditional
conception of the Mesopotamian king as shepherd of his people and protector of widows and orphans was somehow at odds with his demand for total obedience and his claim of total ownership of the land and its inhabitants.

Yet, in the patrimonial conception of society, the ruler’s despotic authority over his “household” (i.e., his kingdom) coexists quite naturally with his paternal benevolence for its members. In such societies, traditional values have always included the right of the master, at every political level, to dominate his sons and servants and to claim their goods and services. There is no need to oppose, as Yoffee and Buccellati do, the ubiquitous belief in personalized kingship against what they assume to have been the qualitatively different phenomenon of the impersonal coercive “state.” To do so is to reduce the native understanding of the king’s role to the status of ideological mystification—an opiate for the people—rather than accepting it as actually constitutive of ancient Mesopotamian social order, as one must do from a hermeneutical perspective.

In my opinion, the evidence we possess can be explained more adequately by viewing complex urban society in ancient Mesopotamia according to the substantively rational patrimonial model rather than a formally rational bureaucratic model of the impersonal state. In the Bronze Age, in particular, kingship and law are best understood in terms of traditional legitimation and substantive rationality. This is not to deny that Hammurapi’s laws were politically expedient and ultimately self-serving in their appeal to traditional values. But Hammurapi’s publication of a particular set of laws throughout his domain is not by itself indicative of formal rationality, and it in no way proves that his regime was a bureaucratic state based on depersonalized legal-rational legitimation. Nor is it evidence that his regime was at heart a secondary alien imposition, grafted onto a qualitatively different phenomenon of the impersonal coercive “state.” To do so is to reduce the native understanding of the king’s role to the status of ideological mystification—an opiate for the people—rather than accepting it as actually constitutive of ancient Mesopotamian social order, as one must do from a hermeneutical perspective.

In a patrimonial state characterized by substantive and practical rationality—regardless of its size and degree of centralization—there is typically no real market in basic commodities, most of which circulate instead on the basis of reciprocity and redistribution. The famous analytical distinction between “reciprocity,” “redistribution,” and “market exchange” was popularized during the 1950s and 1960s by the economic historian Karl Polanyi, who insisted that there was no appreciable market economy in ancient times because economic transactions were embedded within traditional social relationships. Polanyi’s view was accepted, with slight modifications, by the

20 In his book How “Natives” Think: About Captain Cook, For Example, Marshall Sahlins (1995) makes a similar point in response to the charge that he and other Western anthropologists have wrongly and ethnocentrically concluded (for example) that Hawaiian islanders of the late eighteenth century were lacking in rationality and so regarded Captain Cook as a god, against all “evidence” to the contrary. As Sahlins vigorously retorts, this criticism is itself ethnocentric because it attributes to Hawaiians and other non-Western “natives” a modern “bourgeois” mode of rationality that is not documented empirically. Sahlins speaks for many anthropologists when he says (p. 14) that “common-sense bourgeois realism, when taken as a historiographic conceit, is a kind of symbolic violence done to other times and other customs. I want to suggest that one cannot do good history, not even contemporary history, without regard for ideas, actions, and ontologies that are not and never were our own. Different cultures, different rationalities.”
eminent Assyriologist Leo Oppenheim, although a number of other Near East specialists have since criticized this approach (see the edited volume Trade and Market in the Early Empires [Polanyi et al. 1957] and Oppenheim’s essay therein; also Polanyi 1968; Oppenheim 1970; for a strongly anti-Polanyi position, see Snell 1991 and 1997:149–52).

Johannes Renger (1984; 1993; 1994), however, continues to defend Polanyi’s basic position, citing textual evidence from the Old Babylonian period in particular. Renger discusses in some detail the theoretical background of Polanyi’s model and the revisions that must be made to Polanyi’s initial formulation of the “substantivist” argument. For example, the physical existence of marketplaces in ancient Near Eastern cities (which Polanyi denied) must be acknowledged, as well as the occurrence in certain cases of fluctuating market-based prices and the use of silver as an indirect medium of exchange (as opposed to being simply a unit of account or a store of value), especially in the context of long-distance trade. The best-attested example of this is the Old Assyrian donkey caravan trade with Cappadocia in the early second millennium B.C., in which tin and woolen textiles acquired in Mesopotamia were exchanged for Anatolian silver and gold (see Veenhof 1972; 1995; Larsen 1976). But despite his acknowledgment of the existence of occasional “market elements” (which Polanyi himself accepted in his later work), Renger supports Polanyi’s basic thesis, with which I also concur, namely, that market exchange entails actions that are relatively disembedded from traditional social relationships, and so was insignificant in most economic contexts in the Bronze Age Near East.

Renger thus perpetuates the view of an earlier generation of Assyriologists, including Leo Oppenheim (1970:18f.), who found no evidence that in Bronze Age Mesopotamia the localized “internal trade” in foodstuffs (or even in locally circulating luxury items originally obtained by the elite via long-distance trade) made use of “regularly functioning and integrated markets,” although Oppenheim notes that there were occasional “market situations”; for example, simple in-kind credit and lending services offered by tavern-keepers and craftsmen. There was an important economic change in this regard in later Mesopotamian history, however, which was related to the widespread rationalization of economic and social life during the first millennium B.C. In the Neo-Babylonian and Achaemenid periods there is considerable cuneiform evidence of market exchange and the widespread use of silver as true money, including a thriving urban retail trade in salt, wine, beer, pottery, and other goods (see Joännès 1995 for a survey of the evidence). According to Oppenheim (1970:21), this is in contrast to the earlier situation: “It is rather obvious from the letters and administrative documents of the Old Babylonian period that, at that time, silver as a means of exchange was hardly changing hands outside of the contexts of the palace and the overland trade.”

In Weber’s terms, a market economy involves the dominance of instrumentally rational action in which there is a means-end calculation on the basis of formally rational rules of behavior that are applied uniformly and without regard to personal status. Redistribution and reciprocity, on the other hand, involve value-rational action based in substantive rationality, which must take account of personal relationships, especially in cases where the substantive values that are invoked entail a belief in what constitutes acceptable behavior within “father-son,” “brother-to-brother,” and “master-servant” social relationships. Reciprocal and redistributive modes of economic exchange are therefore very common at all social

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21 Snell (1982) has studied cuneiform accounts from the city of Umma in order to determine whether they reflect administered equivalencies (which Polanyi’s model envisions) or true market prices. Rather than demonstrating the outright dominance of market exchange, however, Snell (1991:133) is forced to acknowledge that “the prices in the series do not correspond to what one expects from the demand curve of classical economic theory. . . . That is, merchants do not buy more of a product when it is cheap and less when it is expensive.” Furthermore, “a significant but not predominant proportion of products [42%] was perhaps traded by equivalencies,” because their prices either did not change or increased by large percentages or round-number multiples of the former price. Snell makes much of the apparently market-based prices of the other 58% of the commodities, but aside from the difficulty of generalizing about the overall economic impact of market exchange on the basis of one sample of accounts, it is worth noting that Snell’s “market-priced” commodities are mostly metals, minerals, and timber or tree products. In other words, they were not locally produced but were acquired via long-distance trade (which also explains why their total silver value was five times greater than that of the equivalence-priced commodities). As Renger (1984) points out, it is necessary to distinguish long-distance trade in high-value commodities from the local circulation of subsistence goods, which dominated what was always a basically agrarian economy.

22 Oppenheim notes that there is some evidence for Bronze Age retail trade in various commodities, but for him (and for Renger) the existence of such “market situations” does not bespeak an integrated market economy, because they are clearly a minor feature and supplemental to the basic subsistence economy. Finley (1985) makes the same argument with respect to the classical period.
levels in a traditional patrimonial society, in which universal impersonal rules of social action are largely unknown and personal relationships serve to mediate and constrain most economic activity. But it must be stressed that it is a question of the relative proportions of reciprocity, redistribution, and market exchange, and not their simple presence or absence (despite Polanyi’s own rather dogmatic assertions on this score).  

Furthermore, it should be noted that the concept of redistribution allows for the possibility of a multinodal redistribution of goods operating on many different social levels, that is, operating simultaneously within each household in a nested hierarchy of households. It is not necessarily a matter of a massively centralized redistributive mechanism in which all goods flow into the palace or temple and are disbursed from there.

In general, the development of a liquid money economy, which is necessary for the wide-scale adoption of market exchange, is related historically to the emergence of legal-rational legitimation and bureaucracy, as Weber (1978:63ff.) points out. Patrimonial administration usually corresponds to a system in which officials are remunerated with land grants that tend to become personal estates; whereas bureaucracy depends on monetary compensation, which entails the conversion of state revenues into an intermediate form of wealth, thereby enforcing the separation between government assets and the personal property of government officials that is necessary to the functioning of an impersonal rationalized bureaucracy. A money economy also fosters formal

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23 Hence it is by no means an argument against the substantivist position to point out, as Snell (1991) does, that reciprocity and redistribution still occur in certain social contexts (e.g., giving a bottle of wine to one’s host) even in modern capitalist societies, which are otherwise dominated by impersonal market exchange.

24 Weber (1978:375ff.) notes that the development of a depersonalized money economy leads to a weakening of household authority and the disintegration of the household as the basic economic unit. This is in contrast to the oikos economy characteristic of patrimonial regimes, of which Weber says: “Its dominant motive is not capitalistic acquisition but the lord’s organized want satisfaction in kind. For this purpose, he may resort to any means, including large-scale trade. Decisive for him is the utilization of property, not capital investment” (p. 381). In relatively pure cases of a patrimonial household characterized by a high degree of economic self-sufficiency, “an apparatus of house-dependent labor, which often is highly specialized, produces all the goods and personal services, economic, military, and sacrificial, which the ruler requires. His own land provides the raw materials, his workshops with their personally nonfree labor supply all other materials. The remaining services are provided by servants, officials, house

25 Polanyi himself was careful not to reject the choice-in-scarcity concept in principle. It is one aspect of a universal “logic of rational action.” But, like Weber, he avoids a Western ethnocentric definition of what is “rational.” As he says, “Rational action is here defined as a choice of means in relation to ends. Means are anything appropriate to serve the end, whether by virtue of the laws of nature or by virtue of the laws of the game. Thus ‘rational’ does not refer either to ends or to means, but rather to the relating of means and ends. It is not assumed, for instance, that it is more rational to wish to live than to wish to die, or that, in the first case, it is more rational to seek a long life through the means of science than through those of superstition. For whatever the end, it is rational to choose one’s means accordingly; and as to the means, it would not be rational to act upon any other test than that which one happens to be-
Smith’s great work *The Wealth of Nations*, born of Enlightenment rationalism, modern economic theory has assumed the culturally specific ideal type of the socially disembodied and atomized “economic man” who operates on the principles of material utility and least effort. Indeed, this restrictive assumption is what permits the mathematization of economics as a highly formalized predictive discipline. Although the formalist approach works well for modern economies, the substantivist approach to premodern economic phenomena is supported by the ethnographically attested fact that “scarcity” itself is culturally defined, as Marshall Sahlins (1972) has emphasized in his essay “The Original Affluent Society.” In other words, the need to choose among scarce means to achieve one’s ends may well be a human universal, but our modern perceptions of what constitutes “scarcity” and “utility” are not. This limits the relevance of any formal economic theory that is based on these culture-bound perceptions, because premodern economic choices are very difficult to disembed from symbolically mediated substantive values and social settings for purposes of formal analysis.

What kind of analysis is possible, then, if we reject the formalist approach as anachronistic but also recognize that the substantively rational patterns of economic action involved in reciprocity and redistribution frequently entail a self-interested practical rationality that is concerned with the strategic deployment of available means to achieve certain ends? Here we must pay attention to what is actually involved in reciprocity (and in redistribution as an extended and indirect form of reciprocity). Anthropologists have long noted that in many societies lavish gift-giving increases one’s “symbolic capital,” as Pierre Bourdieu (1977:171–83) has called it, in the form of prestige or social status, and hence one’s command of material and labor resources. But this symbolic capital is embedded in specific personal relationships that have a noneconomic kinship or political dimension.26 Indeed, accumulating such capital requires disguising one’s naked self-interest by the skillful use of “regulated improvisations,” as Bourdieu puts it, that play upon the many possible ways of exchanging gifts, in terms of their timing and quality. It is therefore misleading to conceive of such an exchange in formalist terms as a simple economic transaction involving a utilitarian choice among scarce means, for this “would telescope into an instant a transaction which gift exchange disguises by stretching it out in time.” Bourdieu emphatically rejects the objectivism and economism of formalist approaches, noting that:

The theoretical construction which retrospectively projects the counter-gift into the project of the gift has the effect of transforming into mechanical sequences of obligatory acts the at once risky and necessary improvisation of the everyday strategies which owe their infinite complexity to the fact that the giver’s undeclared calculation must reckon with the receiver’s undeclared calculation, and hence satisfy his expectations without appearing to know what they are. . . . If it is true that the lapse of time interposed is what enables the gift or counter-gift to be seen and experienced as an inaugural act of generosity, without any past or future, i.e., *without calculation*, then it is clear that in reducing the polythetic to the monothetic, objectivism destroys the specificity of all practices which, like gift exchange, tend or pretend to put the law of self-interest into abeyance. . . . In reducing the economy to its objective reality, economism annihilates the specificity located precisely in the socially maintained discrepancy between the misrecognized or, one might say, socially repressed, objective truth of economic activity, and the social representation of production and exchange. [Bourdieu 1977: 171ff.]

Clearly, we are back in the realm of an interpretive “motivational” analysis of economic behavior, as opposed to an objectivist causal analysis. Underlying Bourdieu’s intriguing notion of symbolic capital and his description of the process by which economic capital (material wealth) is converted into symbolic capital is a phenomenological attention to the specificity of human lived experience, as this is expressed in language. By avoiding the reductionism that collapses all human rationality into formal rationality, Bourdieu is able to explicate the complex interplay of what Weber called practical and substantive rationality in patterns of premodern economic action. Typically, this entails disguising the instrumental calculations that serve pragmatic self-interest in order to

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26 A classic study of gift exchange is the 1925 *Essai sur le don* by Marcel Mauss (1967).
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preserve the substantive value placed upon open-handed generosity and good faith in one’s personal dealings—a value-postulate that legitimates “objective” relationships of domination and subordination. In the Bronze Age Near East, in particular, Bourdieu’s hermeneutical concept of symbolic capital helps to illuminate the dialectic of fact and symbol within the segmentary hierarchies that make up the patrimonial “house of the father.”

5. Long-Distance Trade and the Hermeneutics of “World-Systems”

Following Polanyi and others, I have questioned the dominance of impersonal market exchange in premodern settings, and hence the relevance of objectifying methods of formal economic analysis, stressing rather the importance of socially embedded and thus symbolically mediated patterns of reciprocity and redistribution. An important exception in certain cases is long-distance trade, which in the ancient Near East was usually trade in nonsubsistence or luxury commodities (goods of high value in relation to their weight that are worth shipping over long distances despite the high cost of transportation). Long-distance trade might well involve transactions between strangers; that is, merchants who are far from home and thus have no choice but to be socially disembedded economic agents. Such trade can therefore exhibit features of modern market exchange, regardless of its historical and cultural setting. This accounts for the appearance in the Old Assyrian donkey caravan trade of market phenomena such as price-formation on the basis of supply and demand. Under the right conditions, long-distance trade may also have a significant impact on patterns of social action, perhaps by elevating the social status of merchants or by creating an integrated interregional market in a specific commodity.

Nevertheless, such effects will be ephemeral and marginal in proportion to the entire agrarian economy in the absence of formally rationalized legal and political mechanisms that can reliably enforce the implicit or explicit contracts entailed in market exchange. High transaction costs due to a lack of formally established financial mechanisms, as well as high transportation costs (especially in overland trade) due to a low level of technology, have combined in most premodern societies to inhibit the development of a market economy. This is true not just in the ancient Near East but even in the more highly monetized classical Greek and Roman economies, as Moses Finley (1983; 1985) has argued. Finley is scathing in his indictment of the essentially functionalist argument from silence, so often advanced, which says that an integrated market economy “must” have existed in highly urbanized ancient societies, despite the lack of textual evidence for this.

Furthermore, in the second-millennium Near East, in particular, there is considerable evidence to support the view that most long-distance merchants were not independent entrepreneurs but were royal agents and ambassadors who engaged in palace-to-palace “administered” or “gift” trade, even if they also did some private trading on their own account. This

27 Bourdieu (1977; 1991) has achieved great popularity among anthropologists and sociologists by elaborating the insights of hermeneutic (Heideggerian) phenomenology in a Marxist idiom which is familiar to many social scientists, but it should be remembered that he uses objectivist Marxist terms like “capital,” “class,” and “market” in a decidedly non-Marxist sense. His resonant metaphor of the “economy of linguistic exchanges” (as in the subtitle of his 1982 book Ce que parler veut dire: l’économie des échanges linguistiques), with submetaphors such as “symbolic capital,” “symbolic profit,” and “linguistic market,” serves to highlight and clarify aspects of social relationship which hermeneutically oriented social theorists have expressed in other ways (e.g., Weber’s “legitimate authority”).

28 The clash between “primitivist” and “modernizing” views of ancient economies was inaugurated by Karl Bücher and Eduard Meyer in the 1890s. In the early decades of this debate, scholarly discussion was sidetracked into the question of the degree of monetization of the economy and whether large-scale trade and industry existed. But as Finley emphasizes, the important question is whether economic production, distribution, and consumption were fundamentally organized by means of wide-scale integrated markets, or on some other basis. Reactions to Finley’s substantivist approach with respect to the Athenian economy of the fifth century B.C., in particular, are discussed in Davies 1992 (see more generally Davies 1998).

29 The Old Assyrian trade with Cappadocia remains an exception, but this more “private” and decentralized trading network is explicable in the particular political environment of the early second millennium B.C., in which no strong imperial powers existed to inhibit it either in Anatolia or Mesopotamia (see Oppenheim 1970:17). It is not clear how “private” this trade was, in any case, as Renger points out, because we do not know the precise nature of the Assyrian traders’ relationship to the rulers of Assyrian merchants, or the social position of their counterparts in Anatolia (Renger 1984:38).

It is plausible to assume that in periods of political centralization, especially, long-distance merchants required diplomatic recognition and royal protection: “They had to be recognized as emissaries by their own king and by those of the regions crossed. Moreover, the capital needed to support large scale trade ventures or even to share in them could hardly have been amassed except under the protection of the palace organization, in fact, only by members of the latter” (Oppenheim 1970:16). Renger therefore disputes the characterization of Mesopotamian merchants as “private entrepreneurs” by Adams (1974:247). In the “Palast...
The most obvious form of long-distance trade was in its own way embedded in highly personalized social relationships, namely, the relationships among kings who regarded themselves as “brothers” or “fathers” or “sons” of one another, and who were also frequently related by marriage (see chapter 12.1 below on the household basis of political and diplomatic terminology). Letters between kings—for example, the Late Bronze Age correspondence found at el-Amarna in Egypt (ancient Akhetaten, the capital of New Kingdom Egypt under the pharaoh Akhenaten)—discuss exchanges of goods in terms of very specific kinds of reciprocal gifts, for which there are no market-based prices but rather rough equivalencies. These equivalencies are themselves subject to dispute and negotiation on a case-by-case basis, involving personalized pleas for adherence to customary standards of generosity and fair dealing.\(^{30}\)

It is worth noting also that royal sponsorship extended to maritime trade by ship in the Mediterranean Sea and was not restricted to overland caravan trade, even though caravans are mentioned more frequently in the Amarna letters. It is true that the cost of transportation per unit of weight was always far less in waterborne trade, so that under the right conditions one might expect to find a more utilitarian sea trade in staple commodities. But for the Bronze Age, at least, this functional consideration is not easily translated into proof of decentralized and depersonalized market exchange. Much more in the way of human and material resources was required to build a ship and to outfit a maritime trading venture than to mount overland caravans, which were thus always potentially more autonomous or “private,” as in the Old Assyrian case. In addition, political rulers controlled the seaports, and hence the maritime trading routes, to a degree that was not matched in overland trade. It is not surprising, therefore, that the available textual and archaeological evidence suggests that seaborne trade formed part of a network of centrally administered palace-to-palace reciprocal exchanges, especially in the context of the second millennium B.C. “palace economies” of the Eastern Mediterranean and Aegean.\(^{31}\) There is simply no evidence for private merchants and shippers who had access to credit facilities and other resources apart from their political position within a palace establishment. We may be tempted to view certain kings (in Ugarit, for example) themselves as mercantile oligarchs, who invested heavily in trading ventures that were conducted by their designated trading agents or “merchants” (Akk. tamkārū). But we cannot separate a ruler’s role in trade from the political and social relationships in which he was embedded as a vassal of this or that emperor and as the patrimonial master of a given territorial domain. Similarly, the merchants and other officials who served in a palace organization as part of the royal household were themselves embedded in local social relationships to an extent that renders anachronistic any comparison to the capitalistic entrepreneurs and seafarers of recent centuries.\(^{32}\)

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\(^{30}\) On “ceremonial exchange” between kings in the second millennium b.c., see Zaccagnini 1973; 1987; 2000. Concerning “brotherly” complaints about the quality of royal gifts, note in particular the Late Bronze Age Amarna letters EA 19, 20, 26, and 27, translated in Moran 1992. The situation was the same in the Middle Bronze Age, as the Mari letters demonstrate. Concerning the Mari evidence, Jack Sasson (1998:459) remarks that this is a case “where ‘trade’ is but a euphemism for rulers recycling gifts among themselves, and where exchange of valuables among the elite was regulated less by market forces than by custom, honor, and fear of scandal.”

\(^{31}\) This view is expressed succinctly from the Aegean perspective by Anthony Snodgrass (1991). Snodgrass notes the huge quantities of copper and tin carried on both of the two Late Bronze Age ships whose wrecks have been discovered to date (cf. Bass 1986; 1991; Bass et al. 1989). It is likely that these metals were being brought from the east (the copper from Cyprus, in particular) to the Aegean, and: “If we search at the Aegean end for a plausible destination for such a cargo, then there can only be one answer: a palace; a palace like Pylos, which had a standing force of at least 274 bronzesmiths, listed on the set of 27 relevant [Linear B] tablets . . . .” (Snodgrass 1991:18).

\(^{32}\) In his detailed study of international trade in the Late Bronze Age, Eric Cline (1994:85) notes the abundant textual evidence for “diplomatic merchants and emissaries representing the Crown”; however, like many other scholars he also assumes that “examples can be readily found for independent commercial merchants.” But where are these examples in the imperial period of the Late Bronze Age? Archaeological evidence alone proves nothing other than that goods were exchanged over long distances, and there is not a scrap of direct textual evidence for the existence of a private commercial sector independent of the palace organizations. At most, it is plausible to assume that royally sponsored merchants might also have engaged in some private trading on their own account in the course of their missions. There is an important methodological issue here, because highly anachronistic (and I think erroneous) analogies with medieval and modern Western phenomena are repeatedly adduced in discussions of Bronze Age Mediterranean trade. Yet it is an empirical question and not a matter of mere deduction as to whether ancient peoples were, in this regard, “just like us.”
The prevalence of socially embedded reciprocity even in long-distance trade calls into question attempts to apply Immanuel Wallerstein’s (1974) Marxist and strongly economistic “world-system” model to the Bronze Age (and earlier) Near East. Wallerstein himself stresses the uniqueness of the “modern world-system,” which is based in the capitalist world economy that has emerged since the sixteenth century A.D. His model specifically rests on the notion that the world-system, as a structure of domination and exploitation, is held together economically and not politically, unlike earlier imperial systems:

In the late fifteenth and early sixteenth century, there came into existence what we may call a European world-economy. It was not an empire yet it was as spacious as a grand empire and shared some features with it. But it was different, and new. It was a kind of social system the world has not really known before and which is the distinctive feature of the modern world-system. It is an economic but not a political entity, unlike empires, city-states and nation-states. . . . And it is a “world-economy” because the basic linkage between the parts of the system is economic . . . [Wallerstein 1974:15]

The economic linkage that ties together this world-system—appropriating the surplus of the “periphery” for the benefit of the “core” without the need for direct political domination—functions by means of depersonalized market exchange. And Wallerstein follows Polanyi in concluding that this kind of exchange, involving the interregional integration of patterns of production and consumption in response to market forces of supply and demand, became pervasive only because of “the techniques of modern capitalism and the technology of modern science,” especially the technology of transportation which made it feasible to ship bulk commodities over long distances. Wallerstein argues that market-based integration of economic activity across widely separated regions produces a hierarchical division of labor that is analogous to Marx’s dichotomy of the exploited proletariat and the capitalist bourgeoisie, but is worldwide rather than merely local in scope.33

Gil Stein (1998b:224) notes that the world-system model has become very popular among anthropologists and archaeologists “because it links politics, economics, geography, and even ideology into a unified construct that addresses the developmental dynamics of complex societies at an interregional scale.” Modified versions of the world-system model have been applied to the ancient Near East, especially to Uruk-period Mesopotamia and its hinterland in the fourth millennium B.C. (Kohl 1987; Algaze 1993).34 But Stein questions its relevance in this context (see also Stein 1999). Although archaeologists who apply the world-system model to the premodern world do not necessarily assume capitalistic market exchange in bulk commodities, and thus diverge substantially from Wallerstein’s own definition of the model, they retain three fundamental assumptions that are problematic in many contexts in the Bronze Age Near East, as Stein points out:

First, [the world-system model] assumes a fundamental power asymmetry between the different parts of the system so that the core politically dominated the periphery, either through direct colonial administration or through a combination of economic and cultural hegemony. Second, the model assumes that as a result of these asymmetries the core was able to control the exchange system. Third, the model assumes that long-distance exchange relations structure all other aspects of political economy in peripheral societies. [Stein 1998b:226]

Stein challenges the validity of all three of these assumptions for the Uruk period (and hence for the Chalcolithic and Bronze Age Near East in general, in light of the similar conditions for long-distance trade that prevailed until at least the first millennium B.C.). He argues that the “leveling effect of distance” prevented the imposition of asymmetric exchange relations on the nonurban periphery by the newly urbanized southern Mesopotamian core, nullifying whatever advantage the core might have had in terms of superior organization and technology. Indeed, the metal-rich Anatolian “periphery” was actually superior to Mesopotamia in its metallurgical technology. It did not supply southern Mesopotamia with unfinished bulk commodities, as in Wallerstein’s model of asymmetric exchange, but with highly processed and technologically advanced products. Stein points out that even Phil Kohl, a proponent of a world-system model for the Bronze Age Near East, acknowledges

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33 It should be noted that Wallerstein’s core-periphery model for the modern world, which is rooted in “dependency theory,” is itself highly disputable, apart from the question of its applicability to premodern settings.

34 For other examples of the application of the world-system model to premodern settings, see Abu-Lughod 1989; 1990; Chase-Dunn and Hall 1991; Kardulias 1999; and especially Chase-Dunn and Hall 1997, which provides a detailed survey and comparison of attempts to identify premodern world-systems. Despite his initial emphasis on the uniqueness of the modern world-system, in a later essay Wallerstein (1990) himself appears to leave open the possibility of identifying world-systems other than the modern capitalist world economy, although it is not clear what he regards as the minimal essential criteria for a world-system.
that “[t]he existence of multiple cores, easily transferable technologies, and primitive means of transportation and communication led to the emergence of a contingent and rapidly changing social landscape in which core areas could only exercise ephemeral and circumscribed influence over neighboring regions” (p. 227).

In short, there is no evidence (in this or in later Bronze Age contexts) for a system of unequal exchange that was somehow coercively imposed by a dominant core—unless one speaks of direct imperial domination by military means, in which goods are transferred one-sidedly to the political core in the form of tribute and not by means of “exchange” at all. Although the term “world-system” is occasionally used loosely to refer to the economic aspects of political empire, this is incorrect and deprives the term of any useful meaning. As originally defined, the term refers specifically to an economic system of domination in the absence of overt political control, an arrangement that is less expensive and more efficient from the perspective of the core.

Furthermore, as Stein emphasizes, it is erroneous to assume that long-distance trade is a major factor in local economic and social change, unless “the volume of trade reaches a sufficiently high level to transform peripheral production into an export-oriented system,” a situation that is not often attested in the ancient Near East. Stein’s own “distance-parity” model of exchange relations takes full account of the limiting effects of distance in a world in which means of transportation and financial mechanisms were both quite rudimentary:

Under these conditions, core-periphery interaction is expected to be limited in scale and influence such that the peripheral political economies do not necessarily develop the specialized dependency relations predicted by the world system model. Economic compartmentalization and the lack of fungibility or interchangeability between different spheres of economic activity would tend to limit the degree to which small-scale elite exchanges of luxury goods could have transformed entire economic systems in the periphery. [Stein 1998b:229]

It is clear that there are serious empirical weaknesses in any world-system model of the Bronze Age Near East that assumes asymmetric exchange and hence core dominance. This leads Stein to abandon the world-system approach altogether. But others would argue that we can still usefully conceive of a premodern world-system in which there is neither depersonalized market exchange on a wide scale, nor even a system of asymmetric reciprocal exchange. All that is needed is some notion of intersocietal interregional interaction network is called a world-system, Stein summarizes his very cogent and damaging critique of this approach as follows: “In its original form, the three assumptions of Wallerstein’s world-systems model (core dominance, asymmetric exchange, and long distance exchange as the prime mover of social change) simply do not work, and have had to be discarded by the vast majority of people who have attempted to apply the model to pre- or noncapitalist societies. Attempts to modify Wallerstein’s model by relaxing most of its main assumptions are equally problematical for two reasons. First, the construct becomes so broad and amorphous that it loses any kind of analytical power, except as a generalized philosophical outlook. However, when virtually every multi-group interaction network is called a ‘world-system,’ then the term becomes meaningless. Second, even in its modified form, the world-systems construct still views the external dynamic of interregional interaction as the main structuring element at both the local and macro-regional level. As a result, despite its explicit recognition that peripheral polities can be important in the overall scheme of things, the modified world-systems perspective continues to minimize the role of agency and internal dynamics in these ‘peripheries’ since the causes of change are always situated somewhere on the outside... If we adhere to Wallerstein’s original construct, then we have a model which has clear assumptions and an explicit core-dominant view of the world, but simply does not work for pre- or noncapitalist societies. On the other hand, if we embrace the modified world-systems perspective, then we have a construct that gains broad cross-cultural applicability to virtually all interregional networks everywhere, but does so by sacrificing all analytical specificity about how these systems actually work” (Stein, in Kardulias 1999:159).

35 The work of Chase-Dunn and Hall (1997) is an extreme example of this “everything is a world-system” approach. They dispense not only with core-periphery asymmetry but even with material exchange itself, arguing that world-systems exist in all societies on every possible scale, from small bands to complex states. Here “world-systems” are redefined in the broadest possible terms as “intersocietal networks in which the interactions (e.g., trade, warfare, intermarriage, information) are important for the reproduction of the internal structures of the composite units and importantly affect changes that occur in these local structures” (ibid., p. 28). Stein summarizes his very cogent and damaging critique of this approach as follows: “In its original form, the three assumptions of Wallerstein’s world-systems model (core dominance, asymmetric exchange, and long distance exchange as the prime mover of social change) simply do not work, and have had to be discarded by the vast majority of people who have attempted to apply the model to pre- or noncapitalist societies. Attempts to modify Wallerstein’s model by relaxing most of its main assumptions are equally problematical for two reasons. First, the construct becomes so broad and amorphous that it loses any kind of analytical power, except as a generalized philosophical outlook. However, when virtually every multi-group interaction network is called a ‘world-system,’ then the term becomes meaningless. Second, even in its modified form, the world-systems construct still views the external dynamic of interregional interaction as the main structuring element at both the local and macro-regional level. As a result, despite its explicit recognition that peripheral polities can be important in the overall scheme of things, the modified world-systems perspective continues to minimize the role of agency and internal dynamics in these ‘peripheries’ since the causes of change are always situated somewhere on the outside... If we adhere to Wallerstein’s original construct, then we have a model which has clear assumptions and an explicit core-dominant view of the world, but simply does not work for pre- or noncapitalist societies. On the other hand, if we embrace the modified world-systems perspective, then we have a construct that gains broad cross-cultural applicability to virtually all interregional networks everywhere, but does so by sacrificing all analytical specificity about how these systems actually work” (Stein, in Kardulias 1999:159).
what kind of socioeconomic integration might be created by the distribution of luxury goods, calls for a hermeneutical as opposed to an objectivist approach. In many premodern societies, the worth and utility of material objects are not objectively defined by market forces in terms of a uniform monetary scale of value, but are culturally defined in terms of a given constellation of intersubjective symbolic meanings. This point is emphasized by Jane Schneider (1977), who published a widely cited article that was an early attempt to apply Wallerstein’s world-system model to premodern social settings.36 She agrees with Wallerstein that before the emergence of the capitalist world-system “most exchanges between distant places involved the movement of luxuries—of exotic goods or ‘preciosities’ which were very high in value per unit of weight and therefore relatively easy to transport” (p. 21). But she argues that Wallerstein neglected “the systemic properties of the luxury trade” (ibid.) itself, because he made too strict a dichotomy between ad hoc trade in high-value luxury items as opposed to the integrative market exchange of bulky staple commodities, which promotes a specialized division of labor such as we see in modern capitalism.

Schneider’s own world-system concept envisions a luxury-trade analogue for the systemic integration created by large-scale market exchange of bulk commodities. For her, the precursor of the world-system entails the reciprocal and redistributive transfer of highly valued items for explicitly political purposes; in particular, for “the construction of great patron-client chains” (p. 27). She notes that “the relationship of [luxury] trade to social stratification was not just a matter of an elevated group distinguishing itself through the careful application of sumptuary laws and a monopoly on symbols of status; it further involved the direct and self-conscious manipulation of various semiperipheral and middle level groups through patronage, bestowals, and the calculated distribution of exotic and valued goods” (p. 23).

But here we are led back to Bourdieu’s antiobjectivist concept of “symbolic capital,” based in substantive and practical rationality rather than formal rationality. In the end, Schneider’s paternalistic world-system of gift and power is radically different from Wallerstein’s objectivist concept, although she does not acknowledge this. Her world-system is not a matter of impersonal market forces underpinned by formally rational capitalist institutions that can disem

1. Phil Kohl (1987) takes a similar approach and cites Schneider approvingly in his own influential paper on “the Bronze Age world-system” in the Near East.

36 Phil Kohl (1987) takes a similar approach and cites Schneider approvingly in his own influential paper on “the Bronze Age world-system” in the Near East.
Weberian methodological individualism, world-system models, like other objectivist models of social systems, have as their basic components illicitly hypostatized patterns of social action rather than individual social actors, whose actions are always symbolically mediated and constrained.

It is difficult to discern, therefore, what meaning the term “world-system” could have for historical situations that are fundamentally different from what Wallerstein originally defined in terms of a market economy. Unless one adheres to functionalist and formalist assumptions and simply deduces the existence of integrated market economies in premodern contexts, regardless of the lack of evidence for this, one must redefine the concept of a world-system in such a way that it loses whatever analytical value it has even within the objectivist intellectual framework in which it was formulated. Whatever the merits of Wallerstein’s analysis of market-driven exploitation and interregional dependency relationships in the colonial and postcolonial modern world, the Bronze Age Near East does not satisfy the most basic criteria of his model, as he himself was the first to point out. In the absence of evidence for sustained and widespread mechanisms of monetized and marketized trade in subsistence goods, it is difficult to see how an economically integrated world system might arise. And without market-based economic integration on a wide scale, one must question the relevance of Wallerstein’s world-system model for premodern contexts.

This is not to deny the existence of intersocietal “core-periphery” integration before the modern era, nor to deny that the transfer of goods can help to accomplish this integration. But the mode of integration here is ideological and personal, not “economic” in the modern sense, because the precapitalist “luxury” trade was just that: trade in luxuries whose local value and use was determined by a culture-specific network of symbolic meanings. From the perspective of modern formal economics, it is a matter of nonessential “preciosities” (as Wallerstein puts it) circulating in a socially restricted sphere, rather than the extensive circulation of fungible utilitarian commodities. And this is so precisely because the goal of this trade was not to supply basic needs or foster an economically efficient division of labor. Long-distance luxury trade served rather as a symbolic vehicle for the legitimation of political domination (or for the mutual legitimation of the respective spheres of domination of trading partners who acknowledged one another as equals and wished to maintain friendly relations). This sort of “economic” transaction does indeed allow a “symbolic capitalist” to exercise practical means-end calculation in order to accumulate and mobilize resources. But the exchange of goods achieves these practical goals, in the end, by transmitting and reinforcing substantive value-postulates such as the duty of obedience to one’s patrimonial lord, who is gracious and benevolent, or the duty of brotherly love and generosity among political peers and potential allies.

Now, if we concede that in the ancient Near East a powerful “core” typically dominates and exploits its “periphery” by old-fashioned military-political means, and not through the indirect agency of an international market economy, there is still the question of whether imperial political integration itself implies any kind of economic integration between core and periphery, or even a recognizable economic advantage for the imperial power. Of course, imperial conquerors demand tribute from their victims, both as a material reward for themselves and their followers, and as a visible manifestation of their domination and the rightness of their rule. Both functions are combined in the common practice of using such tribute (usually portable high-value luxury items such as gold, silver, ivory, etc., and perhaps also specialized craftsmen to work them) in order to furnish the conqueror’s lavish palace and the temple of his god, by whose favor he has been appointed to rule. Imperial armies and garrisons also requisition foodstuffs and animal and human labor, both in their homelands and in conquered regions. They may even destroy agricultural crops, orchards, herds, and whole towns systematically, as a means of punishing their enemies. But what is “economic” about any of this in the modern sense of formal economic rationality? In my view, a fundamental issue in ancient studies that is too often ignored concerns the discrepancy between the largely ideological motivations for empire that are actually attested in the ancient world and the “real” economic motivations that many modern historians and archaeologists assume to have been at work.

In this regard, it is worth noting the exchange between Barry Kemp and Stuart Tyson Smith in the *Cambridge Archaeological Journal* (1997) on the question of the motivations for the creation of the New Kingdom Egyptian empire in Nubia. Kemp criticizes Smith’s reductionist emphasis on inferred economic motives at the expense of native expressions of imperial goals:

> From numerous reliefs and inscriptions it is quite easy to compile an outline of the theology of conquest of foreign lands by Pharaoh. ... Conquest was simply fulfilment of a duty towards the gods “to enlarge the boundaries of Egypt” ... Although this ideology permeated official records and even modes
of letter-writing, Smith allows it little value in the shaping of ancient policies. He appeals instead to a number of neat and very broad categorizations of imperialism drawn up by people predisposed to theory. [Kemp 1997:125; see also Kemp 1978]

Kemp (1997:127) rejects the view that ideology is merely a means of legitimating economic goals after the fact: “For ancient Egyptian kings theology . . . provided the principal framework of reference which gave meaning to mundane acts.” A generation earlier, Moses Finley had voiced a similar criticism of the economistic view of imperialism in the classical Greek world in his essay “The Athenian Empire: A Balance Sheet” (reprinted in Finley 1983).

Smith, however, citing Mario Liverani’s approach to Late Bronze Age royal ideology in the book Prestige and Interest (1990), responds to Kemp by saying that the contradiction (as he sees it) between Egyptian imperial ideology and actual imperial practice is what leads him to assert “the primacy of economic factors in determining imperial policy,” such that the ideology was simply “aimed at legitimizing royal authority to an internal audience” (Smith 1997:301). Now, there is no doubt that ideology is a vehicle of legitimation; yet, despite all of the attendant self-serving distortions and illusions, the question remains whether we can make such a neat separation between ideology and practice, as if practical motivations can be disembedded from the ideological symbolizations that actually constitute the social order.

I will discuss the issue of imperial ideology further in a planned second volume on the “house of the father” (currently in preparation), with respect to the Neo-Assyrian and Neo-Babylonian empires of the first millennium B.C.E., in particular. In the present volume, Liverani’s approach to the political ideology of the Late Bronze Age will be treated in more detail in chapter 12.1. It should be clear, however, that from an interpretive perspective concerned with subjectively meaningful motivations for action, native expressions of the reasons for imperial conquest must take precedence methodologically over modern researchers’ inferences concerning those reasons, especially if these inferences assume a mode of economic rationality whose existence is not documented among the social actors involved.

The continuing economism and objectivism of much archaeological research on premodern empires, with a corresponding tendency toward oversimplified historical generalizations, is evident in the works cited by Carla Sinopoli (1994) in her review article on “The Archaeology of Empires.” In contrast, it is worth emphasizing the specific religious and cosmological dimension not just of pharaonic Egyptian imperialism but also of Neo-Assyrian imperialism. This has been described by Bustenay Oded in the following terms:

The Assyrian perception of just war and the legality of embarking on war was based on their political aspirations and steeped in religious beliefs and ethical attitudes. In each pretext [for attack], the religious dimension is elicited since war is part and parcel of the religious sphere. They appeal to the authority of the gods as justification for their aggressive wars. Only the gods have the legitimate authority for declaring war. The Assyrian king was only their messenger and the “staff of their anger.” Thus, war was mandatory, not optional. . . . There was no point at which the Assyrian expansion could stop as long as any country remained independent. In an orderly world the Assyrian king stood at the summit of the universal-political hierarchy. To reject this model of centralized universal empire would have meant violation of the world order; introduction of chaos; offense against the gods and rebellion against the “great king.” [Oded 1992:185; see also Oded 1991]

Again, our starting point should be symbolically mediated social action, which gives rise to agent-oriented motivational models based in hermeneutical understanding as opposed to objectifying causal models that predict social-evolutionary “stages” or “modes of production.” There is no autonomous social-structural reality lurking behind the symbolization of social interaction; no infrastructure of systemic economic forces that generate (according to Wallerstein and many other Marxist theorists) the ideological superstructures of capitalist and precapitalist society. As Ricoeur has emphasized, and as I will elaborate further in the next section, symbolically expressed ideologies of economic exchange and of political domination, ancient and modern, are not simply inversions and distortions of “real” social relationships, but are in the first instance integrative and constitutive of human society itself.
RATIONALIZATION is a major theme in Weber’s historical sociology. We noted above that he distinguished four different types of rationality and identified diverse rationalization processes that occur in various spheres of human social life. But one kind of rationalization, in particular, became socially effective on a wide scale during the first millennium B.C. and subsequently had profound effects on the development of both Western and Islamic civilizations. This is the theoretical rationalization of the substantive “patrimonial” values that underlay the Bronze Age “house of the father,” a rationalization that in turn fostered processes of formal rationalization in economic and political spheres. An unpredictable confluence of material and ideal factors led to this reorganization of traditional values and ensured its impact on widely shared symbolizations of social order. Theoretical systematization of traditional patrimonial values took different forms in different locales, but within the West Semitic or “Canaanite” cultural milieu of the peoples living along the eastern shore of the Mediterranean, the parade example is the development of Jewish monothemism.

This much-discussed topic will be treated in more detail in a second volume on the ancient Levantine “house of the father,” provisionally titled Tradition and Rationalization in the Axial Age. At this point, I will simply note that the patrimonial cosmos imagined in Bronze Age Near Eastern polytheistic mythology, and in Canaanite mythology in particular, consisted of a grand hierarchy of patriarchal households, nested one within another (see chapter 14). From the head of the pantheon to the humblest human household, the same substantive pattern was replicated at each level of the hierarchy. By being thus integrated into the divine realm, human beings were integrated into society by means of a symbolic vision that served to legitimate and sustain traditional political and economic relationships. The symbol of the “house of the father” was extremely durable because it was rooted in everyday lived experience, and because it was sufficiently flexible and extensible to encompass all kinds of political and economic relationships.

This patrimonial cosmos was radically simplified in Israelite and Jewish religion, however, and the multiplicity of the divine was concentrated in the symbol of an all-powerful and increasingly transcendent patriarch. Earlier patrimonial values were not simply rejected, but they were reorganized in relation to the more abstract concept of a single transcendent lord. The patrimonial hierarchy was flattened to such an extent that it was now possible to imagine universal formal principles that govern the relationship between God and humanity as a whole, with less regard to one’s substantive position within the hierarchy as the son or servant of this or that human lord. In other words, the outworking of the inner logic of this theoretical rationalization meant that the patriarchal relationship was more and more concentrated in the relationship between the individual and God, and was no longer diffused indirectly through great chains of hierarchical patriarchal relationships encompassing many layers of human and divine masters. Of course, this rationalization was not total or universal, even in Jewish society. But the opening of this new vista in the symbolic imagination of the cosmos in turn permitted utopian visions that challenged the traditional patrimonial ideology by imagining and even prophesying the fundamental reordering of human social relations on the basis of increasingly egalitarian and universalizing principles.

To understand the rationalization of the “house of the father” in the first millennium B.C. we must therefore explore the dialectical relationship between “ideology” and “utopia.” On one level these are opposed to each other, for a utopian vision steps outside the current situation via the exercise of imagination and thereby establishes a critical distance from it. Hence an imagined utopia as a vision of a different social world offers a distanciating rational critique of traditional ideology. But ideology and utopia are also alike in being expressed symbolically and in reflecting substantive values; indeed, when they are widely adopted, utopian values themselves become an ideology that legitimates certain patterns of social action. This interplay of traditional ideology and rational utopia reflects what Weber called the “problem of salvation”—the awareness of the gap between transcendent and mundane levels of existence, and the consequent necessity of rationally remaking oneself and one’s society as a whole in line with a vision of transcendent reality. During the so-called Axial Age

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1 See also Guenther Roth’s comparison of Weber’s concept of rationalization and Braudel’s longue durée approach to long-term historical developments in his essay “Duration and Rationalization” (in Roth and Schluchter 1979).
of the first millennium B.C., the rationalization of traditional symbols led to an acute awareness of the problem of salvation, and thus led to utopian solutions to this problem, with potentially powerful social effects.

1. Traditional Ideology and Rational Utopia

In the conventional Marxist view, ideology is always a self-interested distortion of a more fundamental reality, namely, the “praxis” or practical human activity of material production and exchange. But this praxis, which Marx and Engels in The German Ideology call the “language of real life,”2 is always already symbolically mediated and thus ideological, as Ricoeur stresses in his essays “Science and Ideology” and “Ideology and Utopia” (in Ricoeur 1991; see also Ricoeur 1986). If the purpose of ideology is to present the particular interest of the ruling class as the common interest of all members of society, as Marx argues, then it is clear that ideology is not just a “superstructure” causally generated by economic forces in some mysterious fashion, but must be understood in terms of the subjectively meaningful motivations which constitute both the “material” interests of those who dominate and the acquiescence of those who are dominated; that is, ideology must be understood as an attempt to legitimate a relationship of domination. According to Ricoeur, because of its neglect of motivated action, Marxist thought does not adequately explain the connection between the ruling class and the ruling ideas of a society, that is, the connection between domination and legitimation.

In contrast to this, in Weber’s sociology of domination careful attention is paid to the motivations involved in the maintenance of political domination, which normally depends not on raw force but on a generalized belief in the legitimacy of domination. The possible forms of this belief are typologized by Weber in terms of traditional, legal-rational, or charismatic criteria of legitimacy, as was noted above in chapter 4.1. Yet, as Ricoeur notes, there is always a “credibility gap” between the claim to legitimacy, on whatever grounds, and the belief in legitimacy, and this is where the Marxist concept of ideology can be reintroduced: “could we not say that the main function of a system of ideology is to reinforce the belief in the legitimacy of the given systems of authority in such a way that it meets the claim to legitimacy?” (Ricoeur 1991:315). The role of ideology in filling this credibility gap is what gives rise to dissimulation and distortion, because “any authority always claims more than what we can offer in terms of belief” (ibid.). But this kind of distortion is conceivable only if material praxis is already symbolically mediated and thus ideological to the core. There is no substratum of “real life” that is not already ideological.

In other words, it may well be that the ruling ideas of a society are the ideas of the ruling class, and so are prone to distortion in the interests of the rulers; but how is it, precisely, that these ideas are generated in the first place by the rulers’ own practical actions and so express their material interests? The connection between practical action and the ideas by which it is explained can be made only if even the most basic social actions are meaningful to the actor (e.g., a member of the ruling class) because they are motivated in terms of his or her meaningful interests, and thus are already symbolic.

In making this point, Ricoeur draws on Clifford Geertz’s essay “Ideology as a Cultural System” (in C. Geertz 1973), summarizing Geertz’s conclusions as follows:

Under the layer of distorting representation we find the layer of the systems of legitimation meeting the claim to legitimacy of the given system of authority. But under these systems of legitimation we discover the symbolic systems constitutive of action itself. As Geertz says, they provide a template or blueprint for the organization of social and psychological processes, as genetic systems provide such a template or blueprint for the organization of organic processes. A first corollary of this statement is that the initial opposition between real active life process and distorting representations is as such meaningless if distortion is not a pathological process grafted on the structure of action symbolically articulated. If action is not symbolic from the very beginning, then no
This is an important point because it renders futile any attempt to delve beneath ideology to a “real life” of nonsymbolic practical action that can be the subject matter of a “real, positive science” (Marx and Engels 1959:248); that is, a science which consists of causal explanations in terms of hypostatized economic “forces.” Ideology is able to distort only because it first legitimates and integrates the social order, symbolically constituting all patterns of social action, including the most basic actions involved in material production and exchange. This is true of the ancient imperial ideology discussed in the previous section, which is too often dismissed as an obfuscating superstructure to be stripped away in order to reveal the “real” economic motivations that underlie military conquest. More generally, it is true of patrimonialism as a political ideology expressed symbolically in terms of the “house of the father.”

The rationalization of such a symbol and of the ideology it expresses is therefore not simply a matter of changes in underlying economic “facts,” which give rise to new forms of consciousness and symbolism that reflect new “modes of production.” Ecological and technological factors do play a part as external constraints on social action, but economic relationships as social relationships are themselves symbolically constituted; thus a purely theoretical intellectual or religious rationalization that is perhaps only distantly affected by material conditions can nonetheless foster an ideological transformation that has a powerful effect on practical economic and political activity.

Such ideological rationalizations entail a reconfiguration of traditional structuring symbols through the exercise of imagination. Here we encounter what Ricoeur describes as the dialectic of ideology and utopia, which parallels what he discusses elsewhere as the dialectic of “participatory belonging” and “alienating distanciation” in the context of the debate between Gadamer’s hermeneutics of tradition and Habermas’s critique of ideology (see chapter 1.3 above). On the one hand, ideology is traditional in the sense that it preserves and conserves a given social order, whether at the level of the basic constitution of meaningful social action, or at the level of the legitimation of political authority, or at the level of the lies and distortions that preserve the power of a particular ruling group. On the other hand, the very same ideology, seen as a traditional symbolic system that underpins the existing social order, may contain within itself the seeds of a new order through the reinterpretation of traditional symbols in a utopian mode—through “the imaginary project of another kind of society, of another reality, another world” (Ricoeur 1991:319). If ideology is the vehicle of social integration, utopia is the vehicle of social disintegration and political subversion, because the projection of an imagined world places one elsewhere (i.e., “utopia” is “nowhere”), and thus places one at a critical distance from the previously unquestioned verities of the current social order. Hence utopian imagination serves to unmask the false pretensions of the existing system of legitimation, laying bare the “credibility gap” that the regnant ideology attempts to fill.

Utopia is not simply the opposite of ideology, however. It is the other face of ideology itself, for it exploits the possibility for rational critique and reform that is already inherent in any traditional symbolism. Ideology, precisely because it is a symbolic mediation of practical action, has already established a distance between the immediate practical reality of human interaction and the various ways in which that reality may be constituted as a meaningful and durable social order. Thus ideology, the vehicle of the “participatory belonging” of human beings within a symbolic tradition, has in itself an inherently utopian potential for “alienating distanciation” from that tradition. Conversely, utopian visions that originate as critiques of the current order can become so widely influential as to constitute a new socially integrating ideology. At the very least, most utopias, however bizarre and even pathological they may be, are fundamentally akin to ideologies in expressing a possible way of being-in-the-world. As Ricoeur puts it:

This interplay of ideology and utopia appears as an interplay of the two fundamental directions of the social imagination. The first tends toward integration, repetition, and a mirroring of the given order. The second tends toward disintegration because it is eccentric. But the one cannot work without the other. The most repetitive and reduplicative ideology, to the extent that it mediates the immediate social ties, the social-ethical substance, as Hegel would call it, introduces a gap, a distance, and consequently something potentially eccentric. And as regards utopia, its most erratic forms, so long as they move within “a sphere directed toward the human,” remain hopeless attempts to show what man fundamentally is when viewed in the clarity of utopian existence. [Ricoeur 1991:323]

This dual aspect of traditional symbolic systems, which are both ideological and utopian, both socially integrative and potentially subversive, is well illustrated in the ancient Near Eastern symbol of the “house of the father.” The symbolic system surrounding the
patriarchal household is the basis of possibly quite coercive and exploitative patrimonial regimes, because it justifies the claim to all-encompassing despotic authority on the part of the patriarchal lord. At the same time, the language of fatherhood and family creates the expectation that paternal benevolence and graciousness will be forthcoming in return for filial loyalty. The patriarchal ideology itself thereby provides the basis for a utopian critique of abuses of power, a critique that draws its force precisely from the value traditionally accorded to this aspect of the existing symbolic system. Indeed, this utopian aspect of intrahousehold love and loyalty is what permits patrimonialism to be sustained as an integrating ideology—it is ideology only because it is also utopia.

In the later forms of Israelite religion the utopian potential of the Bronze Age ideology of the “house of the father” was exploited by means of an increasing emphasis on the absolutely consistent benevolence, graciousness, and wisdom of the divine kinsman, Yahweh, on behalf of his human kindred, in contrast to the all-too-evident fickleness and selfishness of mundane patrimonial rulers (and in contrast to the comparable fickleness and selfishness of the divine patriarchs pictured in Bronze Age Canaanite and Mesopotamian mythology). The qualitative gap thus established between mundane rulers and the transcendent lord served to relativize and demystify all earthly rule.

Nonetheless, the ancient network of symbols surrounding the “house of the father,” however much it was reorganized in order to simplify radically the cosmic hierarchy of gods and men, was by no means simply erased. The utopian theoretical rationalization that characterized Israelite and then Jewish religious thought did not create a new cosmic vision ex nihilo; it did not reject the traditional symbolism but rather assumed it and employed it in expressing a new understanding of reality. Thus the traditional Near Eastern ideology of “genealogy” (or “sonship”) and “inheritance” remained fundamental for the constitution of relations of domination and subordination in a divine-human cosmos consisting of children and heirs of a single transcendent father. Dynastic succession and political innovation alike were still legitimated in terms of a genealogically based right of inheritance; that is, the ascendance to rule and to the possession of the ancestral patrimony of a “son” (either by descent or by divine adoption) who was chosen to be the executive agent of the divine father. Obviously, the monotheistic version of the “house of the father” could serve as just another distorting political ideology, however utopian in inspiration it might be. Still, the powerful symbol of the father’s house must be taken seriously in both its polytheistic and monotheistic manifestations, for it was not merely a mask for “real” relationships but was itself constitutive of the entire social order in the Bronze Age and Iron Age Levant.

The utopian rationalization of the father’s house in Israelite and Jewish religion will be treated at length in a future volume, in the context of the political events and socioeconomic changes of the first millennium B.C. In Part Two of the present volume the focus is on the constitutive role of the “house of the father” in the polytheistic societies of the Bronze Age Near East, and of the Levant in particular, in light of what we can discover about everyday life in ordinary patriarchal households. The chronological arrangement of these volumes therefore reflects the explicit contrast I have drawn between the symbolically constitut ed mode of social organization of the second millennium B.C., on the one hand, and the epoch-making rationalization that characterizes the first millennium B.C., on the other.

But this periodization needs further explanation and defense, for at first sight it may appear to derive from a dubious Hegelian division of human history into an inevitable teleological succession of historical epochs, characterized by increasingly rational modes of thought, and culminating in our present “enlightened” age. Hegel’s notion of progressive rationalization, which is underpinned by his ontology of Reason as a cosmic subject coming to self-realization, is not a view I wish to defend (on Hegel’s philosophy of history see Taylor 1975:389–427). Nevertheless, as I have noted above, there is a non-Hegelian concept of sociohistorical rationalization, articulated by Weber and his followers, which helps to make sense of the sweeping economic and social changes that we undeniably observe. Indeed, the unpredictable, historically contingent success of the sorts of symbolic innovations I have been discussing, which for various reasons became socially effective on a broad scale, permits us to speak of an “Axial Age” in the first millennium B.C. The challenge is to clarify the reasons for this contingent historical development after the fact in terms of the unpredictable but perhaps still explicable interplay of inner symbols and outer facts, that is, in terms of the meaningful patterns of action of embodied human agents.

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3 Compare Cleisthenes’ reform at the end of the sixth century B.C. in Athens, in which traditional kinship terms such as phylé (“tribe”) and demos were not jettisoned but were redefined in the service of a rationalized democratic constitution.
2. The Problem of Salvation and the “Axial Age”

In the sense in which it is used here, the term “rationalization” refers to empirical social phenomena that result from the conscious organization of social life and individual conduct according to some transcendent principle. More precisely, we are speaking of the increasing impact of depersonalized formal rationality in political and economic relations, which in turn depends on a theoretical rationalization and hierarchical systematization of existing substantive values, which thereby provide the transcendent principle to which formal rationality appeals (see the discussion in chapter 4.3 of “Types of Rationality and Processes of Rationalization”).

Furthermore, comparative historical study shows that the formal rationalization of social life is not a universal phenomenon but depends on a perception of a great gulf or tension between the transcendent and mundane levels of existence—a perception that is not found to the same degree in every human society. Where it does exist, awareness of this tension creates the “problem of salvation,” as Weber put it; that is, the necessity of remaking oneself and one’s society as a whole in line with a vision of transcendent reality. In the Western world, the solution to this problem has been sought in various kinds of theoretical rationalization stemming from classical Greek philosophy and its intellectual heirs, on the one hand, and in Judeo-Christian theologies, on the other, and in various combinations of the two.

It is striking, however, that the problem of salvation and the attendant rationalization of individual and social life seem to have emerged on a socially effective scale quite independently, and more-or-less contemporaneously, in different places during the first millennium B.C. This development occurred in different ways in Confucian China, in Brahmin and Buddhist India, and in Zoroastrian Iran, as well as in ancient Greece and Israel. Weber’s recognition of the broad historical effects of the problem of salvation, and of the various rationalizing attempts that have been made to solve it, led him to study the major world religions in some detail, and this program of research has persisted in various forms in subsequent scholarship. The philosopher Karl Jaspers (1953), for example, who was strongly influenced by Weber, has called the period from ca. 800–200 B.C. the “Axial Age” (Achensezeit) in world history, because it witnessed the emergence of revolutionary new understandings of human existence that have subsequently had profound effects in Asian and Western history.

Jaspers’s notion of a distinct Axial Age has been criticized as too Hegelian, however, by the political philosopher Eric Voegelin, among others. In his earlier work, Voegelin (1952:60; 1956:1–11) himself made use of Jaspers’s concept, but later he criticized Jaspers for limiting the Axial Age to a particular period, arguing instead that breakthroughs to more differentiated forms of consciousness have occurred at times that both predate and postdate Jaspers’s Axial Age (Voegelin 1974:2–6).

5 Voegelin, like Jaspers, takes as his philosophical starting-point the experience of human existence, although his language is more Platonic than that of other “existential” or hermeneutical thinkers of the twentieth century. Voegelin speaks of the qualitative “leap in being” in human consciousness that occurred in different ways in ancient Israel and Greece, and resulted in what Weber called the “de-divinization” or “disenchantment” (Entzauberung) of the mundane sphere, when the unified experience of reality that had been “compactly” symbolized in cosmological myth became symbolically differentiated. But whether symbolized compactly or in a more differentiated manner, human experience entails what Voegelin terms the “tension of existence” between mundane and transcendent poles of existence. Eugene Webb (1981) provides a useful summary of Voegelin’s thought, explaining that his key concept of symbolic differentiation refers to “the process by which one notices and develops an articulated, explicit consciousness of a previously ‘compact’ (comparatively implicit, unarticulated) field of experience” (ibid., p. 120). In ancient Greece, this took the form of “noetic” differentiation, which involves systematic rational inquiry; whereas in Israel (including Judaism and early Christianity), it took the form of “neumatic” differentiation, which involves the realization of the absolute distinction between the transcendent and mundane poles of existence. As Webb puts it (summarizing Voegelin): “What made both the noetic differentiation of classical Greece and the neumatic in Israel and early Christianity epochal events is that they involved a distinctively new realization of the difference between the transcendental pole of the tension of existence and the entire field of immanent reality. The substance of the noetic breakthrough was the clear realization that human conceptions of truth and goodness were grounded in a reference beyond themselves to the True and the Good as such. The substance of the neumatic breakthrough was the realization that the object of transcendental love and obedience was not one more figure within the cosmos but a supreme reality beyond all created beings” (ibid., p. 121). Like other hermeneutical thinkers, Voegelin does not accept either of these “breakthrough” symbolizations literally, in a realist or objectivist manner, but he is eager to retrieve the perennial truths about human existence that he believes they reveal.

6 Voegelin’s main example of a breakthrough earlier than 800 B.C. involves Moses; but it should be noted that the date of origin of “Mosaic” theological conceptions in ancient Israel is much debated in biblical scholarship, and they do not necessarily predate Jaspers’s Axial Age.

4 In addition to Ancient Judaism, Weber wrote about Islam, Confucianism, Hinduism, and Buddhism (see Käser 1988: 235ff. for references).
Voegelin emphasizes, furthermore, that such breakthroughs did not abolish cosmological symbols and other “compact” forms of consciousness. In place of cosmogenic myths featuring intraicosmic gods, for example, we find the teleological concept of a unilinear human history leading from a particular beginning to a climax in the present. But this widespread notion is simply another undifferentiated symbol that has persisted from the time of the earliest empires until now—indeed, it underlies much modern historiography and philosophy. The chief problem with the concept of an Axial Age, therefore, from Voegelin’s point of view, is that a chronologically delimited “axis” of human history implies a unilinear progression entailing an external unity or connection among epoch-making historical developments that were, in fact, rather widely separated in space and time, and are demonstrably unconnected.

Voegelin proceeds to solve the problem, as he sees it, that is created by the existence of multiple independent breakthroughs in human consciousness by positing an underlying unity of human experience that is rooted in common participation in Being. For Voegelin, the breakthroughs or “spiritual outbursts” are located in the universal experience of what Plato symbolized as the divine-human “In-Between” (*metaxy*), hence they are fundamentally mysterious and unpredictable, and can occur at any time. Accordingly, he believes that Jaspers’s emphasis on a particular Axial Age in the first millennium B.C. risks obscuring the common human participation, in all periods, in the universal experience of the “tension of existence” between the transcendent and the mundane, however that be symbolized. Voegelin notes that such periodizations have often led to the wrongful derogation and dismissal of earlier, mythological forms of consciousness, which tend to be placed within the framework of an overarching historical development of which the modern thinker is the culmination, a unilinear progression that is itself merely a “mythological” symbol.

Voegelin is certainly right to reject any interpretation of the Axial Age in terms of a unilinear and unitary progression of human history—the illusion, as Ricoeur puts it, of a “supreme plot” that lies behind every historical narrative. But even though Jaspers himself may have fostered this illusion, other Weberian scholars do not necessarily interpret the Axial Age in this way. For example, Eisenstadt (1986) uses this term simply as a convenient shorthand to describe empirically observable changes in social organization and in the symbolization of social order that were indeed revolutionary in their sociohistorical effects, and apparently irreversible, but did not occur as a result of some inner teleological necessity, and worked themselves out concretely in unpredictably different ways in different locales. The value of a term such as Axial Age is that it highlights the underlying similarities among these diverse historical phenomena—however coincidental and contingent they may be—in terms of a demonstrably restricted number of types of symbolization of social order that undergo common processes of theoretical rationalization (or “differentiation”).

It is true that the difference between Axial and pre-Axial intellectual and social phenomena need not always be analyzed chronologically, as Voegelin points out. Indeed, in terms of Weber’s sociology of domination, “Axial” simply refers to bureaucratic societies based on legal-rational domination, whereas “pre-Axial” refers to patrimonial societies based on traditional domination, wherever and whenever they exist (acknowledging that there are many empirical gradations in between these two polar ideal types). On the other hand, Axial societies have emerged, in all known cases, from earlier pre-Axial societies. There is an inner logical connection between pre-Axial and Axial symbolizations of order that is captured in Weber’s concept of rationalization, which points to a process of symbolic differentiation and systematization that is not teleologically inevitable or predictable and takes many forms, but must also be viewed in the framework of temporal development. For these symbolic transformations can take place only in the context of cultural continuity (without which it is meaningless to speak of “history” of any kind), as part of the process of learning and transmitting inherited understandings of the world. Rationalized symbolic systems are not created ex nihilo but necessarily incorporate earlier traditional symbolizations, as both Voegelin and Ricoeur have emphasized, for “a symbol never dies, it is only transformed” (Ricoeur 1976:64). Thus, even in his latest

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7 As Ricoeur puts it in *Time and Narrative* (1988:205f.): “We no longer seek the basis upon which the history of the world may be thought of as a completed whole . . . In fact, it is the very project of totalization that indicates the break between Hegel’s philosophy of history and every model of [historical] understanding . . . akin to the idea of narration and emplotment. Despite the seduction of the idea, the cunning of reason is not the peripeteia that can encompass

8 In this way it is possible to retain the focus on increasing symbolic differentiation that is found in earlier evolutionary models of religious development, which have long been
work on the “Ecumenic Age” ushered in by Alexander’s conquests, Voegelin (1974) himself continues to make a fundamental chronological division based on the differing symbolizations of social order that prevailed in various epochs.9

Moreover, by emphasizing the mysteriousness of breakthroughs to more differentiated forms of consciousness, Voegelin tends toward Platonism, despite his acknowledgment that we should not go beyond lived experience to hypostatize philosophical symbols such as “participation in Being,” the “divine-human In-Between,” or the “transcendental pole of the tension of existence,” as if they are external entities that determine human consciousness.10 This Platonism is evident in Voegelin’s own multivolume history of the ordering symbols of the Western world, entitled Order and History (1956–1974), in which he discusses at great length the use and reinterpretation of such symbols by intellectuals, but pays little attention to the concrete experiential ground of those symbols in embodied human existence. The suspicion arises that he sees human symbols of order as approximations to a transcendent Truth to which human beings have access—not, it is true, through their reason, as Plato would say, but through their lived experience—yet that embodied experience of the world in the end is regarded as merely a vehicle to transcendence and is not granted the ontological and analytical primacy it deserves. Weber, by contrast, sought to understand historical episodes of symbolic differentiation or “rationalization” in terms of the detailed analysis of mundane social and economic relationships. This approach is superior, in my view, to Voegelin’s brand of Platonic existentialism, for we must not reduce the irreducible dialectic of fact and symbol to only one dimension, be it the ideal or the material, if we hope to account for the kinds of intellectual breakthrough that highlighted the problem of salvation and offered solutions for it in terms of new ordering symbols. In the terms I have adopted in chapter 2.2, Voegelin’s “history of symbols” neglects its dialectical counterpart in the “prehistory of facts.”

For Voegelin, as for other twentieth-century thinkers in the hermeneutical tradition, the universal human capacity for symbolic differentiation—what Ricoeur (1977) has described as the ability to invent metaphors that accomplish the creation of meaning—is rooted in the common human capacity for language and the common experience of embodied existence, which entails some kind of basic experience of the “tension of existence.” But if we refuse simply to attribute the emergence of new conceptions of order to the mysterious eruption into human consciousness of an independent transcendent reality, the sociohistorical problem remains as to how and why certain creative symbolic innovations were transmitted beyond an intellectual elite and became socially effective on such a scale that we can speak of a revolutionary or “axial” change that inaugurates a new epoch.

Having said that, there is no point denying that the notion of a development from a pre-Axial to an Axial Age, and Weber’s concept of sociohistorical rationalization more generally, is a particular historical “plot” among other possible plots. It is not the only possible way of grasping a multitude of temporally sequenced human actions (or patterns of action) as a meaningful whole. Indeed, it is because other meaningful plots are possible that the emplotment of events in terms of a process of formal rationalization

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9 In spite of his criticism of Jaspers, Voegelin (1974:6) admits that there is a temporal aspect to breakthroughs in consciousness, noting that “advances from compact to differentiated consciousness ... bring the time-dimension in the flux of divine presence to attention.” In other words, symbolic differentiation occurs within a sequence of historical development and therefore provides a basis for historical periodization. Hence Voegelin continues to maintain a distinction between the “ecumenic empires” and the earlier “cosmological” societies of the ancient Near East which parallels Eisenstadt’s distinction between Axial and pre-Axial civilizations. Webb (1981:249f.) concisely summarizes this distinction as follows: “[The earlier cosmologically ordered societies] were ‘concrete’ societies in the sense that there was an organic relation between the symbolism of order and its substance in the life of actual communities of persons sharing common experiences, memories, and loyalties. The ecumenic empires, for their subject peoples, were virtually abstractions that attempted to impose themselves from above, with no recognizable roots in the history and culture of communities. In the earlier cases the imperial symbolism—the Egyptian Pharaoh or Mesopotamian emperor, for example, mediating between human society and the society of the gods—served to express men’s sense of the hierarchy of being and the preeminence of the transcendental order.”

10 Webb (1981:64f.) defends Voegelin on this score, noting that the terms he uses “are not the names of entities but symbols used to identify the poles of the experiential tension and to suggest their character in relation to the tension.”
should not be absolutized as the one “supreme plot” underlying an hypostatized unity of human history. I think that this plot of rationalization is “true” in the sense that it is plausible and enlightening, for it makes sense of all of the evidence in a way that other more reductionist approaches do not. But I make no claim for its absolute validity, acknowledging that it is only one possible way of selecting and arranging the evidence. It must be admitted that the changes of the Axial Age of the first millennium B.C. are seen as part of a linear historical progression with important cultural consequences only from a particular, historically situated point of view, namely, from the perspective of the modern Western world, in which formal rationality is regarded as a crucial criterion because of the substantive value we place on it in our egalitarian and individualistic society.

Still, we cannot do without some sort of encompassing historical plot. If a totalizing view of a single unitary human history is mistaken, then so is the notion of a total disjunction between historical periods that are characterized by supposedly incommensurable modes of thought, as in Michel Foucault’s (1972; 1977) postmodernist conception of a disconnected series of “truth-regimes” or “discursive formations.”

As a number of critics have pointed out, Foucault contradicts his own theoretical position in practice because there is an implicit narrative plot in his own historical studies—the tragic plot of the insidious spread of objectifying and dehumanizing power structures that make of modern liberalism, despite its high ideals, just another and more effective cage.

This “totalizing” plot emerges in spite of himself because Foucault, like any other historian, cannot stand outside the inherited symbolic tradition by which he makes sense of the world. His own employment of events plainly derives from the value he places on individual autonomy, a value which he inherits from the post-Enlightenment cultural tradition (or “truth-regime”) of which he is a part. As Charles Taylor (1985b:182) puts it: “The reality of history is mixed and messy. The problem is that Foucault tidies it up too much, makes it into a series of hermetically sealed, monolithic truth-regimes, a picture which is as far from reality as the blandest Whig perspective of smoothly broadening freedom.” Indeed, as Clifford Geertz (1978:6) says, in Foucault’s own work “we seem to be faced with a kind of Whig history in reverse—a history, in spite of itself, of The Rise of Unfreedom.” In a similar vein, Habermas points out that Foucault is unable to apply radical historicism consistently in his own historical studies:

the first glance in any one of Foucault’s books teaches us that even the radical historicist can only explain the technologies of power and practices of domination by comparing them with one another—and by no means by taking any single one as a totality on its own. In doing so, one inevitably connects the viewpoints under which the comparison is proposed with his own hermeneutic point of departure. This can be seen in, among other things, the fact that Foucault cannot avoid dividing up historical epochs through implicit reference to the present. [Habermas 1987:277]

Equally mistaken, however, is the notion of a total lack of change in the relative preponderance and hence the social effectiveness of the various possible modes of human rationality. This notion underlies the objectivist and formalist view of Enlightenment modernism, which anachronistically projects our own “bourgeois” mode of instrumental reason into the distant past, thereby absolutizing the modern symbolization of the cosmos and the atomistic values which it expresses. If, on the one hand, we cannot settle for a postmodernist historical account that consists of a series of isolated vignettes whose connecting thread is simply the anonymous deus ex machina called “power,” neither can we settle for the comforting modernist illusion that there is nothing new under the sun. This illusion yields an antiquarian history that has nothing to teach us because it is simply a voluminous catalogue of meaningless variations on a timeless structure of familiar formal rationality which is supposedly based in “real” economic and material facts. On the contrary, our understanding of human action in time has, as Ricoeur has said, an irreducibly narratival dimension because it is rooted in our own

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11 Jürgen Habermas (1987:253) concisely summarizes Foucault’s concept of the “archaeology of knowledge” (a “transcendental historicism” inherited from Nietzsche) as follows: “the space of history is seamlessly filled by the absolutely contingent occurrence of the disordered flaring up and passing away of new formations of discourse. No place is left for any overarching meaning in this chaotic multitude of past totalities of discourse. The transcendental historicist looks as if into a kaleidoscope . . . Under the stoic gaze of the archeologist, history hardens into an iceberg covered with the crystalline forms of arbitrary formations of discourse. But since the autonomy proper to a totality without origin accrues to every single one of these formations, the only job left for the historian is that of the genealogist who explains the accidental provenance of these bizarre shapes from the hollow forms of bordering formations, that is, from the proximate circumstances. Under the cynical gaze of the genealogist, the iceberg begins to move: Discourse formations are displaced and re-grouped, they undulate back and forth. The genealogist explains this to-and-fro movement with the help of countless events and a single hypothesis—the only thing that lasts is power, which appears with ever new masks in the change of anonymous processes of overpowering . . .”
meaningful experience of a temporal lifeworld, in which we constantly exercise symbolic imagination in order to project ourselves from an inherited past into an undetermined future. Indeed, our being-in-the-world is meaningful for us precisely because we already belong to a narrated symbolic tradition—a tradition which critical historical study can illuminate and explicate, helping us to “follow the story,” without requiring us to abandon the imaginative and narrative dimension that gives our story its meaning.
Chapter 6. Before the Bourgeoisie

We cannot discuss the economic and demographic features of patriarchal households in the Bronze and Iron Age Levant without first sketching a picture of urban life in the ancient Mediterranean world and considering whether the urban mode of life differed qualitatively from rural life in agricultural villages and pastoral encampments. This is an area in which an “interpretive” or hermeneutical sociohistorical approach produces quite different results from a nonhermeneutical functionalist approach. At one level, of course, the comparison of urban and rural ways of life is an empirical issue. But the available evidence, both archaeological and textual, is sufficiently ambiguous that one’s assumptions about the nature of urbanism have a powerful effect on one’s reconstruction of this aspect of ancient society.

In particular, the widely accepted view of a fundamental urban-rural dichotomy in the ancient Mediterranean world is more the result of an a priori theoretical assumption, rooted in modern functionalist logic, than the result of direct evidence. Here is an example of the way in which we tend to project our modern mode of “bourgeois” rationality into the distant past, rather than taking seriously the native understanding of economic and social relations. It is because such relations are symbolically constituted, as I have argued above, that we should not insist that the urban-rural dichotomy was a basic structural feature of ancient society when there is no evidence that the people involved recognized such a dichotomy. In other words, this dichotomy is not an ahistorical natural phenomenon but is itself the historically contingent product of particular symbolic innovations.

1. The Urban-Rural Dichotomy

The notion of a qualitative dichotomy between “urban” and “rural” modes of life is deeply entrenched in modern historical and archaeological scholarship. But there is reason to doubt that this dichotomy is generally applicable in the ancient Mediterranean world and in the Bronze Age Near East, in particular. In my opinion, the available archaeological and textual evidence indicates that most city-dwellers were not full-time specialists engaged in nonagrarian pursuits as traders, artisans, priests, administrators, or soldiers. Rather, they were farmers who, like their counterparts in smaller towns and villages, subsisted primarily by means of agricultural activities undertaken by themselves or by their servants, clients, or tenants. I will demonstrate this in some detail below in Part Two for the Late Bronze Age city of Ugarit, which is generally regarded as the epitome of urbanism in the Bronze Age Levant. If the urban-rural dichotomy can be dismantled for the kingdom of Ugarit, then it may be argued a fortiori that this dichotomy is inapplicable to many other less well documented polities in the region.

The relationship between town and country has been discussed in considerable detail with respect to classical Greek and Roman evidence, which therefore provides a useful basis for comparison with the earlier Near Eastern situation. Graeco-Roman cities were not all alike, of course. Imperial Athens, in particular, like Rome later, was unusually large and had a substantial nonagricultural population. But even when these exceptions are taken into account, Weber was correct to say that:

Historically, the relation of the city to agriculture has in no way been unambiguous or simple. There were and are “agrarian cities” (Ackerbürgerstädte), which as market centers and seats of the typically urban trades are sharply differentiated from the average village, but in which a broad stratum of the burghers produces food for their own consumption and even for the market. . . . And the further to the south or back toward Antiquity one turns, the more frequent becomes the presence of large amounts of farmland within the territory (Weichbild) of the towns. If today we are quite correct in regarding the typical “townsman” as a man who does not grow his own food, the contrary was originally true for the majority of typical cities (poleis) of Antiquity. [Weber 1978:1217f.]

This view is echoed by Moses Finley (1985:97), who defends “the vague but sure proposition that most people in the ancient world lived off the land.” He notes that commercial cities which depended on trade were special cases, observing that “ancient cities in the great majority counted farmers . . . as the core of their citizenry” (ibid., p. 131). Huge “parasite cities” like Rome, to which tribute flowed from distant places, were even more rare. The high cost of transportation by land prevented all but a few urban centers—those with ready access to waterborne
shipping—from importing bulky commodities over great distances. Most towns therefore depended on their immediate hinterlands for food; moreover, most urban households did not purchase staple items in the marketplace but were individually self-sufficient in basic necessities. It is not surprising to Finley, therefore, that ancient Greek writers report cases in which surprise attacks on cities—even commercial cities that were heavily involved in trade—found only a few people within the walls because the rest were outside working in the fields (see Finley 1983:22 and references therein; also Osborne 1985:119 n.1, where several other texts are cited). Finley (1983:4f.) argues that the ambiguity of the Greek term polis itself, which denotes both the city and its rural territory, reflects the absence of a fundamental conceptual or structural distinction between town and country in ancient Greece.

More controversially, Finley argues that the manufacture of goods for export (as opposed to local consumption) was usually a negligible source of income, even for most of the commercial cities that served as clearing-houses for other kinds of goods. He acknowledges that a rudimentary money economy existed in classical antiquity, but he points out that although true market exchange occurred in which prices were set by supply and demand, markets were poorly integrated and financial arrangements were primitive, thereby hampering commerce. By and large, the household self-sufficiency engendered by poor transportation facilities and inflexible markets served as a brake on economic development. Finley (1985:139) maintains, therefore, that those cities which needed to import food paid for it, not with a wide array of manufactured items, as in the diversified economies of the late-medieval and modern periods, but with their services as ports-of-call (e.g., Hellenistic Rhodes) or as clearing-houses, or with a few high-value products, such as metals mined in their territory—or else, like imperial Rome and fifth-century Athens, they received food in the form of tribute from subject provinces.

Finley accordingly emphasizes, following Weber (and before him the economic historians Karl Bücher and Werner Sombart), the basic distinction between the “ancient” city and the “medieval” and later European type of city, which in economic terms is a distinction between “consumer cities” and “producer cities.” Of course, goods were produced in the ancient city, but these were food and manufactured items created by small-scale producers that were intended mainly for local consumption. In other words, the ancient city, unlike the medieval and modern Western city, was primarily a center of consumption and did not itself generate wealth through large-scale export-oriented manufacture. Thus Finley, in his treatment of ancient urbanism, as in his treatment of other aspects of Graeco-Roman society and economy, rejects the “formalist” approach and adopts the “substantivist” approach to ancient economies that was popularized by the economic historian Karl Polanyi (see the discussion of Polanyi above in chapter 4.4).1

Furthermore, as Finley (1985:191f.) remarks, it was only in modern times that the analytical dichotomy between urban and rural sectors, which separates the manufacturing city from the agricultural countryside, took root. This socioeconomic separation (which has in fact existed, in Western Europe at least, since the late medieval period) was taken for granted by Adam Smith in The Wealth of Nations, published in 1776, and by later economic theorists, including Karl Marx, who drew heavily on British economic theory.2 Both Smith and Marx assumed that the urban-rural dichotomy was a universal phenomenon in “civilized” societies. The difference between them is that Smith viewed the contrasting economic roles of city and countryside positively, arguing that the division of labor they embodied was the engine of economic exchange and prosperity, whereas Marx and his followers viewed the dichotomy negatively, seeing in it a clash of interests.

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1 See Whittaker 1990 for an updated defense of the “consumer city” model, and a rejection of the notion of an ancient urban-rural dichotomy, on the basis of extensive archaeological evidence published since Finley wrote on the subject in the 1970s. Whittaker summarizes this model in terms of four essential features: “1. The ancient city unlike the mediaeval had no separation of function between town and country (Bücher); 2. The ancient town relied on the products of outside agricultural labor for its existence (Sombart); 3. The major income for urban consumers came from rural rents, not from commercial enterprises (Weber); 4. The commodity production of towns was essentially petty (Finley)” (p. 110). But note the qualifications to Finley’s model proposed by Robin Osborne and Andrew Wallace-Hadrill in their contributions to a collection of essays entitled City and Country in the Ancient World (ed. Rich and Wallace-Hadrill 1991).

2 As Finley notes, Smith (1970:479) begins Book III of The Wealth of Nations with the words: “The great commerce of every civilized society is that carried on between the inhabitants of the town and those of the country. It consists in the exchange of rude for manufactured produce . . . The country supplies the town with the means of subsistence and the materials of manufacture. The town repays this supply by sending back a part of the manufactured produce to the inhabitants of the country. . . . The gains of both are mutual and reciprocal, and the division of labour is in this, as in all other cases, advantageous to all the different persons employed in the various occupations into which it is subdivided.”
between peasant producers and urban elites. Nonetheless, the perennial division of town and country into two opposing socioeconomic sectors is presupposed as a fundamental dynamic in Marx’s theory of social evolution, just as it is in Smith’s economic theory.3

Finley’s influential attack on this dichotomy with respect to ancient Mediterranean urbanism was formed in reaction to the market-oriented approach championed by a number of twentieth-century historians and archaeologists, who saw the Greek city-states, for example, as far less agrarian and economically more developed than did Bücher, Sombart, or Weber, on the analogy of the later European trading centers of the Hanseatic league. This formalist approach persists to this day, of course, and Finley’s substantivist approach, which emphasizes the degree to which ancient economic behavior was embedded within traditional social institutions and relationships, has itself attracted criticism. It is argued, for example, that the traditional Greek system of self-sufficient households began to break down over the course of the fifth century B.C., at least in Athens (see Davies 1992 for a review of this debate and a summary of recent developments). But Finley himself stressed the fact that imperial Athens (which happens to be much better documented than the other Greek cities) was not typical but is an exceptional case. Such exceptions, in his view, do not vitiate the general conclusion, which he based on diverse lines of evidence, that there was no strict urban-rural dichotomy in classical antiquity, and that there is thus a fundamental typological difference between ancient and medieval European cities.

Although Finley restricts his attention to Weber’s contrast between ancient (i.e., Graeco-Roman) and medieval European cities, Weber (1978:1233) himself pointed out that, according to his typology, the cities of classical antiquity shared many of the characteristics of later Islamic (and earlier Near Eastern) cities. Both the Graeco-Roman and Islamic types were “consumer cities”; moreover, both were primarily organized around local urban “clans” (fictive or not) or cultic commensal associations. Weber acknowledges that the classical Greek polis must be distinguished from the Islamic city, in certain respects, because of the emergence in some poleis (notably Athens) of various kinds of formalized and egalitarian citywide organization which presage the peculiarly urban institutions of medieval and modern Europe. By contrast, territorially defined citywide institutions, to which “citizens” attached themselves as individuals rather than as kin-groups, never emerged in the Islamic city, which retained its personalized clan-based mode of organization until quite recently (Weber 1978:1244ff.).

Despite this difference, however, the ancient Graeco-Roman city is much closer, in Weber’s typology, to the Islamic city than to the medieval and later Western form of urbanism with which economic historians have been most familiar. The result of Weber’s analysis is to highlight the peculiarity of Western urbanism as it has developed since the medieval period, over against classical and “Oriental” varieties of urbanism. Indeed, Weber saw in the “producer cities” of medieval Europe the wellspring of the social and economic innovations characteristic of modern Western capitalism.

Before turning to the ancient Near East, then, it will be useful for comparative purposes to review briefly and selectively some of the evidence concerning precapitalist urban households and neighborhoods in the Mediterranean region, from both the classical and Islamic worlds—not only from an architectural and ethnographic perspective, but also from the perspective of historical demography, with its formal methods of analysis based on mathematically derived demographic models. These more recent and in some ways better documented cases can shed light on the urban experience in Late Bronze Age Ugarit and other ancient Near Eastern cities, to the extent that we are dealing here with similar kinds of clan-based “consumer” cities, be they Islamic, European, or ancient Near Eastern.

In the Bronze Age Near East, in fact, we might expect to find relatively pure examples of this ideal urban type, because Bronze Age society was pre-Axial, without a deeply rooted tradition of more rationalized modes of social and economic organization. But the degree to which pre-Axial urban phenomena persisted even after the Axial Age is testimony to the tenacity of the sort of rationality that prizes above all the substantive personal relationships of friendship or dependence that exist among persons who construe their closeness in terms of kinship. Far less common historically is that degree of formal rationalization of urban life, characterized by the overriding importance of universal rules, applied

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3 In the first volume of Capital, Marx (1977:472) states that: “The foundation of every division of labour which has attained a certain degree of development, and has been brought about by the exchange of commodities, is the separation of town from country. One might well say that the whole economic history of society is summed up in the movement of this antithesis.” The two-sector model of Bronze Age Near Eastern society (discussed below in chapter 9.2), which contrasts a specialized urban class with rural agrarian producers, is based on this tenet of Marxist social theory.
atomistically and without regard to persons, which ultimately produced the urban-rural dichotomy so characteristic of the modern world.

2. Houses and Households in Classical Greece and Italy

The evidence of domestic living arrangements, both archaeological and textual, is more abundant for the classical period in Greece (fifth–fourth centuries B.C.) than for earlier periods, or for subsequent periods until we reach the Roman era. This evidence is useful for comparative purposes because it shows that even in classical Greece—“the birthplace of Western civilization”—urban households had much in common with those in later Islamic cities of the Mediterranean region, as well as with those found earlier in the Levant and in the ancient Near East in general. Michael Jameson (1990) has summarized the archaeological and textual evidence concerning house design and household structure in the Greek city-states of the fifth and fourth centuries B.C. Orthogonal town plans were beginning to be employed in this period and they became the norm in the later Hellenistic and Roman periods, indicating that the city government played a much greater role in determining the overall urban layout than was the case in most Islamic cities or in the ancient Near East. It is probable that urban neighborhoods in this new type of city were not defined by kinship to the same extent as in “traditional” Mediterranean cities (both before and after the Graeco-Roman period) characterized by irregular streets and blind alleys; or, if they were, kinship organization was not reflected in city plans that were imposed mechanically from above rather than being the cumulative result of many individual actions.

E. J. Owens (1991), in a broader survey of Graeco-Roman urbanism, has emphasized the role of Greek colonization and the founding of new poleis in stimulating the widespread adoption of the so-called Hippodamian grid system (named after Hippodamos of Miletos, the putative originator of orthogonal town-planning). This is plausible in light of the fact that many long-established cities, including Athens and Rome, remained largely irregular in plan, with narrow streets winding between irregular blocks of houses and public buildings, in contrast to newly founded or rebuilt cities of the period. In particular, it seems that within the political context of the classical polis, as its distinctively abstract and egalitarian constitutional basis was being developed, new theoretical and aesthetic ideals converged with the practical political necessity of a uniform and equitable distribution of property, especially in newly founded cities. Thus geometrical street patterns were laid out to create elongated rectangular insulae measuring 35 × 200 m or 35 × 300 m, reflecting the same kind of regular land division as was done within the agricultural territory of a newly settled polis (see Boyd and Jameson 1981). Such grid patterns often ignored the natural topography of the town site, and were criticized by contemporaries for their disadvantages with respect to military defense and the lack of shade from the sun, as compared to older irregular towns with narrow winding streets. Clearly, the growing prestige of the regular town plan was not just a matter of the functional requirements of urban life being given special attention in a period when many new cities were being established, because functionally well adapted cities have grown up in a highly irregular fashion both before and after the Graeco-Roman period. Rather, it is a matter of a conceptual shift, because the city as a whole, abstractly conceived, had become the fundamental planning unit, rather than the individual houses or neighborhoods from which earlier Greek cities were amalgamated. As Owens says,

Greek town planning originated as a practical response to the problems of establishing new towns or occasionally, if the opportunity arose, redesigning an existing community. It cannot be totally fortuitous, however, that the beginning of regular planning coincides virtually with the formative period in the evolution of the polis as a political, social and religious institution. The nature of early Greek planning both reflects and caters for the incipient polis by clearly defining and allocating land for its component parts. At its simplest, early Greek planning was a rationalization of the already emerging cities of Greece. [Owens 1991:50]

Regardless of the presence or absence of a Hippodamian grid system, most individual Greek dwellings were examples of the traditional Mediterranean courtyard house, as Michael Jameson points out:

The Classical Greek house shows a common, underlying conception even though the actual working out may vary considerably, from town to town and from house to house. The house is a closed unit, immediately adjacent to a street but with its interior invisible from it, and sharing party walls with adjacent houses from which it is equally invisible. It consists of a group of adjoining rectangular rooms opening off a rectangular courtyard. [Jameson 1990:97]
This description could be applied equally well to the houses of Late Bronze Age Ugarit (see chapter 13), and to the dwellings found in many medieval Mediterranean cities. In classical Greece, as elsewhere, the inner courtyard was the main locus of activity and most of the rooms opened directly onto it (see figure 1 above). The courtyard was also the main source of light because the house had few windows. If a Greek house had a well, it was located in the courtyard, where the members of the household cooked, ate, worked, and relaxed. In spite of the overall grid pattern of certain classical cities, therefore, the design of individual houses was quite similar to that observed in other periods in the Mediterranean region.

It is true that contemporary textual evidence has generally been interpreted as indicating that in imperial Athens, and presumably elsewhere in the Greek world, extended-family household organization had largely given way to nuclear-family households incorporating substantial numbers of foreign slaves, whose labor replaced that of the kinfolk who, in other settings, would have pooled their resources as a core-sident group. But in architectural terms, at least, all of the features of the typical Greek house that Jameson describes are also found in more “patrimonial” settings.

A noteworthy result of the archaeological investigation of classical Greek cities is the discovery of evidence showing that a wide range of productive activities was carried out within ordinary private houses. Manufacturing and industrial activities were not isolated in specialized structures but in many cases were performed by families in their own homes (Morris 1991:38f.). The household (oikos) was the primary economic unit; thus private houses were centers of production, not just for craftwork but also for agriculture. At two sites in particular—Olynthos, in northern Greece, and Halieis, in the Argolid—urban agricultural activity is well attested archaeologically, and presumably these towns were not unique (Morris

4 Note, however, that Thomas Gallant (1991:15–27) challenges the prevailing view that patrilocal postmarriage residence was rare in classical Greece, citing both textual and archaeological evidence. For example, forensic speeches indicate or imply postmarriage residence in the natal household by at least one child in 74% of the 52 known legal cases from ancient Greece. Furthermore, Gallant observes that: “The classical town site of Olynthos in northern Greece contained roughly the same number of houses over a period of 120 years. There does not seem to have been constant building of houses for newlyweds. Instead, we see in a number of cases that the paternal household was physically divided into smaller units...” (p. 21). Although Greek textual data show that the overall mean household size was relatively small (ca. 4–5 persons), this is consistent with patrilocal residence if we take account of the effect of ancient mortality rates throughout the household lifecycle. This phenomenon is treated in detail below in chapters 7 and 8.
In the Hellenistic period many new cities were founded in the Near East by hellenizing conquerors and colonists. These settlements had both a military and a propagandistic function, and both purposes were well served by the distinctive Hippodamian grid system which had earlier won favor in the classical Greek world. But detailed archaeological evidence concerning individual houses and local neighborhood organization in the Mediterranean region, as opposed to overall town plan, is scarce until we reach the Roman period. Fortunately, there has been considerable discussion of Roman domestic architecture in recent years, especially in relation to the unparalleled wealth of data from Pompeii and Herculaneum, which were famously buried in volcanic ash in A.D. 79. The houses in these towns provide important insights into domestic arrangements in Roman Italy (for a recent review of publications on this topic, see Parslow 1999). Of particular note is the detailed study by Andrew Wallace-Hadrill (1994), who discusses “the social structure of the Roman house” in relation to the many houses unearthed at Pompeii and at Herculaneum. He correlates the appearance and function of various houses and of their constituent parts to differences in social status. Most important for our purposes, however, is his statistical analysis of the distribution of house sizes (pp. 72–82), which reveals what is also evident visually, namely, “the extreme variation in sizes of units and the intricate jigsaw they form, an interlocking pattern of large and small units within virtually every block” (p. 75). This pattern is found in many other traditional Mediterranean towns, including Late Bronze Age Ugarit, and is in contrast to the much greater uniformity in house sizes in orthogonally planned classical Greek cities, whose oddity (presumably due to a consciously egalitarian division of urban space among the citizens of a polis) is thereby accentuated. There is some variation among insulae in Pompeii and Herculaneum, such that in some areas large houses cluster together or small houses cluster together, “but even these minor local variations are not enough to disrupt the underlying pattern of mixture of large and small” (p. 78).

5 The Greek evidence can be compared to textual evidence from Late Bronze Age Ugarit which suggests that a typical farmstead there also had a defensive tower—a rural estate is called, literally, a “tower” (dimtu in Akkadian). The building attached to the tower was probably a courtyard house similar in design to an urban house, as was the case later in Greece, although no Ugaritan rural dwellings have yet been excavated. See Heltzer 1979a on the similarity between the dimtu of Ugarit and the Greek pyrgos; compare also the fortified rural settlements of the medieval Islamic period in the Near East, which were known in Arabic as qaṣir, “castles” (Conrad 1981), however humble they might be.

6 Owens (1991:74f.) notes that: “In the first place, grid planning remained both the most convenient and the quickest method of establishing a new city in potentially hostile territory. However, the barracks-like uniformity of many of the Greek colonies of the Hellenistic east emphasized the essentially military nature of many of the new foundations. Many of the new towns achieved an extremely standardized plan, often with simple mathematical ratios as the basis of their design. The repetitive uniformity of such towns must have been a stark reminder of the new political conditions which prevailed.”
Wallace-Hadrill discusses in some detail the ambiguity of the architectural evidence, but he still thinks it plausible to suggest that the distribution of house sizes found in Pompeii and Herculaneum, and the fact that a high proportion of the total living space was comprised by relatively large houses, reflects the prevalence of what Philippe Ariès in his book *Centuries of Childhood* (1962) has called the “big house” characterized by the “promiscuity” of a “crowded social life.” Wallace-Hadrill envisions several hundred leading families in a town like Pompeii “surrounded by a great penumbra of persons of varied status, much harder to define as family groups” (p. 116). Presumably, in addition to slaves and family members coresident in the same house, the smaller houses which abutted larger houses in an interlocking pattern were occupied mainly by clients, tenants, freedmen, and dependents of various sorts.

Roger Ling’s detailed study of *The Insula of the Menander at Pompeii* (1997) supports this idea. Ling describes a process of continual renovation within this typical *insula* over a period of almost three hundred years, from its founding until the destruction of Pompeii in A.D. 79, including the addition of upper floors and the blocking or opening of doors and windows. He remarks upon the fluidity of the boundaries between individual houses as access to various rooms was redirected (p. 239), producing the interlocking jigsaw pattern that Wallace-Hadrill has also noted. Equally remarkable is the fact that the *insula* was dominated from the time it was built by one large house, the “Casa del Menandro,” which occupied more than half of the *insula*. Over time this house had been enlarged at the expense of the neighboring houses, raising the possibility that the owners of the Casa del Menandro owned the entire *insula*—especially in view of the fact that ownership of a whole *insula* by one person is attested elsewhere in Pompeii—or at least that they owned a large part of it (p. 240).

In any case, the economic basis of the Casa del Menandro is not in doubt: “that one of its sources [of wealth] was agriculture is suggested by the stableyard area at the rear of the property, with its impressive number of staff rooms and storerooms, with evidence of stabling for four animals, and with finds which included the remains of a cart, numerous amphorae, and an assortment of iron farm tools. Our proprietor presumably held an estate outside the city” (Ling 1997:252). This “urban farmhouse” phenomenon is closely paralleled in Late Bronze Age Ugarit, as I will show in chapter 13 below. Ling suggests that wine was the chief product of this estate, as it was for other leading families in Pompeii, since Pompeian wine was widely distributed. The large scale of the operation is indicated by his estimate that sixteen workers could have slept in the stableyard area, and that the total staff resident in the house might have numbered thirty. Less obvious, however, is the relationship between the owners of the Casa del Menandro and the other inhabitants of the *insula* who lived in the much smaller adjoining houses. It appears that these neighbors were largely small craftsmen and shopkeepers, whose homes “clustered round the residence of one of the decurional class” (ibid., p. 253). If so, it is likely that they were politically if not economically dependent on their high-ranking neighbor, as Robinson (1997) has argued, whether or not this patron was their landlord.

During the later medieval period, this sort of patron-client organization of urban neighborhoods continued to be a well-attested feature of the Mediterranean world, both Muslim and Christian. Jacques Heers has described what he calls the “clan” organization of medieval European cities, especially in Italy, noting that

> even outside their courtyards, great families kept their main houses and those of their protégés all close together in the same quarter. This is true, at least, of all the Mediterranean countries, on both shores, right up to Constantinople [Byzantium] where, in the Middle Ages, the ‘quarter’ was in fact only a single, very large house, surrounded by the dependants’ houses. The city had a considerable number of these ‘quarters,’ each of which quite simply bore the name of the head or ancestor of its great family. [Heers 1977: 146ff.]

We need not imagine that this mode of neighborhood organization was as highly developed in first-century Pompeii as it later became in Italy and elsewhere, given the existence in the Roman empire of formal institutions of urban governance which cut across clan and family ties. Indeed, the most significant difference between Mediterranean cities of the Graeco-Roman period as compared to the medieval period may be their relative lack of urban “clans,” indicating a greater rationalization of traditional patrimonial relationships. Certainly this seems to have been the case in the apparently more egalitarian classical Greek *poleis*. Furthermore, it is debatable whether Italian families of the Roman period lived together in an “extended” as opposed to a “nuclear” fashion, for a “big house” in Pompeii or Herculaneum might have consisted of a nuclear family with its slaves and other nonfamily dependents rather than multiple related conjugal couples and their children. In other words, in circumstances where slavery was common, the large “slave family” was a viable
substitute for the traditional Mediterranean extended family.7 But this simply serves to highlight the basic structural parallel between the “big households” of Pompeii and similar domestic “housefuls” in earlier and later Mediterranean urban contexts, however these households were constituted in terms of biological kinship. This social-structural parallel mirrors the striking parallel one may observe between the physical arrangement of domestic architecture in these well-preserved towns of Roman Italy, characterized by an interlocking pattern of houses of disparate sizes, and the layout of similar urban neighborhoods in the ancient Near East—and in the “Islamic city,” to which we will turn next.

3. Pragmatic Space and Local Logic in the Islamic City

Although it is more distant in time, the Islamic city is a better analogue for Bronze Age Near Eastern cities such as Ugarit than is the Graeco-Roman city of the type we have been discussing. Thus the urban geographer Eugen Wirth (1975) has argued that most premodern Islamic cities were very similar to ancient Near Eastern cities in terms of their spatial organization and economic functions. It is true that, broadly speaking, both the Islamic and classical worlds lack the strict urban-rural dichotomy characteristic of modern times, and for that reason they share an emphasis on household self-sufficiency, which in turn fosters large households. In terms of household composition and neighborhood organization, however, the kin-oriented “patrimonial” Islamic city has more in common with its ancient Near Eastern counterpart, in my view. This is reflected in the archaeological evidence. As I shall argue below in chapter 13, the urban dwellings of Late Bronze Age Ugarit and of many other Bronze Age Near Eastern cities exhibit features which indicate that many of them were occupied not by nuclear families but by relatively large “joint-family” households, whose main source of sustenance was agriculture conducted on landholdings outside the city (note that the term “joint family” is preferable to “extended family” because the latter term is ambiguous and has been defined in several different ways; see Laslett 1972:28–32; Kertzer 1991:158f.). The kind of joint-family household in view here is a patriarchal, patrilineal, and patrilocal household of the type which has traditionally been the basic socioeconomic unit of both towns and villages in the Near East (and elsewhere, e.g., China), and so is well known in the ethnographic literature. Such a household typically consists of a conjugal couple and their unmarried children, together with their married sons and their wives and children, as well as other unmarried or dependent paternal kinfolk and servants.8

It is important to note that at certain stages of the household lifecycle, a theoretically joint household will be “nuclear,” consisting only of a conjugal couple and their immature children, even though the full three-generation structure remains the ideal. Indeed, under preindustrial conditions, typical birth and mortality patterns ensure that a minority of all families will actually be “joint” at any one time (this phenomenon is discussed below in chapter 7.2 in terms of formal demographic models). In addition, the poorest households, lacking sufficient wealth in land and livestock, cannot sustain the joint-family structure, and their members frequently attach themselves to wealthier households as individual servants or as nuclear-family client households, usually on the basis of some kinship relationship, real or fictional. For these reasons, the classic joint-family household is often numerically in the minority though it remains the conceptual norm and serves as the physical and social center of a penumbra of lesser subhouseholds. It is therefore possible to argue that a preference for joint-family households (and for kin-based coresidence in general) did exist in Ugarit and in other ancient cities, despite the fact that archaeological and textual evidence indicates that at any one time many—even a majority—of dwellings were actually occupied by nuclear families; that is, families in the nuclear phase of the household lifecycle.

The longstanding preference for joint-family households and other forms of kin-based coresidence is attributable in part to economic factors. Without denying the tenacity of historically contingent cultural traditions, which may dictate particular kinds of marriage and residence practices without regard to economic efficiency, it seems likely that the joint-family household has flourished as a socioeconomic

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7 But note that Yan Thomas (in Burguière et al. 1996: 228–69) uses both textual and archaeological evidence to argue that “polynuclear” joint-family households were much more common in the Roman world than is usually thought. He maintains that nuclear-family households were typical only of the wealthy urban elite, who are disproportionately represented in surviving literary sources (see the review of scholarship on this issue in Dixon 1992:3ff.).

8 This type of household, which has been widespread in the Middle East, is called a zaʿārīla in Palestinian Arabic. For a discussion of family and household types in a Levantine archaeological context, see Stager 1985a:18–20, 29 n. 9, and the ethnographic literature cited therein (e.g., Lutfiyya 1966:142f.).
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unit in Mediterranean and Near Eastern agrarian settings because it serves to pool scarce resources (e.g., land, animals, and labor), thereby spreading among a larger group the risk of individual misfortune that is endemic in a region of uncertain climate and marginal land. At the same time, the joint family supplies a sufficiently large workforce to cultivate the household’s hereditary landholdings, permitting larger-scale and more efficient production. Moreover, the patriarchal authority exercised by the head of the household provides an effective means of managing the production and distribution of goods within a sizable coresident group. Horticulture, in particular, has long been associated with joint-family households in the Mediterranean region because of the initial heavy investment of labor and capital and the residential stability and long-term commitment of resources that are required to cultivate olive trees and grapevines, which are harvested decades or generations after they have been planted (see Boardman 1977:189; Stager 1985b:177f.).

The agricultural and horticultural joint-family households posited for the city of Ugarit are comparable to urban households found in traditional Islamic cities in the Mediterranean region, which are well known both ethnographically and architecturally (for an overview of the literature, see Eickelman 1989:95–125; also Hourani 1991:125ff.). The patterns of socioeconomic organization which have been preserved until quite recently in such places probably reflect a long tradition of Mediterranean urban life, although we lack detailed information about any but its more recent manifestations. Traditional city- and house-plans in North Africa and the Near East reveal an “organic” pattern of tightly packed courtyard houses separated by narrow, winding streets and blind alleys (see photographs 1 and 2 below; numerous plans and photographs of traditional Islamic urban settings are published in Garcin and Revault 1988–91; for earlier descriptions, see Reuther 1910, on Baghdad, and Sauvaget 1941, on Aleppo). Houses are tightly packed because space is limited inside walled cities. Inner courtyards are necessary, therefore, to provide protection and seclusion for each house’s inhabitants. There are very few exterior windows, especially at street level; thus houses tend to be blank and undistinguished on the outside, so that disparities in size and wealth are not immediately apparent. The focus of activity is on the inside, where the ground floor is used for cooking, storage, and stables, and the second floor for sleeping. The flat roof provides extra space for various activities, and the inner courtyard is important as a source of light as well as being the center of circulation for the household.

*Photograph 1. (above)* Traditional houses in the town of Đumayr, Syria

*Photograph 2. (right)* Street in an older district of Damascus

(Photographs courtesy of Fred Donner; taken in June 1979.)
What kind of household organization do we find in such cities? In recent years, there has been a great deal of historical and ethnographic research on traditional urban households in North Africa and the Near East (e.g., Lapidus 1967; 1973; Hourani 1970; Eickelman 1974; Brown 1976; Wheatley 1976; Bonine 1977; Abu-Lughod 1987; for a detailed annotated bibliography see Bonine et al. 1994). Much of this work has been stimulated by a reappraisal of Western scholars’ use of Weber’s ideal type of the Islamic city, which has often functioned in the sociological and historical literature simply as a foil to the medieval European city. As I have noted above, Weber stressed the absence in the Islamic city of the kind of urban communal institutions typical of medieval and modern Western cities, raising the question of how the Islamic city held together at all, or whether the Western concept of the city has any meaning at all in this context, except in the purely economic sense of a large settlement. And there is no denying that, as Weber pointed out, the formal charters and uniquely urban institutions, like guilds, which existed in European cities are not found in the Islamic city. Recent students of the Islamic city have noted, however, that in place of these formalized citywide modes of organization are social institutions based on a network of informal personal ties by means of which urban life is organized. Aspects of social relationships that a modern Westerner might separate under the headings of “family,” “friendship,” and “patronage” are usually all expressed in terms of kinship. It has been argued, in fact, that these personalized, informal relationships are reflected in the spatial organization of the traditional Islamic city, which is a maze of narrow, winding streets and blind alleys. There is no abstract principle underlying the city plan; no overarching rationalized system that would permit a stranger to find his way unaided. On the contrary, one finds one’s way only through personal experience, by having gone there before.¹⁰

For its inhabitants, then, the Islamic city is not experienced as an abstract whole but as a nexus of informal social interactions among neighbors, who express their solidarity—both within households and between households—in the language of kinship. The geographer Paul Wheatley (1976) argues that the residents of traditional Islamic cities think of their urban environment not in terms of abstract “absolute space” but in terms of “pragmatic space”; that is, space as it is experienced in the social interactions of daily life. Pragmatic space is centered on one’s own familiar house and courtyard, and it extends (at least for adult males) to the entire residential quarter but seldom any farther, except along the main streets of the town. Outside one’s own neighborhood one is as lost and out of place as any foreigner. Furthermore, residential quarters or neighborhoods are defined by their inhabitants not as fixed architectural units but in terms of social relationships (see Eickelman 1989: 154ff.). A “quarter” is a cluster of households closely linked by ties of kinship, factional alliance, or patron-client relationship, all of which are expressed in familial terms. Not surprisingly, then, different ethnographic informants have different ideas about the number and composition of the residential quarters in a given town, and which houses belong to which. This is because quarters are not so much physical entities as dynamic social groupings; and the ties between households, as well as the composition of individual households, not only change constantly but are also perceived differently by different persons.

¹⁰ It should be noted that Weber (1978:1231ff.) himself emphasized the preeminent role of “clans” in the Islamic city and the importance of kinship (real or fictive) in Near Eastern urban social organization. Although he is often charged with displaying a crude ethnocentrism, a careful reading of his work shows that it is not necessary to see him as a prisoner of ideology and to interpret his typology of urban settlements as a predictable kind of “Orientalist” denigration of non-Western societies. Weber does not say that non-Western urban forms are maladaptive or inferior in any absolute sense; rather, he wishes to demonstrate that certain formally rationalized and individualized legal and political institutions were peculiar to the West, and that this helps to explain the development there and only there of rationalized bureaucracy (in the strict sense) and modern capitalism, regardless of whether we applaud these innovations. Viewed in this context, Weber’s fundamental typological distinction between the medieval and modern Western city, on the one hand, and the Islamic or Oriental city, on the other, still stands. Thus Mario Liverani’s (1997) attack on this typology, for example, is misguided, because he ignores the complex question of rationality and he disregards the purpose of Weberian ideal types as motivational models that clarify the symbolic constitution of society, as this is formed dialectically within a given economic and ecological setting (see chapters 3–5 above). In my view, Weber’s approach is far more defensible than Liverani’s own preference for objectivist systemic or functionalist models which, despite his denials, tend to level sociohistorical differences in a reductionist manner.
European colonial ethnographers used to complain about the “chaotic irrationality” of the Islamic city, which they attributed to the “alogical” character of the “indigenous mentality” (see Eickelman 1989:104ff.). But more recently, researchers have argued that the spatial organization of these cities reflects a local “cultural logic” in which the entire city, conceived as an abstract whole, is not a meaningful unit of analysis. Rather, from a phenomenological perspective it is clear that the fundamental units are the individual household, and, at a higher level, the residential neighborhood. These social units generate an irregular street plan, not out of a perverse desire for chaos and irrationality, but out of a primary concern for the needs of the household and its immediate neighbors rather than a concern for citywide organization. Wheatley suggests, for example, that this kind of cultural logic explains why the cul-de-sac was a basic component in the spatial structure of the traditional Islamic city. As he puts it, blind alleys are a rational response to a domestic lifestyle that ... focused on the interior courtyard of a house and ignored the external facade. Houses were [in effect] walled with a gate, each presenting a minimal extent of frontage, and that devoid of windows, to the public thoroughfare. In this context a system of culs-de-sac branching off from streets and alleys more or less at right angles afforded an efficient and economical means of semi-private access to a maximum number of habitations. Houses were entered from narrow passages penetrating deep within the block of which they formed a part. [Wheatley 1976:364]

Similarly, Eugen Wirth (1992; 1997) stresses the “privacy” of the Islamic city in contrast to the “public” nature of the classical and Western city. A concern for seclusion and withdrawal produces the five distinctive features he identifies as characteristic of the Islamic city (and to a large extent also the ancient Near Eastern city), namely, “the blind-alley structure of the residential district; protection from outside viewing and [the] inner-courtyard feature of the dwellings; many residential districts side by side and relatively closed off from outside; [a] striving for protection and security within walls, against disputes, unrest and uprising; [and] the suq (bazaar) as a special kind of . . . central shopping and business area” (E. Wirth 1992:22).

And it is not just a matter of family privacy in residential areas, for even palaces, mosques, cemeteries, and bathhouses can be “private” in certain respects. As he says, “it is simply a matter of withdrawing from the public’s responsibility and competence as many areas and localities of the city as possible and thereby withdrawing them from general access, and of dedicating them into places to which only a limited part of the population has access in accordance with some criterion or other” (ibid., p. 27). The end result is that “roads and squares are ‘negative space,’ so to speak—the outcome of spatial exclusion from the private sphere, not a spatial inclusion of urban open spaces which are for public and common use” (p. 29).

Figure 2. Traditional kin-based neighborhoods in walled town of Sefrou, Morocco (after H. Geertz 1979:319, fig. 1)

(The houses of one clan, shown in dark gray, are grouped around a blind alley. House courtyards are shaded light gray.)
The blind alley (or branching group of terminal alleys leading off a main street) preserves privacy, however, only because the houses clustered around these alleys form a social unit that acts as an extension of the individual household, as in the Moroccan town of Sefrou studied by Hildred Geertz (1979; see figure 2 above). Geertz, like other ethnographers of Middle Eastern urban households, discovered that this social unit (the neighborhood or “quarter”) expresses its solidarity in the language of kinship; thus the entire quarter has the character of a single large household, and the intensity of household-type relationships tends to diminish with spatial distance from a person’s own house. A phenomenon observed in many cities and villages in North Africa and the Near East is that the inhabitants of a given quarter claim descent from a common ancestor. Very often the genealogical relationships cannot be demonstrated, but informants believe that the close personal relationships that arise among neighbors must originate in actual kinship. Abner Cohen (1965; 1970) observed this in Palestinian Arab villages and he coined the term “patronymic association” to describe it. The patronymic association of households that make up a residential quarter is often organized around a leading household, whose patriarch acts as the patron of the poorer households nearby. The dynamism of these urban social relationships is reflected in the fact that the “quarter” is not a fixed spatial concept, for the number of households in a quarter can vary widely, as can the size of individual households.

Thus architectural and ethnographic perspectives combine to emphasize the role of individual choices and subjective perceptions operating within the symbolic framework of a highly personalized patrimonial society. In this regard, it is worth noting that Wheatley and Wirth’s emphasis on the global effect of local architectural decisions is reflected in the growing literature on modeling both settlement structure and intrahousehold social interaction using formal mathematical methods. The architectural theorists Bill Hillier and Julienne Hanson, in their influential book *The Social Logic of Space* (1984), emphasize the fundamental difference between architectural spaces which are defined by a higher order surrounding “cell” (e.g., a predefined rectilinear *insula*), as in planned cities, and spaces which are defined simply by the local aggregation of individual cells (e.g., houses and rooms). Local rules of aggregation lead to recurrent global forms which are quite different from the forms produced by top-down planning. This phenomenon calls to mind complexity theory and the notion of “fractal” patterns that are the same across all scales of measurement (discussed above in chapter 3.2). Just as Max Weber’s patrimonial household as a social type is a fractal pattern generated by local social rules which operate recursively to produce self-similar social structures across different scales of measurement, from the smallest social units (extended families) to the largest political units, the local rules of house construction in unplanned organic settlements recursively generate complex architectural formations which have the same fractal structure at various levels of detail. This is strikingly illustrated in Eda Schaur’s monograph *Non-Planned Settlements* (1991)—a detailed topological analysis of unplanned or “organic” human settlements in which Schaur points out the structural similarity of such settlements to various kinds of self-organized natural phenomena.

Furthermore, this fractal phenomenon seems to extend to the social interactions fostered by a given architectural arrangement of urban space, as we have seen in the ethnographic literature already cited. For example, the Mediterranean-style courtyard house encourages a typical set of interactions among its inhabitants, despite variations in the overall size and shape of individual houses, and at each higher spatial scale the same kinds of interactions are encouraged; that is, the types of kinship interactions that occur within a house are similar to those that occur between neighboring houses (i.e., among members of the same patronymic association), which in turn are similar to those that occur between occupants of neighboring blocks of houses (demarcated *insulae* or quarters), who are members of different patronymic associations but may still feel some kind of overarching tribal solidarity. In this regard, it is worth noting that Hillier and Hanson’s own view of the relationship between architectural space and social action leans too much on Durkheimian functionalism and thus neglects the symbolization of social order as it is instantiated in the temporal rhythms of everyday lived experience (so also Blanton 1994); whereas the notion of a recursively generated architectural pattern actually conforms much better to phenomenological social theory, as exemplified in Bourdieu’s *Outline of a Theory of Practice* (1977; esp. pp. 114–32 on the everyday experience of utilitarian domestic space as the space of “cosmogonic” ritual action among the traditional Kabyle people in Algeria).

Within individual households in the preindustrial Mediterranean Islamic world, the patrilineal, patrilocal joint family is the norm, as in the traditional Moroccan households in Sefrou that Geertz has discussed. In other words, married sons, together with their wives and children, tend to live in the same house as their parents. Each conjugal couple and their
immature children have their own private space in the house—usually a single sleeping-room that opens onto the central courtyard and contains their personal possessions; but the entire joint-family household is a single economic unit. Food preparation and other domestic activities are done in common and the whole household eats together, although there is also a great deal of visiting back and forth among relatives in neighboring houses. Agricultural work is done jointly by household members on land outside the city that is owned by the head of the household, and animals are stabled inside the city walls, on the ground floor of the house. Poor relatives often join the household as clients, receiving support and living space in return for their labor; thus a full three-generation household might well contain several adults and an equal number of children.

How useful are these ethnographic and architectural data in interpreting the material from Late Bronze Age Ugarit, or any other ancient Near Eastern city? There has been considerable debate about the Bronze Age Ugarit, or any other ancient Near Eastern cultural data in interpreting the material from Late Bronze Age Ugarit, or any other ancient Near Eastern city? Probably not, for there is little evidence for purely religious causes of these phenomena, whether in the area of family law or in attitudes toward city planning (or the lack of it). The desire for domestic privacy is heightened, perhaps, by Islam’s emphasis on the seclusion of women, but it seems unlikely that this requirement alone is responsible for the courtyard houses and irregular street plan found in many Near Eastern and North African cities. Indeed, these are features of pre-Islamic cities in the region, including Late Bronze Age Ugarit, not to mention the medieval European cities that were also characterized by patronymic associations and their architectural analogues (see Heers 1977 on “family clans” in medieval Europe).

Hugh Kennedy (1985) addresses this issue in terms of the transition from the Byzantine to the Islamic period in Syria, and he concludes that “urban change in the Middle East took place over a number of centuries and . . . the development from the polis of antiquity to the Islamic madina was a long drawn out process of evolution. Many of the features which are often associated with the coming of Islam, the decay of the monumental buildings and the changes in the classical street plan, are in evidence long before the Muslim conquests” (p. 17). Conversely, as Kennedy points out, the creation of orthogonally planned cities was not totally alien to Islamic culture: “When Muslim rulers laid out new cities, they adopted orthogonal plans, dividing blocks of housing by straight and sometimes wide streets. . . . The contrast is that in classical antiquity most cities, including the largest and wealthiest, were planned and ordered; in Islamic society they were not.” As for the causes of this shift in the proportions of planned versus unplanned towns, Kennedy notes that “public, open spaces, be they narrow suqs or wide colonnaded streets, will always be under pressure. They will only survive if they fulfill a perceived and generally acknowledged purpose and are protected by an active and vigilant civic authority” (p. 18). He goes on to suggest several reasons why the constraints on filling public spaces which operated in the classical period became inoperative in the late antique and early Islamic periods. In particular, he attributes this to the much smaller role of the central government and the decline of public patronage by political rulers in the context of an increasingly “minimalist state,” noting that Roman law was concerned with public property and the city as a corporate entity in a way that Islamic law was not, because “for Muslim jurists the important unit was the family and the house. . . . it was held that they should be allowed to do anything they chose as long as it did not harm their neighbours” (p. 21). Presumably, this was part of a general trend after the fall of the Roman empire toward a more personalized, patrimonial view of society—a trend that was not created but was simply accelerated by the Muslim conquests—and away from the more rationalized and abstract social ideal of the classical world.11

The recurrence of those age-old features of the Mediterranean urban environment so well attested in the unplanned Islamic city is no doubt due in part to unchanging economic and ecological conditions characteristic of the entire preindustrial era, but the Graeco-Roman counter-example shows that another factor must have been at work, namely, a persistent ideal of personalized patrimonial authority as opposed to impersonal bureaucrat (see chapters 3.1 and 4.1). The relative balance between patrimonialism and bureaucracy seems to have shifted in the transition from the classical to the Islamic period. One major impetus for this shift is likely to have been the much greater political and social role of extensive tribes in the Arabic-speaking Islamic world. Middle Eastern tribalism has been the subject of considerable discussion among anthropologists and Islamicists

11 S. M. Stern (1970) had earlier made this point. As he says, “Islamic civilization did not, however, inherit the municipal institutions of Antiquity, because, owing to their gradual decline, there was by the time of the Muslim conquest of the provinces of the Roman Empire nothing left to inherit” (p. 26).
the Middle Eastern tribalism is one version of patrimonialism. Speaking more generally, it is clear that classic patrimonialism has never been limited to pastoral nomads but has also been noted, tribal organization has been defined with reference to descent (p. 356). Crone notes that tribal identities are quite discrete and specific, not vague and overlapping, as some have claimed on the basis of non-Middle Eastern ethnographic cases that Crone argues are not tribal in the same sense. Middle Eastern tribal identities are clear and effective precisely because they are defined in terms of one’s parentage. And the fact that tribal membership is an ascribed status based on biological kinship (or the fiction of biological kinship) does not make it less significant or more prone to manipulation than membership in any other kind of socially constructed political community, be it the Greek polis or the modern nation-state. Moreover, as many researchers have noted, tribal organization has never been limited to pastoral nomads but has also characterized urban populations in the Middle East.

Speaking more generally, it is clear that classic Middle Eastern tribalism is one version of patrimonialism; that is, it is one way in which the symbol of the “house of the father” has been deployed in practical social action. But the connection between patrimonialism in the form of tribalism, on the one hand, and settled urban life, on the other, is bound up with the complex question of the relation between tribes and states (“state” being understood here in the most basic sense as a central governing authority that claims a monopoly on the use of force). Some view tribes and states as two antithetical modes of organization which tend to destroy one another, while others regard them as symbiotic components of a single system. Both kinds of relationship are attested, for in some states tribal identities have been actively suppressed, and conversely there have been “tribal states” that originated in conquering tribes and retained a strongly tribal mode of political organization for a long period thereafter.

Indeed, the initial Muslim conquests themselves were carried out by Arabian tribesmen—both sedentary and nomadic—and it has frequently been suggested that the relatively intense tribalism of these conquerors left its mark on urban society within the subsequent Islamic regimes. In this regard, Crone (1993:375), for one, insists that the new Muslim faith did not at all undercut tribal solidarity but rather complemented it: “Muhammad’s monotheism created a community which transcended particularistic loyalties, but it did not set out to destroy the tribes, only to use them as building blocks: all Arabs were sons of Abraham and adherents of his religion; they fused as a super-tribe united by common descent and faith alike.” Here, in my view, is the best explanation for the patrimonial character of Islamic society in both its urban and rural manifestations, because this patrimonialism was deeply rooted in the founding symbolism of Islamic culture, regardless of how dominant particular tribal identities were in day-to-day economic and political life.

Suffice it to say that the Islamic Mediterranean world, with its varying intensity of tribalism in different times and places, and its varying relationships between tribe and state, generally exhibits a degree of patrimonialism not found in the classical world, and this is reflected in urban architecture and urban social

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12 The leaders among these conquering Arabian tribesmen were settled townsmen from Mecca and Medina, not nomads, as Fred Donner (1981) has shown. But they were no less tribal for that reason.

13 Elsewhere Crone (1987a) marshals a great deal of evidence in order to dispute the widely held view that pre-Islamic Mecca was the center of a far-flung trade network, with the result that commercial interests had already broken down tribal particularism before Muhammad came on the scene, and hence that such interests became a factor in the political unification of the Arabian tribes under Muhammad as well as providing a motivation for the subsequent Islamic conquests. She argues that: “The reason why additional motives are so often adduced is that holy war is assumed to have been a cover for more tangible objectives. It is felt that religious and material interests must have been two quite different things—an eminently Christian notion; and this notion underlies the interminable debate whether the conquerors were motivated more by religious enthusiasm than by material interests, or the other way round. But holy war was not a cover for material interests; on the contrary, it was an open proclamation of them . . . . Muhammad’s God thus elevated tribal militance and rapaciousness into supreme religious virtues: the material interests were those inherent in tribal society, and we need not compound the problem by conjecturing that others were at work. It is precisely because the material interests of Allâh and the tribesmen coincided that the latter obeyed him with such enthusiasm” (p. 244f.). If Crone is correct, then here is another case in which modern scholars are guilty of an anachronistic economism in historical reconstruction, failing to seeking motivations for epoch-making shifts in patterns of social action elsewhere than in a familiar modern form of economic rationality (see the discussion of economic rationality in chapter 4.4 above).
relations (for a synopsis of recent opinions, see also the papers in Wilhelm 1997, from a conference which focused on continuities and changes in the “oriental city,” both preclassical and postclassical). Detailed ethnographic, textual, and architectural evidence from traditional Islamic towns is therefore extremely valuable in providing a comparative context within which to interpret the material remains of typologically similar ancient cities of the Mediterranean region, which also, I would argue, exhibit a strongly patrimonial (if not necessarily tribal) conception of the social order. The Islamic evidence should not, however, be interpreted in such a way as to impose a false uniformity on the large number of geographically and temporally diverse urban centers that can be included in the category of the “traditional Islamic city.” These settlements were not all alike, and they are not all equally comparable to ancient Near Eastern (especially Levantine) cities. The agricultural component of such cities, for example, varied greatly in size and importance. The largest preindustrial Islamic cities, whose populations numbered in the hundreds of thousands, no doubt had many households that were not engaged in agriculture but instead devoted themselves to various specialized economic activities, even though the joint-family household and the kin-based quarter often remained the basic social and economic units (but note that even in nineteenth-century Cairo as much as half of the “economically active” urban population was engaged in agriculture, according to a contemporary census cited by Abu-Lughod 1969:164). These huge urban centers therefore have less in common with Bronze Age Syrian cities such as Ugarit, which occupied an area of perhaps 25 hectares and probably had no more than 10,000 inhabitants, than do the smaller walled towns of Islamic North Africa and the Near East in which agricultural activity predominated.

Among the larger Islamic cities, however, a useful comparison may be made with Ottoman Aleppo, which in the eighteenth century A.D. had on the order of 100,000 inhabitants in a built-up area of 365 hectares, with an overall population density of ca. 300 persons per hectare (including nonresidential areas occupied by suqs, mosques, and administrative buildings). Located in inland north Syria, Aleppo is close to ancient Ugarit geographically and is well documented both architecturally and textually. The textual sources, which consist of Ottoman tax accounts and šari‘a law court records, have been analyzed by Abraham Marcus (1989). His use of this evidence to reconstruct the demography of Aleppo “on the eve of modernity” in the eighteenth century will be discussed below in chapter 7.3. Marcus’s reconstruction is complemented by detailed German studies of the extant architecture of houses and quarters in the Old City of Aleppo, in light of historical sources (e.g., Gaupe and Wirth 1984; Gangler 1993). These studies provide precise data on the physical appearance, size, internal organization, and interrelations of individual dwellings in this typical Levantine Islamic city, which, like ancient Ugarit, was characterized by narrow winding streets, blind alleys, and two-story courtyard houses.

In chapter 7.3 it will also be worth commenting on the very similar situation around A.D. 1700 in Ottoman Damascus, whose familial and social structure...
has been reconstructed on the basis of a large sample of probate inventories by Colette Establet and Jean-Paul Pascual (1994). Before examining this evidence, however, and in order to interpret it properly, it is necessary to consider the mathematically based models developed by historical demographers to determine the range of possibility for life expectancy and thus age distribution under preindustrial Mediterranean conditions, which in turn determine family composition or “kinship universes,” assuming specific attested patterns of mortality, fertility, age at marriage, and coresidence. The demographic “facts of life” provide an important external constraint on our reconstructions of household organization, be it medieval, classical, or ancient Near Eastern—reconstructions that must at the same time take account of the historically contingent symbolic framework that constitutes society itself, and the city in particular.
Chapter 7. Mediterranean Historical Demography

In this chapter we turn from culturally specific, symbolically mediated patterns of social action to the impact made on such patterns by cross-cultural human phenomena having to do with the unavoidable biological facts of birth and death. What follows is a highly selective review, both of demographic theory and of the premodern Mediterranean data which this theory helps us to interpret. The goal of this exercise is to establish plausible demographic parameters based on Mediterranean demographic evidence, parameters which are likely to be relevant to even less well documented settings in the ancient Near East because of the impressive demographic uniformities that are widely attested across time and place within the Mediterranean region, whether they are observed in Ottoman Syria, Renaissance Tuscany, or Roman Egypt. These uniformities permit us to speak of a “Mediterranean” type of domestic group organization (Laslett 1983). Of course, this sort of demographic construct cannot be imposed mechanically in the reconstruction of much earlier societies of the Bronze and Iron Ages, which were culturally very different in important respects, because the demographic facts of life do not operate in isolation but interact with historically contingent factors such as the preferred age at marriage (often quite different for men as opposed to women), the incidence of polygamy, the location of postmarriage residence (giving rise to neolocal or “nuclear-family” versus patrilocal or “extended-family” domestic groups), the spacing of births, and the partibility or impartibility of inherited property. Nevertheless, a detailed demographic model derived from later periods is a valuable heuristic device for understanding social organization in Late Bronze Age Ugarit and Iron Age Israel, in particular, because such a model can be checked against available archaeological and textual evidence at certain key points.

1. Marriage and Residence Strategies

In demography, as elsewhere in the social sciences, positivist reductionism has come under attack in recent years (see the essays in Kertzer and Fricke 1997, especially the editors’ introduction; also Carter 1998). In particular, it is increasingly recognized that preindustrial systems of household coresidence can vary widely, even within a single geographical region, depending on local customs and circumstances. This diversity of household systems occurs because domestic group organization is not the predictable product of those few widely applicable demographic and economic variables that happen to be amenable to quantification and mathematical modeling—variables which demographers have tended to hypostatize in a methodologically holistic manner without regard to the subjective meanings of social action. Rather, domestic group organization is the product of a complex set of local interactions and negotiations among family members, who pursue their own material and status interests within the shared linguistic and symbolic framework of inherited traditional practices, which they may faithfully reproduce or perhaps may alter significantly, as their external economic and political situation permits.

David Kertzer (1991; 1995a; 1995b; 1997) has made this point with respect to European household history, paying special attention to the several different types of preindustrial Mediterranean household which are now documented in the historical-demographic literature. Kertzer criticizes the oversimplified typology developed by pioneers in the field of European household history such as Peter Laslett (1972; 1983) and John Hajnal (1983). In Laslett’s typology, for example, there is a distinctive northwest-European nuclear-family household type that was generated by a relatively late female age at marriage (enabled by the widespread practice of having young people live before their marriage in households other than the ones in which they were born, working as servants or apprentices) and by a relatively high incidence of persons who never married. Late marriage delays childbearing and thus, given high rates of mortality, hinders the formation of complex three-generation households. Following Hajnal, Laslett emphasizes the determining role of female age at marriage, and he contrasts the northern nuclear-family household type with Mediterranean and eastern European types of complex-family households (both “stem” and “joint”), which are fostered by a relatively young female age at marriage.1

1 In this terminology, as Kertzer (1991:158ff.) explains: “The nuclear family household consists in its full form of a married couple and their children. This is contrasted with the complex family household, which includes kin beyond the nuclear family. Historians have tended to distinguish between two kinds of complex family households. In the stem family form, one child, and one child only, brings his
As Kertzer (1991) shows, however, a variety of nuclear-family and complex-family household types is attested, both in northwestern Europe and in southern and eastern Europe, calling into question Laslett's broad geographical division between a neolocal northwest and a patrilocal south and east. But in the face of this diversity of household types, Kertzer himself does not eschew demographic generalization altogether. Instead, he emphasizes the predictable interactions which occur between the uniformly applicable demographic factors of mortality and fertility, on the one hand, and historically contingent constraints and incentives created by local political and economic conditions, on the other. Kertzer notes the differences, for example, in the household systems employed by (or imposed on) different subgroups of the preindustrial population, such as serfs (in eastern Europe), sharecroppers, landless agricultural laborers, and peasant proprietors, whose political and economic circumstances were not the same even if they lived in the same geographical area. Serfs tended to have very large, complex households because their landlords encouraged early marriage and maximal coresidence to suit their own economic and political interests. Similarly, Mediterranean sharecroppers had joint-family households in order to maximize their household labor force, so that they could provide the return on the land demanded by their landlords. Landless laborers, on the other hand, often had nuclear-family households. They did not need large households because for them the household was not a unit of production; instead, they lived in small rented dwellings and sold their labor on the market as individuals. As for peasant landowners, a legal tradition of partible inheritance in certain regions (e.g., northern France) led to neolocality and nuclear-family households, whereas a strong tradition of impartible inheritance in the Mediterranean south encouraged complex households, especially the “stem” family (la famille souche) in which only one son, typically, remains in the parental household after marriage.

It is important to keep in mind the complexity and dynamism of these household arrangements, both within an individual’s lifetime and from generation to generation. Although we can indeed detect certain systematic tendencies, the complexity of the situation is best expressed in terms of agent-oriented motivational models rather than holistic causal models. Our analytical focus must remain on motivated individual actions, within the framework of the inherited traditions which mediate those actions and the chance opportunities or disasters which befall individual persons. Thus the various agrarian groups Kertzer identifies as having different household systems should not be regarded as hermetically sealed social castes, at least in the Mediterranean context. Persons who grew up in landowning or sharecropping households could become landless laborers, and vice versa, and the choices they made in terms of marriage and residence would reflect this. Ultimately, then, demographic attributes such as “household type” cannot be predicated of reified social groups, as if these collective entities had an enduring independent existence. More fundamentally, such attributes must be connected to the symbolically mediated actions of individual social agents, for whom a traditional household type such as the patrilocal “house of the father” may loom large as a symbol of the “right” way to do things, without eliminating their power to make and to rationalize pragmatic exceptions to the rule in their own lives.

One major external factor in this domestic dialectic of tradition and rationalization is the scarcity of cultivable land, which is a particular problem in southern Europe and the Mediterranean area in general, and seems to have encouraged the preference for patrilocal residence (and thus complex households) and the legal tradition of impartible inheritance that are so characteristic of this area. The reasons for this are discussed by Richard Wilk and William Rathje (1982), who cite ethnographic research which demonstrates “a strong cross-cultural relationship between land scarcity and extended household structures. . . . Sons must stay within or close to their natal household in order to assert a claim to their father’s land” (p. 628). Furthermore, as they point out, land scarcity and complex households are highly correlated with impartible inheritance, which in turn leads to wide variations in wealth and family size among different households:

As long as land is plentiful enough for households to divide between heirs, partible inheritance is practiced; the household will fission after the death of the head if it has no function in pooling distribution or scheduling production. As population pressure on land resources or capital investment in productive resources increases, there is an increasing tendency for impartible inheritance to be the rule. Only one heir in each generation inherits the bulk of the household patrimony. Further subdivision would result in less than sufficient land being transmitted. Other household members who do not inherit must either stay in the household remaining unmarried and work for the
heir, or depart and find some kind of other economic base. . . . Households that own land often add client . . . households in order to obtain additional labor without giving up rights to land. . . . Landless households, on the other hand, have no resources to distribute and no use for extra labor. Their children have no reason to stay, and go out as servants or emigrate. This leads to the often observed phenomenon among agricultural societies of differences in household sizes that correspond to social status and land ownership. [ibid.]

Wilk and Rathje’s argument has a strongly functionalist flavor, and we should be wary of accepting it as a sweeping generalization. In many cases there may be culture-specific traditions operating independently of rational-maximizing economic incentives—traditions that prevent objectively measurable factors such as land scarcity from affecting household composition in any automatic or predictable fashion. But when properly evaluated, the cross-cultural ethnographic evidence cited by Wilk and Rathje, as well as the historical evidence cited by Kertzer, allows us to conclude that household systems can indeed be predicted by such factors as age at marriage, postmarriage residence (i.e., neolocal versus patriloclal), and partibility of inheritance, as long as we remember that these “demographic” variables are themselves intimately bound up with culture-specific choices and incentives that operate at the level of individual social agents.

Thus, in demography as elsewhere in the social sciences, the spatial and temporal scale of our predictive causal models, which refer to the statistical properties of social groups of varying degrees of size and durability, is constrained by unpredictable human culture and not vice versa. Indeed, it must be emphasized that, contrary to what many demographers and other positivist social scientists have assumed, “culture” is not simply what is left over as a residual “error factor” after constructing an objectifying positivist model of social behavior, as if culture could be defined merely as those aspects of social behavior which cannot be readily quantified or modeled in causal terms. On the contrary, by any meaningful definition of the word, culture encompasses all that we may isolate for analytical purposes as the objective “political” and “economic” aspects of what remain, more fundamentally, subjectively meaningful patterns of individual social action. Our political and economic models are objectified fragments of a much more complex social reality; these models may in turn be incorporated into causal demographic models, but they should not be reified as automatic external determinants of action. Methodologically, what is going on here is the “enveloping.” as Paul Ricoeur puts it, of secondarily objectified causal models (political, economic, and demographic) within a more basic hermeneutical perspective that focuses on understanding individual motivations or “reasons for acting”—a mode of understanding which itself is “developed” analytically with the aid of these explanatory causal models (see chapter 2.2 above).

Having said this, it is clear that we need not discard generalized household typologies altogether, but that they must be used with care. Kertzer’s examples, like Laslett’s (1983), show that similar economic and political conditions can foster similar household systems in different times and places, although Kertzer deals with this in a much more nuanced historical manner. For example, as Kertzer (1991:166) observes, complex joint- or stem-family households are strongly correlated with sharecropping and impartible inheritance in southern France and Italy, whereas landless wage laborers in the same geographical area tend to have nuclear-family households. This suggests that while there is not a simple “Mediterranean” type that covers all preindustrial contexts, there may be an identifiable “Mediterranean sharecropping, impartible inheritance, complex-family” type of domestic group organization, which presumably was sustained throughout history by a mutually reinforcing combination of enduring symbolic traditions and unchanging ecological facts. Kertzer is right to stress the complexity of the situation, but on a broader level of analysis Laslett and others are not wrong to note the statistical preponderance of patrilocal complex-family households of various kinds in the Mediterranean region, as opposed to the higher incidence of neolocality in northwestern Europe. Of course, the existence of a complex-family household system in a given geographical and historical setting must be argued on archaeological or textual grounds; it cannot simply be inferred on the basis of some kind of economic (let alone geographical) determinism. But like many ideal types, the “Mediterranean” type of domestic group organization combines causal and motivational models in such a way that we can link predictable patterns of social behavior to recurrent types of human motivations within a given geographical setting, however culturally specific the expression of these motivations may be—especially if we can attribute the recurrence of these motivations to an inherited symbolic tradition like the patrimonial “house of the father,” which continued to thrive in both Christian and Muslim regions of the Mediterranean world.

For our purposes, the significance of this “Mediterranean” domestic type lies in the fact that the
sharecropping, impartible inheritance, complex-family household seems to have been characteristic of the Bronze and Iron Age Levant, as I will attempt to demonstrate in some detail below in the case of Late Bronze Age Ugarit. In contrast, the “landless-laborer nuclear-family” type and the “partible inheritance nuclear-family” type which are also found in preindustrial Mediterranean settings are much less applicable. Textual evidence suggests that good land was scarce and inheritance was impartible in the Levant by the second millennium B.C., if not earlier. Moreover, landless persons did not eke out a living as wage laborers who maintained their own meagre households, as in more recent times. By and large, the inhabitants of Bronze and Iron Age Syria and Palestine lived in nonmonetized (or very weakly monetized) agrarian societies in which market exchange and wage labor were minimal and rents (or “taxes”) were normally paid in kind; thus landless persons survived not as wage laborers but as dependent household workers (slaves or clients), who joined complex-family sharecropping households by adoption or in some form of servitude.

The great exception to this occurred in the early Iron Age, from 1200–1000 B.C., when new land was cleared and settled in the highland regions of Palestine by various refugees and landless persons, including those who became the Israelites; but even there good land was limited, and within a few generations there was the “closing of the Highland frontier” (Stager 1985a). In well-established states such as Bronze Age Ugarit and similar Canaanite kingdoms (before 1200 B.C.), and Iron Age Israel (after 1000 B.C.), the sharecropping model is most plausible, for there is evidence that these were relatively simply regimes in which the king as patrimonial ruler owned all of the land, which was parceled out to his subordinates, and in turn to their subordinates, in return for a share of the crop. The modern concept of private property did not exist, thus the category of independent peasant proprietors was absent and the postmedieval European distinction between peasant proprietors and sharecroppers is irrelevant. Impartible inheritance (of usufruct) was characteristic of sharecroppers in this context, however, because the right to cultivate land was hereditary. Hence in the Bronze and Iron Age Levant there is combined in one type of cultivator both of the incentives to form complex households which are known in preindustrial Europe: impartible inheritance, as in the case of European peasant proprietors, and the demand of landlords to maximize the household laborforce and thus their return on the land, as in the case of European sharecroppers.

In reconstructing patterns of coresidence, however, it must also be borne in mind that the culturally and economically preferred option is not always chosen. Individual interests and circumstances may lead in some cases to neolocal as opposed to patrilocal residence, as we have seen, even though patrilocal residence remains the avowed preference and the symbolic center of gravity. This sort of discrepancy between cultural norms and pragmatic individual choices is also evident in the selection of a spouse. In traditional Middle Eastern society, for example, there are frequent exceptions to what remains a very durable preference for marriage between patrilateral parallel cousins; that is, it is believed that a man should marry his father’s brother’s daughter (FaBrDa)—the bint al-amm in Arabic—or a more distant cousin on his father’s side. This is a very ancient cultural tradition, at least in the Levant, for it is attested in the Hebrew Bible, which in general bears witness to typically “Middle Eastern” patterns of marriage, kinship, and family life (Patai 1959; de Vaux 1961).2

Now, the longstanding Middle Eastern preference for FaBrDa marriage presents a problem for structuralist kinship theory, which was given classic expression in 1949 by Claude Lévi-Strauss in his book On the Elementary Structures of Kinship (1969) and was elaborated further by Edmund Leach (1961).3 In the selection of a spouse, household coresidence intersects with the broader kinship relationships that constitute clans and tribes as political and economic associations. Working with Southeast Asian ethnographic data, Leach showed that matrilateral crosscousin marriage (marriage to one’s mother’s brother’s daughter, or MoBrDa) is the ideal endogamous match in a patrilineal society because it creates stable kin-based alliances between separate patrilineal groups (wife-givers and wife-takers), while still permitting marriage to a close relative, so that wealth

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120 Mediterranean Houses and Households

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2 The marriage and kinship practices described in the Bible are endogamous, patrilineal, patrilocal, and sometimes polygamous, and they frequently include FaBrDa marriage, as in more recent Islamic Middle Eastern society. It is not surprising, therefore, that in biblical Israel the basic socio-economic unit was the patrilineal clan (Heb. mišpähā), an endogamous “patronymic association” of persons claiming descent from a common ancestor, which consisted of a number of patrilocal joint-family households (Heb. bêt ḫab or “house of the father”) that were joined by intermarriage (see the more detailed discussion in chapter 8.4 below).

3 A summary of this approach to marriage and kinship, focusing on the structural significance of the exchange of women, may be found in the essay entitled “An Anthropological Perspective on Kinship and the Family” by Françoise Zonabend in The History of the Family, vol. 1 (Burguèire et al. 1996:8–68).
stays in the family. Patrilateral parallel-cousin (FaBrDa) marriage would thus appear to be excessively endogamous and so dysfunctional as a means of forming alliances.

The anthropologist Fredrik Barth (1973) has argued, however, that Middle Eastern kinship systems differ from both the sub-Saharan African and Southeast Asian kinship systems that have figured so prominently in kinship theory. To understand FaBrDa marriage, one must take a different approach to kinship entirely. Like Pierre Bourdieu (1977), Barth criticizes the objectivism of structuralist and functionalist attempts to create abstract models that account for all behavior, because these models place too much emphasis on after-the-fact native rationalizations and so neglect actual practices. Barth maintains that one should focus instead on “the dialectic between the concrete behaviour of persons, groups and categories on the one hand, and the collective institutions and customs as the outcome of a complex aggregation of numerous micro-events of behaviour, based on individual decisions in each person’s attempts to cope with life” (ibid.), although these individual decisions are of course influenced by actors’ shared knowledge and enculturation, with the result that there is a dynamic relationship between the “confrontation of reality” experienced by a group of persons and their shared understandings or “culture.” In other words, Barth advocates Weberian methodological individualism in kinship theory rather than the methodological holism characteristic of Durkheimian functionalist and structuralist anthropology, which tends to reify patterns of individual behavior as efficacious collective entities.

Barth goes on to argue that Middle Eastern kinship, in particular, should be viewed from this agent-oriented perspective, because the standardized organizational potential of collective kinship phenomena is very limited in the Middle East, as opposed to other parts of the world, confounding structuralist kinship theory. As he says, the “we-versus-they” confrontation is inherent in social interaction, and the experience of who “they” are will shape the actor’s conception of “we”; and because people in traditional societies see who “they” are in terms of the whole kinship network, different marriage systems imply different images of “we” versus “they.” In the endogamous Middle East, uniquely, the wife-givers and the wife-takers are the same group—as a result of FaBrDa marriages, “the most elementary distinctions of kinship between agnates, matrilaterals, and affines will be confounded” (p. 7). Kinsmen of all kinds are regarded as essentially similar, because the father’s brother is also the wife’s father, and also a matrilateral relative. The consequences of this for political life are elaborated by Barth as follows:

Next, let us consider the political confrontations that take place between the larger descent group to which an actor belongs, and the “they” group to which it is counterpoised. It is my thesis that the actors’ experience of this opposition will affect the cultural codification of the meaning and content of descent. Detailed analyses from different parts of the Middle East document again and again that such political confrontations do not in fact follow a simple segmentary charter of fusion and fission . . . . There are limited situations, such as in the allocation of usufruct rights to jointly owned land, when a nesting hierarchy of segments is made tangible; but even then the actual politics is of a more complex nature. In far the most confrontations involving related parties, the opposed units are not unilineal descent segments but factions, built on bilateral and affinal relations, friendship and opportunistic alliances as well as a selection of agnatic relations . . . . the main schisms of descent groups occur between close collaterals . . . even full brothers; and these schisms serve as a focus for the alignment of others—not without regard to segmental position, but transmuted by cognatic and affinal ties created in part by FaBrDa marriage. [Barth 1973:13]

Barth notes further that FaBrDa marriage, while preferred and often viewed as a right, is by no means universal in any Middle Eastern society when looked at statistically. Rather, it is “one of an arsenal of possible moves for securing social position” (p. 14). It serves to make peace between close collaterals with opposing interests, or to bolster some collaterals against others. FaBrDa marriage is not the only option selected because it may be necessary from time to time to make a new alliance with more distant kin or to renew an old one with matrilaterals (via matrilateral cross-cousin marriage, for example). Thus a mixed marriage system is perpetuated, while FaBrDa marriage and endogamy remain the cultural norms and are also the dominant choices statistically because they keep property within the family (cf. Bourdieu 1977:30–71).

This approach to Middle Eastern kinship provides a corrective to certain misuses of ethnographic analogy in explaining the biblical narratives which originated in Iron Age Israel. Various structuralist interpretations of biblical narratives, especially the patriarchal narratives of the book of Genesis, have failed to take account of the distinctive features of Middle Eastern kinship systems (e.g., Leach 1969; Andriolo 1973; Donaldson 1981; Oden 1983; see Emerton 1976 for a critique of Leach’s use of the
biblical materials). Robert Oden (1983), in particular, explains the patriarch Jacob’s matrilateral cross-cousin marriages (MoBrDa) in terms of kinship theories derived from African and Southeast Asian ethnography. He argues that Jacob becomes “Israel” and thus father of the nation in Israelite tradition because his marriages are ideal endogamous matches in structural terms. But this ignores the fact that patrilateral parallel-cousin marriage (FaBrDa) is much more frequent among the patriarchs and other biblical figures (see Patai 1959:27–31) and is preferred in traditional Middle Eastern societies.

Terry Prewitt (1981) analyzes the Genesis material more convincingly, drawing on Barth’s treatment of Middle Eastern kinship. Prewitt notes the presence of both FaBrDa and MoBrDa marriages in the patriarchal narratives and suggests that a man’s marriage preference, in the kinship system represented in Genesis, is determined both by his mother’s identity as a member of an already allied kin-group and by his father’s identity as agnate to a number of potential alliance groups in the region. Whether a new alliance is formed or an existing alliance is strengthened will depend on the factional situation at the time of the marriage (see also Steinberg 1991).

Presumably, the patriarchal narratives reflect some stage of Israelite history, but their precise historical setting is not important here. What is clear is that ancient Israel at some point, and probably throughout its history, shared in the typical Middle Eastern pattern of highly endogamous FaBrDa marriages and what we can call “dynamic-factional” rather than purely “structural kinship” modes of organization, which is why exogamy is frequently criticized in the Bible and endogamous restrictions were vigorously reasserted under Ezra after the Babylonian exile. While this latter development may reflect a postexilic setting is not important here. What is clear is that ancient Israel at some point, and probably throughout its history, shared in the typical Middle Eastern pattern of highly endogamous FaBrDa marriages and what we can call “dynamic-factional” rather than purely “structural kinship” modes of organization, which is why exogamy is frequently criticized in the Bible and endogamous restrictions were vigorously reasserted under Ezra after the Babylonian exile. While this latter development may reflect a postexilic

Aside from the individual strategies and choices concerning marriage and residence of the kind Barth discusses (what Pierre Bourdieu has called “regulated improvisations”), in many cases patrilocal residence in a joint-family household is simply not an option because of the death of a man’s father before his own marriage. In the interpretation of domestic architectural remains and relevant textual evidence we must therefore take into account not only non-normative individual choices which are contingent upon particular circumstances, like the occasional choice of MoBrDa instead of FaBrDa as one’s spouse, or the choice of neolocal as opposed to patrilocal post-marriage residence; we must also take into account the household lifecycle or “life course” produced by preindustrial rates of mortality and fertility. The interaction of mortality and fertility rates determines the proportion of the population who would have living kin of a given sex at a given age, and thus the proportion who could be expected to be in the nuclear-family phase of the complex household lifecycle. We must reckon with the effect of mortality rates on the average size of families, and hence the amount of living space that even a full-fledged joint family would typically have occupied. It is necessary to quantify these proportions and sizes, however roughly, so that comparisons may be made with available archaeological and documentary evidence. Here we can make use of the model life tables and computer-based microsimulations developed by historical demographers for just such purposes. In summary, these mathematical calculations show that even if we assume a high fertility rate, nuclear family sizes in the Bronze and Iron Age Levant were relatively small, on the order of 3.5 persons on average, while patrilineally extended joint families had only 7 members on average.

The effect on family size of a high mortality rate was pointed out with regard to households in Iron Age Palestine by Lawrence Stager (1985a:18–20 and table 4, p. 21), who based his figures on Thomas Burch’s (1972) calculations. These are summarized

4 Stager’s own figures are somewhat higher than necessary because he added 2 to Burch’s figures to allow for the parents. This correction is not needed, however, because Burch had already calculated the total female stable popula-
below in Table 1 for two different rates of mortality: a
low average life expectancy at birth (e0) for females
of 20 years, which is fairly close to what is attested
for the ancient Mediterranean world, and a much
higher average life expectancy at birth of 40 years,
which is unlikely in any preindustrial context. “GRR”
in this table is the “Gross Reproduction Rate”; that is,
the average number of daughters borne by a woman
who lives through her childbearing years, which is
the basic demographic measure of fertility (see Wrig-
ley 1969:20–22). GRR does not take account of mor-
tality, unlike the “Net Reproduction Rate” (NRR),
which is 1.0 by definition in a “stationary” population
in which there is no net increase or decrease in total
population size. The average life expectancy at birth
in years (“e0”) measures the mortality rate.

Table 1. Estimates of Average Family Size

<table>
<thead>
<tr>
<th>GRR</th>
<th>Nuclear e0 = 20</th>
<th>Extended e0 = 20</th>
<th>Nuclear e0 = 40</th>
<th>Extended e0 = 40</th>
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<tr>
<td>1.0</td>
<td>2.4</td>
<td>1.0</td>
<td>2.6</td>
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<td>3.0</td>
<td>2.0</td>
<td>3.4</td>
<td>2.0</td>
</tr>
<tr>
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<td>3.6</td>
<td>3.0</td>
<td>4.2</td>
<td>3.0</td>
</tr>
<tr>
<td>4.0</td>
<td>4.2</td>
<td>4.0</td>
<td>5.0</td>
<td>4.0</td>
</tr>
</tbody>
</table>

Source: Burch 1972:96, table 2.1

Note that a low average life expectancy at birth
reflects a high rate of infant mortality (total life ex-
pectancy is of course much higher for those who sur-
vive childhood). A case can be made that e0 was
greater than 20 years in ancient Mediterranean popu-
lations—perhaps as high as 25 or 30, although cer-
tainly not as high as 40. But Burch’s calculations are
simply intended to provide the lower and upper limits
of family size in relation to mortality, and they show
that the family sizes produced when e0 is 40 are only
slightly larger than those produced when e0 = 20, so
the impact of a higher estimate of life expectancy within
the plausible preindustrial range of 20–30 is not
great. Note also that Burch assumes a mean age at
marriage for women of 20, which may be too high for
many ancient Mediterranean societies. A younger
average age at marriage tends to produce a higher
birthrate, but this can accounted for simply by choos-
ing a higher GRR from Burch’s table. Even assuming
a very high GRR of 4.0, his estimates show that ex-

tended family size would be only 8–10 if e0 were as
high as 25–30. These figures are confirmed by avail-
able census data (discussed below) which indicate
that joint families consisting of more than seven or
eight persons were rare under preindustrial Medi-

The Roman social historian Richard Saller (1994)

presents similar results concerning family size, esti-

mating relatively small numbers of living kin for
people in the ancient Mediterranean region during the
period of the Roman empire (see tables 2 and 3 be-

low). Saller’s figures are based on Coale and De-
meny’s (1983) “Model Life Table Level 3 West,”

which assumes an average life expectancy at birth for
females of 25 years (which corresponds to a slightly
lower e0 for males of 22.852; ibid., p. 43). This life ex-
pectancy falls in the middle of the range of 20–30
years that is well documented for preindustrial soci-
eties, but it might still be too high an estimate in light
of census data from Roman Egypt that fit “Model
Life Table Level 2 West,” which assumes a some-
what lower average life expectancy at birth for fe-

males of 22.5 years (20.444 for males; see Bagnall
and Frier 1994:87; Saller 1994:67). Saller’s more
generous estimate is advisable here, however, in or-

der to be sure that we are not biasing the calculations,

for our concern (and his) is to emphasize the dramatic
effect of preindustrial levels of mortality in reducing
family size and thus the proportion of three-
generation families, an effect which is strikingly evi-
dent even when e0 is as high as 25 (see Saller
1994:12–25 for a detailed defense of “Level 3 West”
in light of Roman-period evidence).5

Saller’s results are even more useful than Burch’s
calculations for our purposes because Saller uses the
survival rates predicted in “Model Life Table Level 3
West” in a computer simulation of the “kinship uni-
verse” produced by successive marriages, births, and
deaths (see table 2 below). In calculating these fig-
ures, the total population is assumed to be stationary
(never increasing nor decreasing); that is, NRR is

5 Coale and Demeny (1983) calculated their model life

tables by combining and extrapolating detailed census data

from actual populations around the world. These model
tables are intended for use as predictive tools in situations

where reliable census data do not exist. They chart the pre-
dicted distributions of both female and male mortality by
age for different “regions” (North, South, East, West) and

for different underlying rates of female mortality, starting
with “Level 1” (e0 = 20), “Level 2” (e0 = 22.5), “Level 3”
(e0 = 25), and so on, in 2.5 year increments. “Model West”
is the most generic of the four model tables and is designed
to be used when no data exist that permit a choice among
the four tables. Note, however, that Model West has been
shown to fit quite well the census data from Roman Egypt
(Bagnall and Frier 1994), so there is empirical evidence
that it is a suitable choice for ancient Mediterranean con-
texts.
1.0. To maintain replacement fertility when $e_0$ is 25, GRR must be ca. 2.5, or an average of five births for every woman who survives her childbearing years (Saller 1994:42). This average is plausible for the preindustrial Mediterranean, encompassing as it does a wide range of variation in individual fertility, including women who never marry, women who are sterile or become sterile, and women who bear ten children or more. The basic assumption here of a stationary population is reasonable because a slight increase or decrease in total population over time (0.1–0.2%) has little impact on the simulation of the kinship universe, whereas the existence of a significant overall percentage increase or decrease, which would have had dramatic effects within a few generations, is not supported by the available evidence (and it is generally accepted that most ancient populations grew very slowly, if at all).

Table 2 simulates an “ordinary” population versus the “senatorial” population shown in table 3 below. In Saller’s terminology, the ordinary population has higher average ages at first marriage (20 for women and 30 for men) as opposed to the lower average ages at first marriage (15 for women and 25 for men) attested for the Roman senatorial class (these estimates are based on data from tombstone epitaphs and on anecdotal literary and legal evidence). The “senatorial” earlier-marriage model is better for our purposes than the “ordinary” later-marriage model because the lower average ages at marriage are more likely for pre-Hellenistic Levantine populations, in keeping with ethnographic evidence from traditional Middle Eastern contexts as well as cuneiform textual evidence from Mesopotamia dating to the first millennium B.C. (see M. Roth 1987). The Roman Egypt census data also indicate that both men and women married somewhat earlier on average than in Saller’s ordinary population, although this difference within

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<th>15</th>
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<th>25</th>
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<tr>
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Source: Saller 1994:51, table 3.1.d

In Saller’s terminology, the ordinary population has higher average ages at first marriage (20 for women and 30 for men) as opposed to the lower average ages at first marriage (15 for women and 25 for men) attested for the Roman senatorial class (these estimates are based on data from tombstone epitaphs and on anecdotal literary and legal evidence). The “senatorial” earlier-marriage model is better for our purposes than the “ordinary” later-marriage model because the lower average ages at marriage are more likely for pre-Hellenistic Levantine populations, in keeping with ethnographic evidence from traditional Middle Eastern contexts as well as cuneiform textual evidence from Mesopotamia dating to the first millennium B.C. (see M. Roth 1987). The Roman Egypt census data also indicate that both men and women married somewhat earlier on average than in Saller’s ordinary population, although this difference within the Roman empire may reflect the unique Egyptian institution of brother-sister marriage, as Saller points out. But the senatorial estimates in table 3 are less useful than they might otherwise be because, despite the longer child-bearing period permitted by earlier marriages, Saller assumes a substantially lower GRR for the senatorial population (2.29) than for the ordinary population (2.44) in order to keep the total population stationary (NRR = 1.0). The senatorial GRR used in table 3 seems too low for our purposes, but if we assume a nonstationary, slowly increasing population (as in Roman Egypt, where the census data suggest that the annual growth rate was ca. 0.2% and GRR was ca. 2.9; Bagnall and Frier 1994:139), we can adjust the senatorial figures upwards to allow for a higher birthrate. At any rate, it is useful to compare Saller’s “ordinary” (later-marriage, higher birth-rate) and “senatorial” (earlier-marriage, lower birth-rate) populations in order to see how individual kinship universes are affected by these variables. The
most important factor, in any case, is the disparity between male and female ages at marriage assumed in Saller’s simulations, reflecting the “Mediterranean” pattern of late male and early female marriage that is well attested in later sources (see Laslett 1983) and is also attested in the census data from Roman Egypt (Bagnall and Frier 1994:118) and in the Neo-Babylonian marriage agreements studied by Martha Roth (1987).

Table 3. Mean Number of Living Kin for “Senatorial” Roman Males

<table>
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<th>Kin</th>
<th>0</th>
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<th>10</th>
<th>15</th>
<th>20</th>
<th>25</th>
<th>30</th>
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<th>40</th>
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<th>55</th>
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<tbody>
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<td>Wife</td>
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<tr>
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<td>0.4</td>
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<tr>
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<td>0.8</td>
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<td>0.6</td>
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<td>0.9</td>
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<td>0.9</td>
<td>0.8</td>
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<td>Son</td>
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<td>Daughter</td>
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<tr>
<td>Paternal grandfather</td>
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<td>Grandson</td>
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<tr>
<td>Maternal aunt</td>
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<tr>
<td>Paternal aunt</td>
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Source: Saller 1994:57, table 3.2.d

From these simulated kinship universes, Saller concludes that few married men had a living father and few children had a living paternal grandfather; hence the three-generation joint family represents a relatively brief phase of the household lifecycle, when it occurs at all. In addition, the computer-generated estimates of the mean number of living kin show that adult men between the ages of 20 and 50 had an average of only 0.5–0.9 living brothers (initially 0.9 brother at age 20, decreasing to 0.5 by age 50). In other words, in a given family at most two sons on average would survive to adulthood and thus be eligible to marry and expand the joint-family household under a system of patrilocal residence, ensuring that the total size of such a household remained relatively small. Moreover, the typically early death of the senior male meant that at any one time the proportion of joint-family households in the entire population was only around one-third, even if patrilocal postmarriage residence was universally practiced whenever possible.6 It is hard to pin down because a joint-family household (albeit not three-generational) is still possible after the senior male’s death if the surviving sons continue to live together—assuming there is more than one surviving son. It is hard to judge how frequent this sort of fréreche was in the preclassical Levant, but comparative evidence suggests that such an arrangement is normally unstable and the younger brother soon moves out, or else the house is repartitioned to create separate domestic units by blocking up doorways, as is evident, for example, in Late Bronze Age

have a living grandfather is ca. 30–40% (this proportion declines from 45% at birth to 20% at age 10 to 10% at age 15 and only 4–5% at age 20). But a man will be 60–65 years old before the mean age of his grandchildren reaches 5, and very few who have attained grandparenthood will survive much beyond that age. A proportion of one-third of all families being three-generation joint families at any one time is also in keeping with the census data from Roman Egypt and Renaissance Tuscany, which are discussed in more detail below (although the analyses of these data do not neatly distinguish the three-generation joint family from the “extended conjugal family” and “multiple conjugal families” household types for which summary figures are given).

6 According to Saller’s figures (table 3.1.b, p. 49, and table 3.1.e, p. 52), the proportion of children aged 5 years who
Ugarit (see chapter 13.2). For Roman Egypt, Bagnall and Frier (1994:60) calculate that approximately 36% of all households were occupied by an “extended conjugal family” (15%) or by “multiple conjugal families” (21%, including fréreches), as opposed to 43% inhabited by a “single conjugal (nuclear) family” and 16% inhabited by solitary persons (who were usually of advanced age and “most often the sole survivors of their families”; note that “the proportion of solitaries is exaggerated since short [census] returns have a better chance of being well preserved” [ibid.]). Nuclear-family households predominate, but because extended- and multiple-family households are larger than nuclear-family households, it is perhaps more significant that at any given time in Roman Egypt a majority of all family members (58%) lived in a non-nuclear complex household.7

In light of both Burch’s and Saller’s demographic calculations, it is safe to say that patrilineally extended families in an ancient Mediterranean population would have had no more than 7 members on average, even assuming early female marriage and frequent births.8 This estimate does not allow for polygamy, however, nor for the presence in the household of more distantly related dependent kinsfolk (e.g., widows and orphans), slaves, and “resident aliens” or clients (the biblical gerîm). It is impossible to know how common polygamy was in ancient Canaan or Israel, for example, but in view of the need to pay a brideprice (Heb. mōhar), it is likely that polygamy was practiced only by the wealthiest householders. Similarly, the evidence we possess suggests that the number of slaves was not great in most households until the Roman period. Still, in interpreting Bronze and Iron Age evidence it seems advisable to increase the theoretically derived estimate of 7 persons per joint family quite generously, to an average of 10 persons per joint-family household, in order to allow for the addition to the coresident group of multiple wives, as well as more distant kin, servants, and other quasi-kin dependents. This figure is comparable to the average household sizes (as opposed to just the family sizes) calculated by Bagnall and Frier (1994:68, table 3.3) for households consisting of multiple conjugal families in Roman Egypt, which ranged in size from an average of 9.38 persons (in villages) to 10.36 persons (in metropoleis)—remembering that their “multiple families” category includes only the most complex households.

This purposely high average of 10 persons per joint-family household will be used below in my analyses of houses and households in ancient Israel and Ugarit. Likewise, I will use a high average of 5 persons per nuclear-family household, as compared to the demographically predicted average of 3.5 per nuclear family.9 Again, I believe it is better to err on the side of caution, choosing the upper limit of what is demographically possible, since my intention in what follows (especially in chapter 13) is to demonstrate that the archaeological and textual evidence from the Late Bronze and Iron Age Levant supports the view that the joint-family “Mediterranean” type of patrilocal household was the cultural norm in Canaan and Israel, even in urban settings, in opposition to the opinion often stated by archaeologists that the small size of typical dwellings indicates that nuclear families were the norm. Even at the upper limit of what is demographically possible, the latter view greatly overestimates the size of patrilocal joint families, failing to take into account the effects of a high

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7 The same is true of fifteenth-century Tuscany (discussed below); see Herlihy and Klapisch-Zuber 1985:283.
8 This is a generous estimate which implies an improbably high birthrate, for Burch calculates only 7.6 with GRR = 4.0 and e₀ = 20 (see table 1). Although e₀ = 20 may be too low an estimate, this is more than compensated for by the very high GRR of 4.0. A more likely figure is produced by summing the relevant kin of a male who survives to grandparenthood as predicted by Saller’s simulations, assuming that all his sons and daughters have married. For a male who survives to age 70 in the late-marrying “ordinary” population (table 2) this gives 1 ego + 0.5 wife + 1.1 sons + 1.1 daughters-in-law (not shown in the table) + 1.6 grandchildren = 6.3 persons (note that only half of the grandchildren estimated in the table are included because ego’s married daughters and their children reside elsewhere). The comparable calculation for a male who survives to age 60 in the early-marrying “senatorial” population (table 3) is 1 ego + 0.9 wife + 0.8 sons + 0.8 daughters-in-law + 1.2 grandchildren = 4.7 persons (note that age 60 is used here instead of 70 to allow for the same relative lapse of time between a man’s marriage and his sons’ marriages, because marriage occurs five years earlier on average in the senatorial population). A higher estimate of ca. 7 persons might be supported, however, if we assume a nonstationary, slightly growing population and increase Saller’s estimates of GRR accordingly, especially for the senatorial population. We can compare this to the census data from Roman Egypt (Bagnall and Frier 1994:68, table 3.3), which indicate that the average size of “extended” families (15% of all households) ranged from 4.24 (rural) to 4.75 (urban), the average size of “multiple” families (21% of all households) ranged from 8.79 (rural) to 8.64 (urban), and the average size of “conjugal” families ranged from 3.31 (rural) to 3.73 (urban). Taking the higher figure in each case and combining Bagnall and Frier’s “extended” and “multiple” categories into the more general category of “complex” family to produce a single weighted average, we obtain an average of 7 persons.
9 E.g., Burch (1972:96, table 2.1) calculates that the mean nuclear-family size is 3.6 when GRR=3.0 and e₀=20.
mortality rate, which are so well demonstrated in preindustrial Mediterranean census data and in the demographic models derived from them.  

3. Three Cases: Roman Egypt, Renaissance Tuscany, and Ottoman Syria

In order to flesh out our picture of typical households in preindustrial Mediterranean settings, it is worth considering three of the best-documented cases currently known: Roman Egypt, Renaissance Tuscany, and Ottoman Syria. The striking similarities among these three examples, so widely separated in space and time, lends support to the notion of a durable "Mediterranean" type of domestic group organization as the product of a self-reinforcing confluence of deeply rooted symbolic traditions and common environmental factors. In particular, in all three cases there is clear evidence that patrilocal joint-family coresidence was common in both urban and rural settings, within the constraints created by a high rate of mortality. It is true that the proportion of complex households is usually somewhat lower in cities than in villages; nonetheless, there is no sharp urban-rural dichotomy in terms of household composition, in contrast to the functionalist view of urbanism which has become so deeply rooted in modern scholarship.

In Roman Egypt, for example, the proportions of household types in metropoleis are as follows: 26.4% are complex households (including both the "extended conjugal family" and "multiple conjugal families" types), 51.4% are simple households (the "conjugal family" type), and 22.2% are households consisting of solitary individuals or multiple individuals but with no conjugal family (e.g., coresident unmarried siblings). In contrast, the proportions in villages are 43.2% complex, 36.8% simple, and 20.0% solitary/no family (Bagnall and Frier 1994:67, table 3.2). Note, however, that the greatest difference lies not in the "extended conjugal family" category (11.1% in metropoleis vs. 17.9% in villages), but in the "multiple families" category (15.3% in metropoleis vs. 25.3% in villages), corresponding to the higher incidence of simple conjugal-family households in urban settings (51.4% in metropoleis vs. 36.8% in villages). In the terminology used here, an "extended conjugal family" typically consists of a single conjugal couple and their children plus a widowed parent and/or the unmarried sibling(s) of the household head, whereas a "multiple families" household is typically a frèreche formed by the coresidence of married siblings (usually brothers) after the death of their father. Thus the higher incidence of complex households (especially multiple families) in villages can be attributed in large part to a stronger preference for frèreches in rural settings, and does not indicate a significantly weaker preference for patrilocal joint-family coresidence in urban settings. Indeed, the proportion of complex households in urban settings in Roman Egypt (ca. 26%) is close to what one would expect on demographic grounds for vertically extended patrilocal joint-family households (assuming that frèreches are relatively rare, as they are in many traditional Middle Eastern contexts), especially given the lower life expectancy in the cities as opposed to the villages (Bagnall and Frier 1994:87; Bagnall 1993:50). Furthermore, as Bagnall and Frier point out, in the simple conjugal-family households documented in the census texts, only rarely is the married couple young:

Normally the spouses are of more advanced age; the average age of husbands is 45.9 (26 ages), and of wives, 37.3 (27 ages). This indicates that newly married Egyptian couples did not regularly leave their parents' home and form new households, as was widely the case in premodern Europe. Instead, the Egyptian conjugal family more commonly resulted from attrition, through the death of parents and other kin. [Bagnall and Frier 1994:61]

The similarity between urban and rural households in terms of postmarriage coresidence and thus household composition is probably related to the fact that many urban households remained heavily involved in agriculture and other forms of primary production, and thus had much in common economically with their rural counterparts, despite the higher incidence of specialized occupations within the largest settlements. The importance of agriculture for urban households has been noted by Roger Bagnall (1993) with respect to Egyptian cities during the late Roman period, which are documented by large numbers of papyri that record everyday transactions. These mundane documents were preserved in the dry climate of Egypt to an extent that is rare in the ancient world, with the exception of earlier cuneiform archives written on clay tablets. The papyri, together with archaeological evidence, indicate that most houses in the cities of Roman Egypt were two-story mudbrick structures with inner courtyards, as was common.

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10 Note that if we assume that one-third of all households are joint with 10 persons on average, and the remaining two-thirds are nuclear with 5 persons on average, we obtain an overall weighted mean of 6.7 persons per household, combining all household types. Similarly, with 7 persons per joint family and 3.5 persons per nuclear family, we obtain an overall weighted mean of 4.7 per family. But these averages mask the difference between the two distinct household types.
both earlier and later in the Near East. Bagnall observes that:

Virtually every house mentioned in a document had a courtyard and a well, along with storage space for animal feed. Hermopolis and Oxyrhynchus were full of animals: cattle, pigs, donkeys, camels, no doubt goats and the ubiquitous pigeons as well. Quite apart from the disposition of human wastes, then, it is small wonder that leases routinely specify that the house is to be turned back to the lessor free of dung at the end of the term, or that “a vacant dung-covered lot” turns up in the Panopolite register of houses. The inhabitants of Egyptian cities kept the sounds and smells of the country with them, except perhaps for the smell of vegetation. Though some pharaonic towns certainly contained plots of land for growing crops, the more crowded quarters of their Roman descendants did not. . . . The town was, however, immediately surrounded by cropland, close enough that the urban dweller could work such plots and bring their produce into the city for processing; such was the case of oil-makers at Panopolis who leased olive orchards around the city. [Bagnall 1993:50]

Bagnall (p. 53) estimates that the total population of an Egyptian city in this period was on the order of ca. 16,000 persons on average. This is based on a rather low population density of 200 per hectare in an average urban area of 80 hectares; but even if we were to double the population estimate, it is clear that a substantial proportion of urban households could have relied on basic foodstuffs which they produced for themselves within walking distance of the town, whether these town dwellers were landowners or tenant farmers—especially in the Nile River environment in which intensive cultivation and multiple harvests are possible. It is difficult to determine the precise proportion of the urban population which was directly involved in agricultural production, as opposed to simply processing grain and other raw agricultural commodities for domestic use. Bagnall (p. 71) estimates that only 6–10% of urban householders were large landowners who supported themselves entirely on the income from their country estates. But in addition to this wealthy elite, an equal number of households owned small landholdings which they rented out or cultivated themselves in order to supplement their income, and beyond that there is an unknown number of other urban residents who farmed rented land in the vicinity of the town or who worked as day-laborers on a seasonal basis, especially at harvest time. In general, it seems likely that a large proportion of the inhabitants of the cities were wholly or partially reliant on agriculture and animal husbandry, regardless of their occupational specialties. By and large, then, urban households were not disembodied from the rural agrarian economy but were thoroughly integrated into it, whether directly or indirectly, calling into question any strict qualitative separation between urban and rural modes of life. On similar grounds, I argue below that agricultural activity was common among the “urban farmers” of Late Bronze Age Ugarit, most of whom were not full-time professional specialists.

A comparable situation is evident in the much later Italian case, although the differences between urban and rural households, in terms of both their composition and means of livelihood, were more pronounced in fifteenth-century Florence and its hinterland, no doubt because we are here dealing with a more highly monetized market-oriented economy. The abundant and detailed data from the Florentine census of 1427 were analyzed in a landmark work by David Herlihy and Christiane Klapisch-Zuber entitled Tuscans and Their Families (1985). The tax collectors of Renaissance Florence collected detailed information from some 60,000 households comprising more than 260,000 persons, and it is striking how well the proportions of households of various types in fifteenth-century Tuscany match the proportions in Roman Egypt more than a millennium earlier, with roughly one-third in the “extended conjugal family” (11%) or “multiple conjugal families” (19%) categories, and 55% in the “simple conjugal family” category (Herlihy and Klapisch-Zuber 1985:292; cf. Bagnall and Frier 1994:60; 171ff.).

This bolsters the argument that Roman Egypt was not unique in its demographic characteristics but was typical of the preindustrial Mediterranean world. As in Egypt, Tuscan households were somewhat smaller on average in the towns than in the countryside. In towns, household size was positively correlated to wealth, and the lower average household size there reflects the higher proportion of poor servants, laborers, and widows who lived alone in urban as opposed to rural settings. In the countryside the correlation of household size and wealth was weaker, and household sizes were also larger on average, because in rural settings there was a higher proportion of share-

11 See also the detailed study of Landowners and Tenants in Roman Egypt by Jane Rowlandson (1996), which is based on numerous papyrus documents found in the trash dumps of the city of Oxyrhynchus—agricultural documents whose very existence in an urban context indicates the economic importance of agriculture for urban residents.

12 As Bagnall says (1993:149), there is no reason to doubt that most urban smallholders, like their village counterparts, farmed their own fields, given the proximity of their landholdings to the cities in which they lived.
predominantly two-story houses. He calculates that per ha, which is plausible in a densely settled city of the eighteenth century, yielding a density of 275 mates that the population was 100,000 Cairo in importance, Abraham Marcus (1989) estimated the city of Aleppo was constituted (see figure 3 below; see also Gaube and Wirth 1984; Gangler 1993). Marcus describes this neighborhood structure as follows:

Most basic goods and services were available to [townspeople] close to home, usually within their own neighborhoods. In this and other respects the neighborhood unit (mahalla) formed a central feature of their physical landscape and social life. In the middle years of the [eighteenth] century Aleppo had eighty-two distinct quarters, each made up of several adjoining streets and their buildings, and identified by its own name. These small cells, which varied in size and social composition, usually accommodated one hundred to two hundred households. No walls marked the boundaries of a neighborhood to differentiate it physically from its surroundings; the backs of the buildings on its periphery were actually attached to those of neighboring quarters. That, however, did little to dilute the distinct identity of quarters. Heavy gates, shut every night and guarded by watchmen, controlled entry into each of them. Each had its own appointed headman, and acted as a collectivity responsible before the authorities for collecting taxes, maintaining security, and looking after public facilities within its precincts. [Marcus 1989:285]

Turning now to the latest case, Ottoman Syria, which is also geographically closest to the ancient Levant, we can see a similar pattern of household sizes and composition, reflecting a deeply rooted tradition of patrilocal joint-family coresidence operating under preindustrial demographic and economic conditions. If anything, urban households in this region seem to have been even larger and more complex, on average, than in the earlier examples. In his study of Ottoman Aleppo, a major metropolis 365 hectares in area and second only to Istanbul and Cairo in importance, Abraham Marcus (1989) estimates that the population was 100,000–120,000 in the eighteenth century, yielding a density of 275–330 per ha, which is plausible in a densely settled city of predominantly two-story houses. He calculates that in the middle decades of the eighteenth century Aleppo had some 11,000–12,000 houses, 10,000–11,000 taxed households, and a total of 14,000–16,000 households. . . . With an overall population of 100,000–120,000 in the eighteenth century, the average household size in Aleppo would have revolved therefore around 7. This relatively high figure is quite in keeping with the observed features of local families: the extended form of many households; the common tendency of single adults to reside with relatives; the absorption of orphaned relatives and aged parents into existing units; the presence of live-in domestics and, among the wealthy, of slaves, concubines, and a plurality of wives; and, above all, the relatively large number of children, of which there were on average between four and five per household. [Marcus 1989:340f.; see also p. 200f.]

Contrary to a common misconception, these neighborhoods were not socially homogeneous urban sub-units, each occupied by members of the same ethnic or occupational group, or by people of uniform wealth or status (ibid., pp. 315ff.). Rather, most such neighborhoods were socially mixed microcosms of the city as a whole. They were characterized by frequent and often intense personal interactions among their residents, like small villages within the city. Their size, typically on the order of 1,200–1,500 inhabitants in an area of 4–5 ha, did not exceed what was feasible to preserve a face-to-face community. Thus, although such an urban community was integrated by means of a complex and, from a bureaucratic point of view, rather messy array of highly personalized social and economic relations, centered on relations with the hereditary neighborhood headman or šâyh, this kind of neighborhood organization was admirably suited to the political and economic exigencies of preindustrial life in a populous city. Indeed, the internal structure of these neighborhood communities mirrored the patron-client system that was characteristic of the wider society, albeit on a smaller scale. Here is another case in which the traditional “Islamic city” should not be explained in terms...
of a “top-down” formal rationality that starts from the abstract concept of the city as a whole, viewed as an hypostatized collective entity with various functional subdivisions, but should rather be understood in terms of the “bottom-up” local logic of interacting individuals, households, and neighborhoods, which constitute the city in an ascending patrimonial hierarchy (see chapter 6.3).

Figure 3. Aleppo in the eighteenth century A.D. (after Marcus 1989:280, fig. 8.1)
A similar urban pattern is well attested in Damascus in the medieval and Ottoman periods (see the detailed bibliographic essay, which focuses on pre-Ottoman Damascus, in Humphreys 1991:228–54). In their study of Damascene probate inventories dating to ca. A.D. 1700, Colette Establet and Jean-Paul Pascual (1994:16) give an estimate of 60,000–65,000 for the total population of Damascus at that time. Their analysis implies a somewhat lower fertility rate and thus a lower average family size in Damascus than Marcus calculates for eighteenth-century Aleppo, but their results are otherwise congruent with his results.

In particular, in both Damascus and Aleppo there is evidence of “almost universal marriage; early age at first marriage; frequent breakup of families due to divorce or the premature death of spouses; common remarriage; high fertility; and high mortality, of children and adults alike” (Marcus 1989:196). Polygamy was practiced, but it was relatively rare: only 10% of Muslim men in Damascus had more than one wife (Establet and Pascual 1994:55), and polygamous marriages are not often mentioned in the court records of Aleppo (Marcus 1989:200). The effect of polygamy on typical household size and composition would therefore have been slight.

The structural similarity between urban households and neighborhoods and their rural counterparts in the preindustrial Levant can be seen by comparing the document-based reconstructions of social phenomena in Ottoman Aleppo and Damascus to ethnographic studies of traditional village life in Palestine during the nineteenth and twentieth centuries. There we see the same pattern of patrilocal joint-family households integrated into a self-governing community, which might constitute the entire village, or just one neighborhood within the village if the settlement was large enough. Such a community is organized under a headman according to patrilineal kinship (real or fictive) as a “patronymic association,” called a hamīlā in Palestinian Arabic (Cohen 1965; Eickelman 1989:155). As I argue below, these lines of evidence provide a useful analogy to the ancient situation, both urban and rural, in the Levant.

This is not to say that there were no differences between town and country, or that peasant life in Palestine and Syria remained self-contained and unchanged from antiquity until the twentieth century. As a number of anthropologists have emphasized, we should not imagine, as the ahistorical structural-functionalist ethnographies of a previous generation tended to do, that the way of life practiced in peasant villages has remained sealed in a time-capsule for millennia, without being affected by broader political and economic changes (see, e.g., Eickelman 1989: 48–53). Many anthropologists now take a more historical approach to the traditional kinship systems and the associated political and economic strategies that they study on a local scale. Henry Rosenfeld, for example, has criticized the assumption that Middle Eastern rural society remained unchanged until quite recently, arguing that:

the very idea of a static tradition village pattern, or a traditional baseline, is incorrect. Those who believe in the Immovable East with an unchanged peasantry existing in some places for five to six thousand years must prove their thesis. They must prove that there have been no population movements; no interchange of nomadic, village, and urban peoples; no intercontinental commercialism, no creation of new peasantry out of tribal elements or out of declased urban elements; and no creation of tens of empires and hundreds of petty states which based themselves on the peasantry. This is a long, changing and complex tradition which needs to be unraveled. . . . Whatever rural internal patterns continue to exist, e.g., marriage, reciprocity, lineage heads, village councils, must be explained in terms of sociohistorical causation. [Rosenfeld 1972:55]

Rosenfeld illustrates his point in a subsequent article dealing with Arab peasant kinship, arguing that peasants are, historically, “tribal peoples who, as rural cultivators, in one way or another had been incorporated into, or made to serve, state ends” (Rosenfeld 1983:154). In the Middle East this process extends far back in time. As tribesmen are transformed into peasants—as “personal relations” give way to “political relations”—Rosenfeld asks: “What happens to ‘kinship’ and community? Are they carried over from tribal society, held as an autonomous internal system while the wielders of power continue, with their business of taxation, oppression and bureaucratic impositions? Or, over time, do kinship and tribal solidarity simply deteriorate, and in peasant society become ‘vestigial,’ or vitiated replicas of what were once the heart of life?” (p. 155).

Rosenfeld suggests a third possibility: that the kinship relations evident in peasant society “reflect the
type of state and its political and economic level—what has been imposed on (generally as administrative-fiscal measures), and what has been transformed irrevocably into, a peasant level of administered, fiscalized, politicized kin relations” (ibid.). Within a state-organized society, peasants may indeed “retain or reconstruct over time what is viable and possible in ‘personal’ and joint relations” (ibid.), but the form and meaning of kinship will be determined by the overall political economy.

Rosenfeld proceeds to discuss the ethnographic overall political economy.

In all periods, village lineages become enmeshed, politically and economically, in the wider state society:

Wealthy kin keep their wealth within the extended family through patrilateral parallel cousin marriages, isolate themselves from their descent line kin, move out of the village and become, as said, patrons. By turning the endogamous marriage pattern into a class marriage . . . they terminate whatever prior, meaningful lineage sense had been retained in descent and kin marriage rules. . . . [The] new “clan-lineage” is constantly, “artificially” being imposed and contained by the . . . state, with (in Ottoman Palestine) the top part of the stratified lineage constantly joining the class element outside the village within which its interests lie—tax-farming, office-holding, and so on—making the village the habitat of the most reduced groups, destined to struggle amongst themselves for honour, recognition, factional links, and ties of clientage. That is, the local patrilineage is a function of those who have to remain on the land. Its extensions outside the village lead to those who profit from the lineage, and from the land. [Rosenfeld 1983:169]

Several questions can be raised concerning Rosenfeld’s approach, however. His dichotomous “class” terminology is inappropriate for a kin-oriented preindustrial peasant society, which is better characterized in terms of what Max Weber called “status groups”; that is, in terms of dyadic or “vertical” patron-client relationships (cf. G. Foster 1967) instead of “horizontal” social classes (see chapter 4.2). Here Rosenfeld falls into the functionalist trap of overemphasizing the economic opposition and structural dichotomy between the urban state elite and exploited rural villagers. According to his own analysis, powerful local lineage heads mediated between ordinary villagers and the state (especially in remote or inaccessible regions), making for a continuum of urban-rural socioeconomic gradations rather than sharply differentiated self-conscious classes. It is true that such patron-client relationships are asymmetrical, but lineage competition and changing fortunes imply a greater variety in life-chances than a simple class analysis would suggest, and the possibility should not be ignored that clients on the low end of the relationship were not only “exploited” but were also able to exploit their ties to patrons. In terms of long-term historical developments in the Middle East, moreover, one may dispute Rosenfeld’s assumption that archaic “prestate” village society was egalitarian and collective, implying that the advent of the “state” mode of organization alone produces
inequalities (although local inequalities may well increase considerably in the context of state-level political relations).

Despite these criticisms, Rosenfeld is correct to stress that rural social patterns in the Levant did not simply persist, hermetically sealed and unchanging, as states rose and fell around them. This does not mean that peasant institutions were created by the state de novo (as Rosenfeld himself points out), or that earlier tribal characteristics vanished without a trace. There was, however, an inevitable interaction and adaptation as large-scale political organization impinged on tribally organized villagers. In my view, this large-scale mode of organization was not necessarily qualitatively different from smaller-scale tribal society, contra Rosenfeld, because centralized states were often built on the same symbolic “kinship” (i.e., patrimonial) basis, especially in the archaic or “pre-Axial” period, through the metaphorical extension of patrilineal kinship among a much larger social group, as I have argued above in chapter 4. But Rosenfeld rightly regards kinship institutions as symbolically malleable and thus historically contingent phenomena which adapt to changing political and economic conditions. This more historical approach provides a welcome contrast to the sort of functionalist ethnography that treats the village as an isolated and unchanging entity with an internal functional logic that can be understood without reference to its contingent historical context.

Speaking of the demise of functionalism in social anthropology, and in Middle Eastern anthropology in particular, Dale Eickelman (1989:49) remarks that “[f]unctionalism appeared to account much better for relatively simple, small-scale societies than for the larger-scale, historically known societies which anthropologists typically encountered in the Middle East. Only by treating Middle Eastern villages and tribes as if they were small-scale, relatively isolated societies was it possible to make sense of them in terms of functionalist theory. Villages and tribes were treated as closed social worlds.” But attempts to apply such a theory to complex and long-documented Middle Eastern societies have revealed functionalism’s inability to provide meaningful accounts of historically known societies. As Eickelman (ibid.) says, “ahistorical assumptions concerning Middle Eastern societies are profoundly inappropriate” because:

Accordingly, both Rosenfeld (1972) and Eickelman (1989:56–67) criticize the ahistorical functionalist tendency of many Middle Eastern village ethnographies of the 1950s and 1960s (e.g., Gulick 1955; Sweet 1969; 1974; Fuller 1969; Peters 1963; Antoun 1972a; Lutfiyya 1966—note that earlier ethnographies were more purely descriptive and did not attempt sociological “explanation” at all, functionalist or otherwise, although they do provide a valuable fund of detailed information; e.g., the studies of traditional Palestinian society in the late Ottoman and British Mandate periods in Dalman 1933; 1942 and Granqvist 1931). Eickelman remarks that Emrys Peters, for one, reevaluated his earlier work on Muslim villagers in south Lebanon, and “saw the error of his first analysis as the attempt to fit all the facts of village life into a single pattern. In his reanalysis, he concluded that the ‘system’ the anthropologist looks for is necessarily open-ended and must be explained at least in part by looking for historical transformations” (Eickelman 1989:66; see Peters 1972).

But such historical transformations are at some level symbolic transformations, among which is, notably, the extension to larger social and political groups of the age-old symbol of the “house of the father,” a symbolic innovation which has characterized many different Mediterranean societies. For that reason, our study of traditional Mediterranean houses and households must pay attention to the dialectic between the history of this stubborn symbol—the history of the patriarchal household as a basic paradigm of social order—and its “prehistory” in a set of predictable demographic facts.
Chapter 8. Demography and Domestic Space in Ancient Israel

At this point it is worth applying to Iron Age Israel some of the insights acquired from Mediterranean households of later periods. The available archaeological and textual evidence confirms that patrilocal joint-family coresidence was strongly preferred by the ancient Israelites—within the constraints of the prevailing mortality regime—even in densely populated walled towns. There was no significant urban-rural dichotomy, in my view, in this or any other aspect of Israelite society, which remained largely agrarian throughout the Iron Age. Important social changes did occur at the end of the Iron Age, in the aftermath of the Assyrian conquest in the late eighth century B.C., especially with respect to the economic, political, and religious role of endogamous “clans” or lineages (Heb. mispâhōt; sg. mispāhā). But these changes affected towns and villages equally, as clans were uprooted from their traditional domiciles in walled and unwalled settlements, and as a few urban centers were greatly enlarged—often forcibly by the Assyrian conquerors—by the influx of uprooted local populations and deportees from elsewhere.

In a future volume I will discuss the changes in the traditional clan structure of Israelite society which were triggered by the massive Assyrian deportations and resettlement of populations. But here the focus will be on individual households and their organization into clans in the “classic” Israelite period before the social and religious revolutions of the seventh and sixth centuries B.C. The basic features of Israelite society in this period are closely comparable, in my opinion, to those of the earlier Canaanite society of the second millennium B.C., which is best attested by the archaeological and textual evidence from Late Bronze Age Ugarit. Thus it is helpful to introduce the Israelite evidence here, before proceeding to discuss Ugarit and the Bronze Age Near East below in Part Two.

What was the typical family size and household structure in ancient Israel, and how did these demographic features relate to the architecture of Israelite dwellings and the overall organization of Israelite towns and villages? The answers to these basic questions about demography and domestic space in ancient Israel are not as obvious or uncontroversial as they might first appear. In particular, it is striking that a number of archaeologists of Iron Age Palestine have arrived at conclusions that appear to be at odds with the view of the Israelite family long held by biblical scholars on the basis of textual sources. These archaeologists conclude from the relatively small size of excavated Iron Age houses that such dwellings were occupied by nuclear families rather than extended families (e.g., Shiloh 1980; Broshi and Finkelstein 1992; Holladay 1992; 1995). The biblical evidence, on the other hand, has usually been interpreted to indicate the existence of much larger extended-family households consisting of three or even four patrilinearly related generations—at least as a cultural ideal, even if it was not always realized in practice (for an overview of the biblical evidence, see Blenkinsopp 1997).

In my view, this apparent discrepancy between the textual and the archaeological data vanishes when our picture of the Israelite household has been corrected in three ways. First, on the basis of comparative ethnographic and historical data, we must raise our estimate of the number of inhabitants that could have been accommodated in a typical Israelite “four-room” or “pillared” dwelling. This adjustment will also require us to increase substantially the prevailing archaeological estimate of the density of occupation in Israelite walled settlements of the Iron Age, a revision that can be defended on comparative grounds using data from a variety of preindustrial walled cities in Syria and Palestine. Second, keeping in mind the biblical indications that the patriarchal joint-family household was a cultural norm in ancient Israel—as indeed it has been in many similar agrarian societies in the Mediterranean region and elsewhere—we must reduce our estimates of both the size and the frequency of three-generation joint-family households which may be expected under preindustrial conditions of mortality and fertility. Third, we must acknowledge the effects of the household lifecycle, discussed in the previous section, which resulted in a regular expansion and contraction of joint-family households as senior family members died and surviving adult sons formed their own households (assuming that the frèreche type of household was generally unstable and did not last long). A family’s space requirements varied considerably at different stages of the household lifecycle, so that at any one time we should expect a considerable range of sizes of both houses and households in Israelite settlements.
By taking these three factors into account, we can conclude that a typical Israelite joint-family household consisted of roughly 10 persons on average, or even fewer, as calculated above in chapter 7.2. This includes servants and more distantly related kin, with an average of only 7 persons, at most, in the joint family itself. Such households were readily accommodated in the larger Iron Age Israelite pillared dwellings, as we will see below. At any given time, moreover, two-thirds of all households were in the nuclear-family phase of the household lifecycle, and thus consisted of only about 5 persons on average, including servants and relatives.\footnote{These figures are not meant to be precise but to indicate plausible relative proportions of household types. The proportions used here (one-third joint and two-thirds nuclear) are based on the demographic calculations discussed above in chapter 7.2, and are corroborated by the census data from Roman Egypt studied by Bagnall and Frier (1994), which are shown to be broadly applicable in preindustrial Mediterranean settings by the existence of similar proportions in fifteenth-century Tuscany (Herlihy and Klapisch-Zuber 1985). The categories of “solitary” (single-person) and “coresident sibling/no conjugal family” households used in Bagnall and Frier’s study are abandoned here for the sake of simplicity, and the 21% of households which fall into these categories are equally apportioned to my “nuclear-family” type (combining here the “conjugal family” and “extended conjugal family” types, which constituted 58% of all Egyptian households) and to my “joint-family” type (Bagnall and Frier’s “multiple conjugal families,” which constituted 21% of all households). In any case, the proportion of “solitary” households (16%) is exaggerated in the Egyptian papyri because shorter census returns have a better chance of being preserved (see Bagnall and Frier 1994:60).} This results in an overall mean household size of 6–7, including both household types. There is a pronounced variation in excavated house sizes, and on demographic grounds we would expect a similar range of variation in actual household sizes wherever the patrilocal joint-family household is the ideal. In fact, the relative proportions of both house sizes and expected household sizes mirror one another quite closely (i.e., one-third joint family; two-thirds nuclear family), suggesting that the joint-family model is correct. In this way, the apparent conflict between current archaeological and textual interpretations is resolved and we gain a clearer picture of the size, structure, and lifecycle of the ancient Israelite household.

Having established the Israelite preference for joint-family households, which I will document in some detail below on both archaeological and textual grounds, the question arises as to whether any diachronic developments may be observed in Israelite demography and the organization of domestic space.

Many specialists, including both archaeologists and biblical scholars, have assumed that changes in Israelite political organization necessarily produced major changes in household structure and local economic organization over the course of the Iron Age, especially with the rise of a centralizing monarchical authority during the tenth century B.C. The conclusions that are reached in this matter have important implications for the interpretation of preexilic biblical literature and for the reconstruction of the history of Israelite religion. Unfortunately, the published archaeological evidence of domestic architecture is rather sparse for the earliest Israelite period, but on the basis of existing evidence it can be argued that, apart from an increasing density of occupation consonant with the organic development of walled towns and the concentration of a previously quite dispersed village population, family size and household structure did not change appreciably from the premonarchic to the monarchical period. There is no evidence that the social organization of Israelite households and clans changed significantly until quite late in the Iron Age, when it was greatly affected by the imperial conquests and mass deportations of the Assyrians and Babylonians.

1. The “Israelite House” and Agrarian Urbanism in Iron Age Palestine

Only a limited number of houses have been excavated in putatively Israelite sites dating to the Iron Age I period (ca. 1200–1000 B.C.), but fairly extensive residential areas have been uncovered at walled sites in Palestine of the Iron Age II (ca. 1000–600 B.C.), and dating to the eighth century B.C., in particular. Beginning in the twelfth century, the traditional Canaanite courtyard house gave way to a new house-type in Palestine. This is the so-called Israelite or “four-room” house (Shiloh 1970; 1978; 1987; Netzer 1992), although the term “pillared house” more accurately describes this type of dwelling, which sometimes had two or three rather than four major subdivisions in its floorplan (see Stager 1985a:17; Mazar 1990a:485ff.). Its distinctive feature is not the number of rooms but the presence of one or two rows of pillars—usually stone monoliths, but in some areas also constructed of stone blocks, wooden posts, or even mudbricks—running at right angles to a single broadroom at the back of the house.

The pillared house has often been viewed as a uniquely Israelite innovation (e.g., Shiloh 1970; 1978; 1987; Finkelstein 1988:254–59), but pre-Israelite examples are now known from Late Bronze Age Tel Batash (stratum VII; Mazar 1997:58–66), and from...
the Iron I Philistine sites of Tell Qasile (stratum X; Dothan and Dunayevsky 1993:1204f.) and Tell eš-Šari‘a (stratum VIII; Oren 1993:1331)—although Holladay (1992) suggests that those found in Philistine settlements were built and inhabited by Israelite expatriates who lived there as traders or mercenaries. The non-Israelite origin of the pillared or “four-room” house is discussed in more detail by Frank Braemer (1982:102–5; cf. the review in Shiloh 1986) and by G. R. H. Wright (1985:295ff.), with references to other sites. Note also that the Iron I village sites of ‘Izbet Šarţah and Tel Masos, which also have pillared houses, were not necessarily “Israelite,” in view of their peripheral locations on the margins of what was later the Israelite heartland (see Dever 1991:86).

Whatever its later ethnic affiliations may have been, Gus and Ora Van Beek (1981) have argued convincingly that the Israelite pillared house, with its regularly spaced vertical monoliths, employed an efficient and economical pier-and-rubble building technique borrowed from coastal Canaan. Lawrence Stager (1985:13) suggests, moreover, that pier-and-rubble construction, aside from being less expensive than solid ashlar construction, was favored over rigid, bonded masonry because it was better able to withstand earthquakes. This technique was known as early as the Middle Bronze Age in northern Syria, and through Phoenician influence it came into wide use in Palestine during the Iron Age and also spread, somewhat later, to the Phoenician and Punic areas of the western Mediterranean. Despite its Canaanite origins, however, it is true that the particular expression of this technique that we know as the “Israelite” house was relatively rare in earlier periods but became very common in, and was characteristic of, Israel and Judah during the Iron II period, where it was obviously well adapted to local needs and accordingly remained in use for several centuries.
The central area between the rows of pillars has often been interpreted as an open courtyard, although Stager (1985a:15f.), John Holladay (1986), and Ehud Netzer (1992:197) have all suggested that this area was covered by an upper floor (see figure 4 above), which provided additional space in second-story living quarters. It has been objected that such an arrangement would have prevented sufficient light and air from reaching the lower story (e.g., Mazar 1990b:485f.; cf. Mazar 1997:207; 262), but the illumination and ventilation provided by the outer doorway and small windows would have been adequate, especially since the ground floor was probably devoted largely to storage and to animal stables.

Stager points out, furthermore, that the height of the pillars, and of the stone lintels that are preserved on top of them in some cases, shows that the ceiling of the lower story could be quite low (ca. 1.0–1.5 m). In an Iron II house at Tell en-Naṣbeh, stone lintels were found in place spanning the pillars at a height of only 1.10 m (McCown et al. 1947:213 and plate 77, nos. 2 and 3; see also Netzer 1992:199 n.21). The presence of animals on the ground floor is also indicated by the frequent use of cobble or flagstone paving in the roofed siderooms between the rows of pillars and the outer walls of the house, and by the presence of stone troughs or mangers (see Stager 1985a:14 and references there). Commenting on an Iron I house at Taanach, Stager notes that: “The flagstone pavement provided a solid floor for the heavy beasts, one from which manure and bedding were easily removed. Urine that was not absorbed by the bedding percolated between the cracks of the flagstones and probably collected in the ‘sump’ below the floor” (p. 14; see the comments in G. E. Wright 1978:151). As a result, little space remained for human occupation on the ground floor, from which we may infer the existence of a second story, which is also suggested by the strength of the stone pillars and by occasional evidence of stone staircases. Further arguments in support of this reconstruction of the Israelite pillared house have been provided by Holladay (1992; 1997a; 1997b).  

In general, the typical Israelite house provides clear evidence that its inhabitants engaged in animal husbandry and agriculture, even during the more urbanized Iron II period, when this type of dwelling was very common in the walled towns of Israel and Judah. Olive cultivation, in particular, is indicated by the presence of oil presses at various Iron II sites. At Tell Beit Mirsim in southwestern Judah, for example, several oil presses—originally identified by the excavator as dyeing vats (Albright et al. 1943:55–62)—were found interspersed among private houses which date to the late eighth century B.C., just before the Assyrian conquest (see figure 5 below). Similar presses are also known from Iron II strata at Tell en-Naṣbeh and Beth-shemesh, in the hilly region near Jerusalem. An oil-pressing installation dating to the eighth century was also found in “House 1727” at Shechem in the central hill country, a town belonging to the northern kingdom of Israel, located southeast of Samaria (Campbell 1994:41ff.). Two well-preserved oil presses have also been found in seventh-century contexts at Tel Batash, biblical Timnah, a border town west of Jerusalem that was excavated in the 1980s by Amihai Mazar (Kelm and Mazar 1985:105ff.; Mazar 1997:155–61, 211–18).

One of the Timnah presses was located in a pillared building (Building 950), which shared a common wall with what was clearly an ordinary pillared dwelling (Building 743), which itself lacked a press, in a pattern reminiscent of Tell Beit Mirsim. That the building in which the press was located was also a domestic structure is indicated not only by its architectural characteristics but by the large quantity of restorable pottery found in it, including cooking pots and serving vessels, as well as many storage jars, which were presumably associated with the use of the press (Mazar 1997:217). The other Timnah press was found in a more poorly preserved area that the excavator calls an “industrial-domestic quarter” (ibid., p. 150). In this case, according to Mazar, “the structure in which the oil press was found does not appear to be a dwelling” (p. 161); but, again, the pottery assemblage found in

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2 Netzer, however, places an open “courtyard” on the upper floor; thus in his reconstruction the second-story roofed area is no larger than if the lower central space were uncovered; but there are no parallels for this arrangement, which seems highly unlikely.

3 Note that Bruce Routledge (2000:54) presents evidence for one-story pillared houses dating to the late Iron I period at the well-preserved site of Khirbat al-Mudayna al-ʿAliya in south-central Jordan. He acknowledges, however, that strong evidence for second stories has been found elsewhere (especially at Iron II Israelite sites west of the Jordan River).

4 David Eitam (1979) successfully challenged Albright’s identification of the Tell Beit Mirsim installations as “dye-plants” and cited a number of similar examples of oil presses found at other Iron II sites.

5 Note that the discovery of ceiling beams lying on the ground-floor destruction debris in what G. E. Wright called the “unroofed” central courtyard of House 1727 indicates the presence of a second story (Holladay 1992:316; Campbell 1994:45). On top of these beams upper-story mud-plaster flooring was preserved 6–7 cm thick.

6 The exact figures are: 124 complete vessels, among which were 38 storage jars, 28 bowls, 15 jugs, 11 kraters, 11 cooking pots, 9 juglets, 3 bottles, 3 lamps, 2 amphorae, 2 votive cups, 2 stands, and 1 vat (Mazar 1997:217).
proximity to the press (on the beaten earth floor H1007 in the room immediately to the west of the press) appears to be domestic in character, comprising 16 restorable vessels (7 bowls, 1 chalice, 3 kraters, 2 cooking pots, and 3 juglets). Furthermore, the room in which the press is located shares a common wall with two pillared structures to the east (Units H918 and H804). Mazar suggests that these had an administrative or commercial purpose (p. 154f.), but given the fragmentary nature of the remains, we cannot preclude the possibility that they were dwellings, as he acknowledges.

The Timnah oil presses are similar in design to the seventh-century presses found in the nearby Philistine city of Ekron (modern Tel Miqne), where they occur in large numbers (more than 100 have been found so far) in what has been labeled a specialized “industrial zone” just inside the city wall (Gitin 1989; 1990; 1997). But at Timnah, as at Tell Beit Mirsim, Shechem, and other Iron II Israelite and Judahite sites, oil presses are found scattered throughout ordinary domestic districts. This leads Mazar (1990a: 490f.) to conclude that in Israel and Judah, at least, the presence of oil presses among ordinary dwellings indicates that “the manufacture of olive oil was a cottage industry practiced by families at their homes,” in contrast to large-scale specialized production in a nondo- mestic setting, as has been proposed for Ekron.

While Mazar is certainly correct to note that the production of oil (and other agricultural commodities) was localized and domestic, the term “cottage industry” suggests production for the market rather than for clan or household use. Some degree of market-oriented agricultural production probably existed, and it might have increased, even in Judah, during the Assyrian period, but it is difficult to know how important it was in the overall economy in the absence of relevant textual data, and the archaeological evidence can be explained without according a major role to market exchange. The question is whether surplus commodities left over after domestic consumption and taxation were normally sold on the market or were rather distributed for the most part by means of personalized relationships of kinship and clientage. In my opinion, it is likely that depersonalized market exchange became important in highland Palestine no earlier than the late seventh or sixth century B.C., when it was facilitated by the breakdown of the clans as functioning socioeconomic units—a phenomenon I will discuss further in a future volume.

**Figure 5.** Tell Beit Mirsim, Stratum A, showing oil-press locations (after Albright et al. 1943:plate 1) (Rooms with oil presses are shown in gray.)
In any case, the fact that in the olive-growing regions of highland Palestine a substantial proportion of eighth- and seventh-century urban households possessed oil presses reflects their close involvement with primary agricultural production, which is also indicated by the ubiquity of animal stables and large numbers of storage jars for storing grain, oil, wine, water, and other foodstuffs. Available data suggest that the proportion of households with presses was 20% or more. Oil presses have been discovered in or adjacent to 7 of the 34 Iron II houses excavated at Tell Beit Mirsim (see figure 5 above), and in 2 of the 5 houses excavated at Timnah, stratum II. In the case of Timnah the sample is admittedly very small, but the sample is sufficiently large at Tell Beit Mirsim to allow us to estimate overall proportions.7 It is true that most houses did not have presses, but this does not mean that households which lacked presses were not involved in olive-oil production. The seasonal workforce needed to harvest the olives and man the presses must have been greater than any individual household could supply, suggesting that neighbors, probably in their role as kinsmen or clients, assisted the press-owners and were allowed to press oil for their own use in return.

It is important to note also that oil-pressing, and agricultural activity in general, was not necessarily restricted to villages and to small walled towns like Tell Beit Mirsim, but probably also existed in the residential areas of major Israelite and Judahite administrative centers like Hazor, Megiddo, Samaria, Gezer, Lachish, and Jerusalem, although little evidence is available concerning ordinary domestic districts at such sites. In my opinion, the contrast between “urban” and “rural” sites in ancient Israel has been overdrawn. Holladay (1992:317), for example, describes Tell Beit Mirsim as a “fortified expanded village, instead of a ‘town’ or ‘city’ in any modern sense of the terms,” contrasting it with administrative centers like Megiddo. But it is arguable whether any Israelite walled town can be considered a “city” in the modern sense, namely, an economically specialized wealth-creating settlement that is qualitatively (rather than quantitatively) distinct in its economic and social organization from a relatively undifferentiated village composed of largely self-sufficient agricultural households. Holladay cites the “lack of significant intrasite architectural variability” at Tell Beit Mirsim (i.e., the absence of a monumental palace or citadel), but the presence or absence of major royal buildings says nothing about the mode of life of the ordinary (nonmilitary, nonofficial) residents of an Israelite walled town. The military and administrative function of certain towns was simply added on top of the traditional, basically agrarian social organization of the previous residents, as was plainly the case at Tell en-Nașbeh, the site of biblical Mizpah (discussed below).

Similarly, although the evidence is clearest for certain Iron II settlements, agricultural urban households were probably also common earlier, in the Middle and Late Bronze Age cities of Palestine. Michèle Daviau (1993:469) echoes a common opinion in arguing that Canaanite towns were more “urban” than later Israelite towns because they contain less evidence for “artefacts and features associated with animal care and management” (see also G. R. H. Wright 1985:289, where the Middle Bronze Age Canaanite “town house” is contrasted with agricultural village houses). But more excavation and analysis of Bronze Age Canaanite houses in Palestine is needed (including attention to faunal, botanical, and lithic remains) before this hypothesis can be sustained, especially in view of the fact that archaeologists have hitherto failed to recognize the substantial evidence for agriculture and animal husbandry at the quintessentially “urban” Late Bronze Age site of Ugarit in coastal Syria, as I shall argue below in chapter 13. An analysis of settlement patterns alone suggests that Canaanite cities must have had a strongly agrarian character—Amihai Mazar (1990b:94), for example, thinks that “the Canaanite town was inhabited to a large extent by farmers who cultivated lands around the town; we do not find a pattern of Canaanite villages alongside the towns in most of Late Bronze Age Canaan.” Indeed, the lack of a strict dichotomy between urban and rural modes of life in any precapitalist period is to be expected, in light of the Mediterranean historical evidence and Middle Eastern ethnographic data I have discussed above.

Such a dichotomy is widely assumed, however, by archaeologists who study ancient Israel. In this regard, it is worth commenting briefly on an article entitled “The Rural Community in Ancient Israel during the Iron Age II” that was published not long ago by Avraham Faust (2000). Faust provides a useful review of recent Israeli excavations of a dozen small Iron II settlements (most of which are not fully published), in which anywhere from one to ten pillared houses were uncovered. Faust discusses the measured house areas

7 It is difficult to determine the precise number of houses that were excavated at Tell Beit Mirsim. Frank Braemer (1982:182–98) identified 27 separate dwellings (15 in the northwestern zone and 12 in the southeastern zone), but he ignored several houses. Yigal Shiloh (1980:29), on the other hand, counted 36 “dwelling units.” In my opinion, a count of 34 is more likely, for reasons that will be discussed below.
in these sites in comparison to those in larger walled settlements, with a view to estimating relative family sizes. But he posits a sharp urban-rural dichotomy that I have repeatedly questioned in this book. In particular, Faust’s observation that village houses were larger on average than those in strongly fortified settlements is not necessarily indicative of a difference in family structure (urban = nuclear and rural = extended). It can be explained simply by noting the practical limits on expansion that massive city walls impose. The difference in average dwelling size reflects the lower density of village settlements, and says nothing about how many dwellings within fortified “urban” settlements — under conditions of higher population density— were themselves still large enough to have housed extended (i.e., joint) families. Later in this chapter I will show that the number of large houses in fortified Israelite settlements is sufficient to allow for a strong preference for patrilocal, joint-family coresidence. The proportion of houses of sufficient size corresponds to the demographically predicted proportions (ca. two-thirds nuclear-family and one-third joint-family, discussed in chapter 7.2) that I believe characterized both “rural” and “urban” settlements in ancient Israel.

2. The Oil Industry of Ekron and Economic Rationality

The social and economic situation in Palestine may have begun to change after the Assyrian conquest. The growth of specialized urban production for export (if not true market exchange) as an important phenomenon during the seventh century seems to be attested to the west of Judah in Philistia, in southern coastal Palestine. The unusual abundance of olive oil production facilities at Philistine Ekron, situated between the olive-producing hills and the coastal plain, was perhaps mandated by the Assyrian conquerors of the Levant in order to manufacture oil to exchange for commodities (especially metals) from the western Mediterranean, outside of the zone of direct Assyrian control, as the excavator of this site, Seymour Gitin (1997), has suggested. If so, then Ekron’s economic specialization as an oil-producing center was favored by its proximity to the Mediterranean coast and its political-ethnic ties to the main Philistine seaport of Ashkelon, although evidence from Ashkelon raises questions about how early in the seventh century the expansion of Ekron as a major oil-producer took place. If Gitin’s early seventh-century dating is correct, this economic shift would have been facilitated by the influx into Ekron after 701 B.C. of large numbers of people, including many from Israel and Judah, who had been uprooted by the Assyrians during Sennacherib’s devastating campaign in that year, and who had thus become detached from their traditional clan structures and mode of livelihood and so were encouraged (or forced) to enter into new, less kin-oriented economic arrangements.8 In other words, the shift toward larger-scale specialized production was enabled by the coincidental breakup of the earlier mode of kin-based social organization (which is not to say that the Assyrians consciously planned it this way).

Even so, it is not clear that large-scale export-oriented production of this sort implies an inter-regional system of market exchange constituting a Wallersteinian core-periphery “world-system,” as Gitin proposes (see the discussion of premodern “world-systems” in chapter 4.5 above). The exchange of Levantine oil for Spanish metals, for example, could easily have been accomplished by means of gift exchange among elites or some other kind of administered trade, of the kind practiced by the Late Bronze Age forerunners of the Iron Age rulers of the Near East and their Phoenician agents (cf. Aubet 1993). The scale of this trade apparently increased in the seventh century in tandem with the increased level of production required to service it, and this level of production perhaps depended on a depersonalization of social relations that was fostered by massive deportations and the resettlement of displaced persons in larger urban centers. But there is no need to infer that a true market economy had developed so quickly, although some of the preconditions for increased market exchange had been created.

For the sake of comparison with an even later first-millennium B.C. Mediterranean case, it is worth noting Thomas Gallant’s (1991:98ff.) conclusion that “in general, peasants in ancient Greece did not regularly mobilize their surplus production through the mechanism of the market,” but were rather engaged in “simple commodity production, the salient characteristics of which are: (a) an emphasis on production for

8 The excavator of Ashkelon, Lawrence Stager, questions Gitin’s dating of the initial phase of construction of the seventh-century buildings with oil presses at Ekron, preferring a date after 650 B.C., so that “what propelled the olive oil industry at Ekron into the international sphere was not a dying Assyria but a rising Egypt, ever the greatest consumer of Levantine olive oil” (Stager 1996:70; cf. Gitin 1997:99ff. n.65). But the subsequent discovery at Ekron of a royal dedicatory inscription commemorating the construction of a temple by Ikasu son of Padi, who reigned in the first half of the seventh century, seems to support Gitin’s earlier dating (Gitin et al. 1997). Furthermore, the presence of substantial numbers of seventh-century Philistine and Phoenician storage jars in western Spain suggests that Levantine oil and wine was taken to Spain by the Phoenicians in exchange for metals (see Aubet 1993:244).
direct use by the producers; (b) an emphasis on exchange for use value . . . ; (c) reliance on domestic labor supplemented by noncommoditized labor obtained through social relations.”

For this reason, we should not be too quick to adopt a “factory” model of oil production in seventh-century Philistia without considering what this would entail in terms of family life and the social organization of production. Unless we envisage a radically innovative slave economy in Ekron after the Assyrian conquest, in which forced labor was used to man the presses and thousands of workers subsisted on centrally distributed rations, we must explain how peasant households could participate in more intensive oil production and support themselves at the same time. Because of our familiarity with market exchange in the modern world, it is tempting to posit a rapid transition to a fully fledged market economy characterized by specialized wage labor and a retail market in essential foodstuffs, where workers could buy what they needed; but there is no direct evidence that this had occurred already in the early seventh century, in what continued to be a basically agrarian and still very poorly monetized society.

More likely, in my view, is that oil production continued to be a domestic affair, although it was perhaps intensified by the ruler’s demands for larger contributions of oil to meet the requirement for foreign exchange. Moreover, such demands on the part of the central governing authority do not necessarily imply that the proliferation of oil-production facilities in a greatly expanded Ekron was a matter of centrally planned “economic development.” Indeed, the proliferation of presses might originally have been an economically inefficient aberration induced by purely political factors in the context of Assyrian rule, for it is a functionalist assumption rather than a self-evident fact that the many oil presses found in seventh-century Ekron were built according to a rational master plan and were efficiently utilized to produce vast quantities of surplus oil for export. This is perhaps possible if we adopt the forced-labor model, with all of the burden of complex administration and the radical break from previous economic organization that this would have entailed. But it is also possible that the presses were built and used for domestic purposes by individual families, as in earlier urban settings in Palestine. Most such families would have recently arrived in Ekron and been granted land and olive orchards in Ekron’s newly expanded territory, which included much of the depopulated foothills of Judah. These families did not possess the social ties to their neighbors (in terms of fictive kinship or patronymic associations) that would have permitted them to share productive facilities such as oil presses efficiently. The resultant overabundance of presses, which was quite inefficient economically but was socially necessary, might then have permitted the king of Ekron and his Assyrian overlords to demand a larger surplus of oil for export, in order to acquire exotic goods such as precious metals that would enhance their own social status and political power.

In other words, it is worth considering the possibility that the increased production of oil for export was not planned rationally from the outset but was the accidental byproduct of individually motivated choices based on traditional forms of practical and substantive rationality rather than an innovative type of formal economic rationality (see chapter 4.3). Each peasant household was motivated to construct its own press to meet its domestic requirements, having been thrown into the company of unreliable strangers in a frighteningly large city, and the king was then opportunistically motivated to exploit the resultant increase in productive capacity in order to achieve his own social and political goals. Neither free-market incentives nor a planned economy of the modern sort are required to explain the growth of the oil industry of Ekron. And this industry certainly did not signal the emergence of an integrated interregional market in subsistence commodities akin to Wallerstein’s modern “world-system.” Even olive oil, so common in the Levant, was a rare luxury commodity from the point of view of its foreign recipients. As in the Roman period, most households throughout the Mediterranean region probably remained self-sufficient in the production of basic subsistence goods (see chapter 6.1), while certain foreign commodities, which by definition were rare and thus highly valued, circulated among the elite by means of administered trade or gift exchange, through which rulers established and maintained political relations, while at the same time acquiring luxury items for hoarding and conspicuous display to buttress their social position at home. For that matter, it is very difficult to determine the volume of luxury-good exports such as oil and wine from the Levant in propor-

9 In early seventh-century Spain, as in Italy, olives were apparently not yet cultivated locally, although horticulture was soon transmitted to the western Mediterranean by the Phoenicians and Greeks (see Boardman 1977). Egypt was ecologically ill suited to olive cultivation and was thus a traditional consumer of Levantine oil, beginning as early as the third millennium B.C. (Stager 1985b). In both places olive oil was an exotic commodity that would have been regarded as a luxury whose possession was a symbol of status for the elite, as is clear from Egyptian texts.
tion to local domestic consumption. All we can say archaeologically from the distribution of Philistine and Phoenician storage jars is that some oil and wine ended up in the hands of foreign elites in exchange for other luxury items. Thus it is not clear to what extent the many oil presses of Ekron might have remained underutilized, for there is no way to prove that they operated at full capacity.

Similarly, the abundance of loom weights found in conjunction with the presses need not be interpreted according to an efficient factory model of production in which a textile industry alternated seasonally with the oil industry. Both weaving and oil production were domestic activities carried on in individual households elsewhere in Iron Age Palestine. Again, assuming that seventh-century kings requisitioned a larger than usual quantity of textiles for export purposes, the artifactual evidence can be explained by positing an intensification of production within a preexisting domestic context rather than a fundamental reorganization of the traditional mode of life. This would also explain why four-horned incense altars of Israelite type were found in the Ekron “factories,” a phenomenon for which no convincing explanation has so far been given. Biblical and archaeological evidence suggests that cultic facilities and implements were normally shared within a clan consisting of related households that manifested itself spatially as a contiguous urban neighborhood (see also chapter 13.2 below on this phenomenon at Ugarit). In the absence of clans, each household had to supply its own implements for religious as well as economic purposes, leading to a much greater than normal incidence of these artifacts and an unintentional increase in productive capacity. If we recall that subsistence-level peasant societies are notoriously underproductive, with a substantial pool of underutilized labor, it is not unreasonable to expect that a larger than normal surplus of certain products could then be produced at the household level in order to meet royal demands. Furthermore, the absence of clans removed a layer of political organization that had traditionally shielded individual households from the eyes of royal tax-collectors by pooling tax payments among a larger kin group, depriving households of the political strength that came with collective organization and making it more difficult to avoid paying in full what was owed.

![Figure 6. Seventh-century B.C. oil-press buildings at Ekron (after Gitin 1989:29, fig. 2.3)](image-url)
Under this interpretation, the buildings of “three-room” type excavated in the “industrial zone” at Ekron were not special-purpose “factories” but domestic dwellings that also served as centers of agricultural storage and production, like the other Iron II houses which have been excavated in Palestine. The large “industrial zone” itself, which hugged the perimeter of the city inside a newly built city wall, would then be interpreted as the area on the outskirts allotted for settlement by the poorest newcomers to the city—many of whom came from formerly Judahite olive-growing regions in the foothills to the east. This can be seen by examining closely the several buildings with oil presses that were excavated at Ekron in two separate areas near the city wall and have been described in preliminary reports (surface surveys detected the presence of more than 100 other presses, but these were not excavated). Typical of these are two adjacent buildings, each with a press, that were excavated in Field III SE (Gittin 1985; Gittin 1989: 29, fig. 2:3; see figure 6 above).

Building 1 measures roughly 5 × 14 m, or 70 m² (measured inside the exterior walls), and Building 2 is roughly 6 × 13 m, or 78 m². Assuming that there was a second story, which is likely in light of the Iron II parallels discussed above and because of the presence of internal partition walls and pillar bases, the upper-floor roofed living space would have been adequate to house at least a nuclear family, if not a joint family (see the discussion of house area and household size below). Only a small part of the ground floor of each building was taken up by the pressing installation, which was located toward the rear of the building, away from the street entrance. Other finds in Building 1, in particular, included such diverse items as a cosmetic palette, a cache of eight iron agricultural tools, and a four-horned limestone incense altar in a stone-built niche. The pottery found in Buildings 1 and 2 included substantial numbers of storage jars, some of which were no doubt associated with the use of the press, but other vessel types were present as well, including many bowls, a few cooking pots, and several kraters and juglets, in keeping with an ordinary domestic assemblage (Gittin 1989:37f., tables 1–4). Storage jars are only 22% (43 of 196) and 31% (56 of 181) of all vessels in Buildings 1 and 2, respectively. The latter figure is strikingly similar to the proportion of 31% storage jars (38 of 124 complete vessels) found in the seventh-century two-story pillared dwelling (Building 950) excavated at nearby Timnah (Mazar 1997:217). When we keep in mind the number of jars required simply for domestic storage of a year’s supply of grain, oil, wine, and other foodstuffs, we need not infer the production of a huge surplus of oil for export based on the large number of storage jars found in the oil-press buildings of Ekron. Indeed, in discussing three small ground-floor rooms in Buildings 1 and 2 (rooms 15b, 14b, 27b), Gittin (1989:36) notes that “a large number of store jars were sunk into their floors. Because none of the jars had holes in their bases, they could not have been used for drainage, but most likely served as primitive food or commodity storage containers.” The jars in question were obviously not part of the process of bottling oil for export, and their use to store grain, in particular, is explicable if these buildings were domestic dwellings and not industrial factories.

In an article on the Israelite house, John Holladay (1992:314) cites Patty Jo Watson’s (1979:292f.) ethnoarchaeological estimate that a family of five needed ca. 1,500 kg of wheat per year, plus half as much barley (750 kg) for animal feed. This amount of grain alone would require 3150 liters of storage capacity at 1.3 liters per kg of wheat and 1.6 liters per kg of barley. Additional storage capacity was required for the household’s annual supply of oil, wine, and perhaps legumes or other foodstuffs. The total

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10 In Building 1 there were 101 intact or restorable bowls, 13 kraters, 2 cooking pots, 6 juglets (incl. 1 Judean “decanter”), 25 juglets, 3 “balloon” bottles, 1 chalice, 2 large “jar/kraters” (probably oil-separating vats), and 43 storage jars of various sizes. In Building 2 there were 86 bowls, 10 kraters, 3 cooking pots, 8 juglets (incl. 2 Judean “decaters”), 10 juglets, 5 “balloon” bottles, 2 votive vessels, 1 large “jar/krater” or separating vat, and 56 storage jars.

11 Holladay cites 1,800 kg of wheat and 1,080 kg of barley, apparently taking account of the amount set aside for seed. Watson’s own calculations mention only the amount of wheat needed for consumption, but she notes that farmers sowed half as much barley as wheat.

12 The figure of 1.3 liters per kg of wheat and 1.6 liters per kg of barley is based on the standard measures of 60 lbs. of wheat and 48 lbs. of barley per U.S. bushel (= 35.24 liters). In support of this, see G. Schwartz 1994:27, table 2, which tabulates Near Eastern archaeologists’ estimates of the volume of storage space needed to hold 1,000 kg of grain. These estimates range from 1.3 to 2.25 m³ per 1,000 kg (= 1.3–2.25 liters per kg), but a figure at the low end of this range is more commonly cited (e.g., Kemp1986:132). Note that estimates of the amount of grain needed per person per year in the ancient Near East must allow for the loss of 15–25% due to spoilage and the use of 10–20% of stored grain for seed. Published estimates range widely from ca. 150 to 500 kg per person per year, depending on the proportion of the diet that was supplied by grain. But as Schwartz (p. 25) notes, Near Eastern textual evidence indicates that an estimated average on the order of 250–350 kg per person is reasonable; thus a nuclear-family household of 5 persons would have required ca. 1,250–1,750 kg per year, and a joint-family household of 10 persons would have required 2,500–3,500 kg, not counting the amount needed for animal feed.
capacity of the 56 complete storage jars excavated in Building 2 in Field III at Ekron is ca. 1,800 liters, assuming 32 liters per jar on average for the 49 jars of large ovoid type (see Gitin 1997:92) and a similar average capacity for the 7 other jars of “late lmlk” and large holemouth type. Of course, more jars might have been in use at any given time than are represented in the sample of restorable vessels from this building, and some of the grain could have been stored elsewhere than in jars, but the point of this calculation is simply to show that a large amount of storage capacity was needed simply for household subsistence, apart from the question of how much olive oil might have been placed in storage jars in Building 2 for nonsubsistence export purposes. In general, the diversity of ceramic types and other artifacts in the Ekron oil-press buildings suggests a domestic rather than a “factory” mode of production, in which the household’s needs were supplied first and what was left over was commandeered by the king. But even a modest annual surplus of oil from each household, in what had grown from a small four-hectare town to become a much larger 34-hectare city, would have provided the king of Ekron with sufficient quantities for export in the context of the Mediterranean luxury trade.

If this alternative interpretation is correct, then an unusually high proportion of households owned oil presses and the seventh-century inhabitants of Ekron possessed far more presses than they needed. But this inefficiency (from our perspective) must be weighed against the need of each household to ensure access to a press—and an altar—in the absence of the clan-based social relationships that would elsewhere have guaranteed this. It must also be remembered that an oil press of the type found at Ekron is not so difficult to construct that it would have been beyond the means of an individual household, if the incentive to build one were great enough. The fact that many of the presses appear to have gone out of use in the final phase before the destruction of the city by Nebuchadnezzar at the end of the seventh century would not then be indicative of a decline in oil production due to the disruption of “Assyrian/Phoenician market trading systems” (Gitin 1997:99), but would reflect the reemergence after a few generations of trans-household social networks or urban clans after the aberrant phase of urban life in seventh-century Ekron which was produced by the rapid influx of a large and disparate group of new residents early in the century. Indeed, as the Assyrians retreated, close contacts developed between Philistia and Egypt in the latter part of the seventh century, thus foreign demand for Philistine oil would likely have continued, if not increased, in view of the fact that the Egyptian elite was traditionally a major consumer of luxury olive oil from Palestine (see Stager 1985b; 1996).

In the absence of textual evidence, we must decide which hypothesis is most plausible in light of what we know about typical social and economic relationships of the period. If we adopt the “efficient factory-production” model, the issue arises of the social organization of “industrial-scale” oil production in what continued to be a heavily agrarian and poorly monetized society throughout the seventh century. There is no evidence that Assyrian governors directly managed the whole process as a kind of long-term forced labor project, as Gitin implies, or that they had any interest in doing so—especially in view of the fact that Ekron continued to be ruled by a native dynasty of vassal kings. It is theoretically possible, of course, that oil production was a vast royal operation under the control of the king of Ekron that was staffed by slaves, whose rations and the olives they pressed came from royal estates, but there are no parallels for this in the Iron Age, or in the Bronze Age, for that matter, as I argue below in Part Two. What is attested at Ugarit and elsewhere in the Levant is a simple, flexible, easily administered sharecropping system in which the king as titular landowner receives rent in kind from self-sufficient peasant households.

When we use modern economic terms and speak of “industrial-scale” production for a “world market,” this implies a complex network of depersonalized market-based exchanges for which we have no evidence. Were those who manned the presses of Ekron protocapitalist entrepreneurs responding to market incentives, or wage-laborers who did not own the means of production and so hired themselves out to press-owners, or part of a huge forced labor system of royal slaves or corvée workers? Did the thousands of press-operators subsist on rations or grow their own food—or was there a market in foodstuffs permitting them to buy what they needed? Did they live elsewhere in the city and commute to the “factories” in the “industrial zone”? If so, why is it that such a large proportion of precious space within the city walls was devoted to nonresidential uses? How were olives supplied to the press-owners and by whom? What was the relationship between the cultivators of

13 It is not clear to what extent Gitin envisions a centrally planned command economy as opposed to a market economy, or a combination of the two, with the Assyrians mandating Philistine production of olive oil in order to earn a profit by selling it (via the Phoenicians) to consumers in the western Mediterranean.
the olives and the press-owners? For reasons I have given above in chapter 4, I think it is unlikely that these various relationships and economic exchanges operated in seventh-century Philistia by means of the depersonalized market mechanisms with which we are familiar in the modern world.

According to the “inefficient domestic-production” model, on the other hand, the many presses of Ekron are explained in terms of a local political and cultural logic rather modern functionalist logic. After 701 B.C. Ekron increased greatly in size, from 10 to 85 acres (ca. 4 to 34 ha), as hundreds of uprooted families—many of highland Israelite and Judahite origin—settled (or were forced to settle) there. This sudden increase could not have resulted from natural population growth (if anything, the overall population of the area declined as a result of brutal Assyrian sieges and deportations), nor is it likely that it resulted from voluntary migration arising from economic incentives, unless we regard the catastrophic loss of a family’s home, livestock, and orchards as a kind of negative “incentive.” The population increase in Ekron can be readily explained, however, in the context of the systematic Assyrian destruction of secondary settlements during Sennacherib’s campaign of 701 B.C., especially in the western foothills (“Shephelah”) adjacent to the Philistine coastal plain. Sennacherib boasted that he besieged and depopulated 46 walled settlements and many more unwalled villages which had belonged to King Hezekiah of Judah (ANET, p. 288). Archaeological surveys of the Shephelah bear this out, showing an 86% reduction in the number of sites (cited in Gitin 1997:83), compared to a similar but less dramatic reduction of population in the core territory of Judah in the highlands to the east (Ofer 1993; 1998). After the Assyrian conquest, Hezekiah’s territory in the Shephelah foothills was parcelled out among the Philistine vassals who had remained loyal to Assyria, including Padi, king of Ekron. For a long time thereafter, many towns and villages of the previously well-populated Shephelah remained abandoned, which accounts for the tremendous increase in the size of neighboring urban centers such as Ekron to the west, which expanded eightfold, and Jerusalem to the east, which expanded fivefold.

This enduring change in the settlement pattern probably reflects an intentional Assyrian policy of forced urbanization or synoikismos, for otherwise the towns of the Shephelah would have been resettled in the decades after 701. Such a policy made it easier to police the conquered populace and quell revolts, because fewer centers of population existed to be besieged, if the need arose, saving much effort and expense. But what was efficient militarily and politically was not efficient economically, for overall agricultural productivity must have suffered through the concentration of a previously dispersed rural population. The high cost of overland transportation, not to mention the time required to travel to remote landholdings, would have inhibited the exploitation of previously productive land beyond a certain distance from the city. For this reason there emerged in the biblical prophetic tradition the motif of the desolation and depopulation of once prosperous regions, which were given over to weeds and wild animals (e.g., Isaiah 5:9f.; 7:23ff.). That Ekron could support its population in its swollen state was presumably due to its proximity to fertile grain-growing regions in the coastal plain that could be intensively cultivated. Comparative Mediterranean evidence shows that with sufficient arable land in the vicinity, large “agro-towns” of up to 10,000 peasant inhabitants could be self-sufficient in food production (Chisholm 1979:47–52, discussed below in chapter 9.3). Settlements could not grow beyond this size without importing food; thus it is noteworthy that Ekron did not expand beyond the limit of sustainability in terms of local food production (see also Wilkinson 1994 on this phenomenon in Upper Mesopotamia). Even at a density of 300 persons per hectare, seventh-century Ekron (34 ha) would have had no more than ca. 10,000 inhabitants.16

The economic inefficiency brought about by this forced urbanization should not surprise us, for there is no evidence that the Assyrians understood or were concerned with the economic development of the territory they ruled, except in the most rudimentary sense of making it easy for themselves to tax or confiscate its wealth. And it is anachronistic to credit

14 The four-horned altars of northern “Israelite” type found at Ekron indicate the presence of refugees from the north, and Judahite influence is evident in certain pottery types and in the scarcity of red-slip decoration, in contrast to the preference for a burnished red slip on seventh-century pottery from Philistine Ashkelon on the Mediterranean coast.

15 These settlements included walled towns 3–4 ha in size like Tell Beit Mirsim and Beth-shemesh, which excavations have shown were abandoned after the late eighth century.

16 For urban population densities in Iron II Palestine ranging from 300 to 400 per ha, see chapter 8.6 below. A large proportion of Ekron was occupied by a palace-temple complex, which suggests a density at the low end of this range.

17 Hayim Tadmor (1975:37) observes that Neo-Assyrian “economic” interests revolved around the desire to obtain “luxury items which were in great demand in the royal court and with the military elite.” For this reason, already in the ninth century B.C., Neo-Assyrian rulers set up military colonies west of the Euphrates, beyond the borders of the main empire at that time, “in order to secure an uninterrupted
them with understanding the economics of how this wealth was produced. Indeed, their readiness to resort to punitive scorched-earth tactics, including the destruction of long-term capital investments in fruit trees, which take generations to replace, betrays an overriding concern not with trade and profits but with the militaristic political and religious values that motivated their conquest in the first place.\footnote{Various Neo-Assyrian conquerors specifically state that they cut down the orchards (kirânu) around besieged cities; e.g., Shalmaneser III at Damascus in 841 B.C. (\textit{RIMA} 3, p. 60 [l. 26]), Šamsi-Adad V in Babylonia in 814 (\textit{RIMA} 3, p. 187 [iv 17]), and Tiglath-Pileser III at Damascus in 733–32 and also in Babylonia (Tadmor 1994:79 [ll. 11′–12′], 163 [l. 24]). Note also the depictions of Assyrian soldiers chopping down date palms in reliefs found in Sennacherib’s palace at Nineveh (see Russell 1991:76 [fig. 42], 154 [fig. 78]). That this is not simply propaganda without a basis in fact is indicated by archaeological evidence of the scale and savagery of Assyrian destructions at sites like Lachish (Ussishkin 1980). It may well be that orchards were destroyed in lieu of actually capturing the besieged city (cf. Tadmor 1994:79), but there is no reason to think that this is merely a literary topos as opposed to being a standard military procedure. My colleague John Brinkman has drawn my attention to an inscription of Ninurta-kudurri-usur, governor of Sûhu and Mari around 750 B.C., who quotes a putative dialogue between two Aramean leaders hostile to the settled population along the middle Euphrates: “Then we will go and attack the houses of the land of Sûhu. We will seize his towns which are (located) in the steppe and cut down their fruit trees (ô[t gûp-ni-sá-nu])” (\textit{RIMB} 2, p. 295 [l. 26]).}

What did the Assyrians offer in exchange for the goods they obtained? The ninth-century Assyrian colonies in the West were conquered towns under imperial control that were presumably set up to control trade routes for the purpose of taxing or confiscating luxury goods to send home to Assyria, and also to serve as military outposts for future imperial expansion. The interest in status-defining luxury goods, in particular, suggests that we cannot separate so neatly what many assume to have been mundane economic motivations for empire from supposedly more ideological religio-political motivations. Both the acquisition of foreign luxuries and related acts of military conquest must be seen as part of a larger set of imperial motivations that were not simply “economic” in the modern sense.

Whatever the social and economic situation in seventh-century Ekron might have been, generally speaking the Iron Age pillared house, and the urban neighborhoods of which this house type was the constituent architectural unit, are directly comparable, in functional and social terms, to the Canaanite “courtyard house” and the clan-based urban neighborhoods found in Late Bronze Age Levantine cities like Ugarit. In spite of the change in domestic architectural traditions represented by the increasing popularity of the pillared house in the Iron Age, it was functionally similar to the earlier Bronze Age courtyard house of the Levant. In both Canaanite and Israelite towns, the largest houses occupied a total area of ca. 100 m² on average (or 60–70 m², excluding the courtyard or roofed central space). In Iron II Israel in particular, at Tell el-Fârakah (North), Alain Chambon (1984:32, 44) reports that the three largest houses in stratum VIIb (out of a total of 12 houses) had internal surface areas of 121 (originally 108) m², 95 m², and 90 m², and the two largest houses in stratum VIIId (out of a total of five) had areas of 103 m² and 86 m². In both Late Bronze Age Megiddo and Iron II Tell el-Fârakah (N), courtyards occupied 30–40% of the total surface area of each house. Even if the ground floor were entirely given over to stables, storage, and workspace, in the largest houses a minimum roofed living area of 60–70 m² would have been available on the upper floor, or as much as 100 m² of roofed living area if the upper floor entirely covered the central space, as is likely for most Iron Age pillared houses.\footnote{At Megiddo there are several examples of MB and LB courtyard houses all measuring slightly more than 10 × 10 m, or 100 m² in total area (Loud 1948:13 [fig. 23], 100 [fig. 242], 102 [fig. 246], and figs. 401–2). For Iron Age Syria-Palestine in general, G. R. H. Wright (1985:294) cites a figure of 100 m² as the mean house size, but this is based on the largest houses.} Numerical estimates are necessarily speculative, but a house of this size could plausibly accommodate a typical joint-family household of 10 persons, as opposed to a nuclear-family household of only 5 persons. In a maximally extended patrilocal joint-family household, consisting of three or four conjugal couples together with their children (i.e., the paterfamilias and his wife, living with two or three adult sons and their wives and children), with each conjugal family occupying one sleeping-room on the upper
floor, a minimum of 15–20 \( m^2 \) of private space would have been available for each conjugal family, with shared use of the flat roof and ground floor for cooking, eating, and other domestic activities. Furthermore, at any one time joint-family households constitute only about a third of the total number of households, even where patrilocal joint-family co-residence is strongly preferred, because a high pre-industrial mortality rate ensures that a minority of all families will include living members comprising three or more generations. Consequently, Canaanite and Israelite dwellings that were substantially smaller than 80–100 \( m^2 \) probably housed families in the nuclear-family phase of the household lifecycle; that is, one conjugal couple and their immature children and perhaps one or two other relatives or servants. Comparative Mediterranean data suggest, however, that such households would nonetheless have been related by kinship or clientage to larger joint-family households in the vicinity. It is significant, therefore, that large and small dwellings are intermingled in Bronze and Iron Age Levantine towns where residential areas have been extensively excavated. There are exceptions, of course: a few Israelite towns appear to have been planned as specialized administrative centers (e.g., Iron II Tel Beersheba), and so exhibit considerable uniformity in house size and layout (see Herzog 1992:258ff.; 1997); but most other sites show greater variation.

It is misleading, then, to ignore the variation in the sizes of families at different stages of the household lifecycle by citing simply the overall mean family sizes reported in Middle Eastern ethnographic literature, which some archaeologists have used as the basis for their assumption that Iron II pillared houses were inhabited by “nuclear families” numbering 4–5 persons (e.g., Finkelstein 1990; Broshi and Finkelstein 1992:48; Holladay 1992:310; cf. Zorn 1994:37). This average masks the wide variation between nuclear and joint families and the sociological significance of this variation. It is true that the overall mean family size (as opposed to household size) is only about 4.5 even when one-third are joint families, but it would be considerably less (only about 3.5) if neolocal residence were universally practiced and there were no joint-family households. To understand the structure of the society, we must go beyond the overall mean family size and determine the preferred mode of postmarriage co-residence, and thus the degree to which the patriarchal joint family was the symbolic ideal and, in fact, the dominant socioeconomic unit in which most people spent at least a portion of their lives.

In the absence of direct census data from ancient Israelite households, we can only hypothesize, on the basis of ethnographic and historical analogies, that the largest houses in Israelite towns were usually occupied by patrilineally extended families. This hypothesis is strongly supported, however, by indirect biblical evidence concerning Israelite household structure. In particular, the laws in Leviticus 18:6–18 that prohibit sexual intercourse with various relatives seem to presuppose cohabitation in the same dwelling (see Fecher 1998 for a recent review of research on Leviticus 18). Included after the phrase “You shall not uncover the nakedness of . . .” are: (1) “your mother”; (2) “your father’s wife”; (3) “your sister”; (4) “your son’s daughter”; (5) “your daughter’s daughter”; (6) “your father’s wife’s daughter”; (7) “your father’s sister”; (8) “your mother’s sister”; (9) “your father’s brother’s wife”; (10) “your son’s wife”; and (11) “your brother’s wife.” Not all of a man’s potential relatives are listed. Judging by the

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20 We may question Roland de Vaux’s frequently quoted statement that such variation at Tell el-Fâr‘ah (N) was evidence of “rich” versus “poor” quarters, and that these were strictly separated in the latter part of the Iron II period (stratum VIIId) reflecting “the birth of an urban proletariat” (see, e.g., de Vaux 1956:134). The two groups of houses on which he based this conclusion (numbering only five dwellings in all!) may not even have been contemporary, as Holladay (1966:120f.) and McClellan (1987:86) have suggested (i.e., the three smallest houses belong to the earlier stratum VIIb because of their similarity to other houses of that period). Furthermore, the “long straight wall” separating the two groups can be interpreted as part of an ordinary building. In any case, the widely varying sizes of the VIIId houses alone (53, 62, 78, 86, and 103 \( m^2 \)) are nonetheless within the range of variation of the supposedly more homogeneous houses of stratum VIIb (53–121 \( m^2 \)); see Chambon 1984:39–47, esp. the VIIId surface-area calculations in table 2 (p. 44) compared to the VIIb calculations in table 1 (p. 32), and the VIIId houses in plan 5.

21 As I have noted above in chapter 7, the overall mean family size in Roman Egypt was 4.4, combining urban and rural households of all types (Bagnall and Frier 1994:68). In fifteenth-century Tuscany, the overall mean family size was also 4.4 (Herlihy and Klapisch-Zuber 1985:282). In both cases, patrilocal joint-family co-residence was strongly preferred, and a majority of the population spent at least part of their lives in a joint-family household. By way of comparison, Burch (1972:96, table 2.1) calculates that the mean nuclear-family size is 3.6 when GRR=3.0 and \( \varepsilon=0 \); and this is borne out by the census data from Egypt (where GRR=2.9) and from Tuscany. Using the ratio I have adopted here of joint families to nuclear families (1:2), and assuming that joint families had 7 members on average (this figure, discussed in chapter 7.2, represents a purposely high estimate) and that nuclear families had 3.5 members on average, we obtain an overall mean family size of 4.7. The overall mean masks the bimodal structure of the underlying distribution of family sizes.
particular family relationships that are mentioned, the biblical scholar Martin Noth, following Karl Elliger (1955), suggested that:

It looks as though originally it had not been a definitely demarcated circle of blood-relationship that was being considered, but rather those normally living together in the circle of the grandparents, in tents or houses. . . . In the large family group there is involved possibly brothers still living together with their father (v. 14) and with their own already married brothers (v. 16); and on the other hand with their own married sons (v. 15). [Noth 1965:135]

Biblical scholars have also noted Yahweh’s threat in the Decalogue to “visit the iniquity of the fathers upon the children to the third and fourth generation of those who hate me” (Exodus 20:5; cf. Deuteronomy 5:9). This threat was not uttered out of exceptional vindictiveness toward the unborn, but because of the “ancient legal practice in which all the members of a household were regarded as implicated in the guilt incurred by any one of their number: ‘the third and fourth generations’ reflects the greatest possible extent of the range of members of any one family actually living together in one household” (Clements 1972:124). Joint-family households may also be in view in the commandment to “honor your father and mother” (Exodus 20:12), which is not addressed to little children but enjoins adult sons to obey and care for their parents—a stricture enforced in the “law of the rebellious son” (Deuteronomy 21:18–21), which prescribes death by stoning for a “glutton and drunkard” who refuses to heed his parents. As C. J. H. Wright (1992:767) puts it: “The son in question should not be thought of as merely a mischievous child, but rather as of an age to seriously threaten, by his rebellious behavior, the very substance of the bêt-’aḥ [lit. “house of the father,” i.e., the patrilocal joint-family household], and incapable of being entrusted with an inheritance from it.” Note also the commandment prohibiting work on the Sabbath (Exodus 20:8–11), which is addressed to the head of the household and says (v. 10): “You shall not do any work—neither you, nor your son, nor your daughter, nor your male slave, nor your female slave, nor your beast, nor your client (gēr) who is within your gates.” Adult married sons are presumably included here as members of their father’s household, although this is not explicit in the text. And the possibility that brothers will sometimes continue to live together in a frèreche after their father’s death is acknowledged in the Deuteronomic legislation, which states: “If brothers dwell together, and one of them dies and has no son, the wife of the dead shall not be married outside to a stranger . . .” (Deuteronomy 25:5).

In the preexilic prophetic literature, also, there are clear hints of joint-family coresidence. In the oracle preserved in Micah 7:5–6, the prophet predicts the day when a man’s own household are his enemies, when a son will be against his father, a daughter will be against her mother, and (what is most indicative of patrilocally coresidence) a daughter-in-law will be against her mother-in-law. A joint-family household is also in view in Amos 6:9–11, where impending doom is prophesied by predicting that:

If ten men remain in one house, they shall die. And when a man’s (literally, “his”) patrilateral kinsman (dôdê)—he who mourns22 for him—lifts him up to bring the corpse (ʾsāmîm, lit. “bones”) out of the house, and calls to the one in the rear of the house, “Is anyone else with you?,” she will answer “No one,” and he will say, “Hush! We must not mention the name of Yahweh.” For Yahweh commands, and the large house is smashed to bits, and the small house to pieces.

The fear of invoking Yahweh’s name suggests a plague, which might yet strike the survivors, not a military action. Thus it is unlikely that what is intended here is the demise, after the siege and capture of the city, of a small surviving group “who gather together in one of the remaining houses for protection” (Paul 1991:214), as some commentators have suggested. If so, how is it that the kinsman knows where to find his relatives whom he has the duty to bury (and also survives himself to do so)? In this prose passage, which is clearly a separate unit from the previous poetic oracle in vv. 1–8 (which predicts not death but foreign exile, especially for those who “lie on beds of ivory and lounge on couches,” v. 4), it is more likely that the prophet is depicting in a different way the devastating and unpredictable judgment of Yahweh, which can wipe out the men of a household by means of sickness, as well as by means of battle and siege. Even in a very large joint-family household with ten men (excluding women and small children), not one man will survive, if Yahweh so commands, hence the necessity for a more distant

22 The difficult hapax legomenon here, the Piel participle with masc. sg. pronominal suffix m’sāʾār,pō, has traditionally been interpreted as a variant spelling based on šrp, “to burn”; hence, “the one who burns (for?) him.” But as Shalom Paul (1991:215) notes, there is no other case of the variant spelling šrp for the common verb šr̄p, and burning the dead or burning incense for the dead was rare. Also, šrp never appears elsewhere in the Piel. It is more likely that there is a textual corruption of an original Piel participle mappʾādō (or Hiphil maspādō) involving a metathesis and dālet-reś interchange (cited by Paul, although he favors a different solution); hence the translation “he who mourns for him.”
kinsman to see to the burial rites.\textsuperscript{23} This explains the closing proverbial refrain that no household, large or small, is immune from catastrophic judgment. Furthermore, in the context of the unhappy fate of an unusually large ten-man household, the contrast here between large and small households probably refers to a difference not in wealth but in number, corresponding to the distinction I am making between joint-family and nuclear-family households.

Biblical texts such as these have led many scholars to conclude that the basic socioeconomic unit of Israelite society in all periods was the coresident patrilineage or joint-family household, called in Hebrew the bêt ṭâb ("house of the father"), which was headed by the senior living male of a given patriline and maximally consisted of his wife, sons, and unmarried daughters, together with his own unmarried siblings and other dependent relatives, his sons’ wives and children, and his slaves, landless clients (gērîm), and livestock (see de Geus 1976:133ff.). Each bêt ṭâb had its own inalienable hereditary landholding, called the nah’ilâ ("inheritance"), which its members farmed to support themselves. Norman Gottwald’s (1979:285–92) detailed discussion of the bêt ṭâb has been widely cited, but he fails to take account of the high mortality rates prevalent in antiquity; thus he errs in suggesting that “a thriving bêt-h-ṭâb might easily comprise from fifty to one hundred persons” and consist of “as many as five generations of Israelites” (p. 285). This is a theoretical maximum that would have been extremely rare in reality, although the biblical writers themselves present an idealized picture of an improbably large bêt ṭâb in the family of the patriarch Jacob and his twelve married sons.\textsuperscript{24}

Although many of the relevant biblical passages putatively refer to the premonarchic (Iron I) period, all of these biblical texts date to the Iron II period or later, and they probably reflect social conditions that existed during the monarchic period. In any case, it is unlikely that Israelite household organization (as opposed to broader “tribal” structures) changed significantly with the emergence of larger and more centralized political units around 1000 B.C., especially since the economy of inland Palestine remained highly localized and agrarian throughout the Iron Age. Of course, at any stage of Israelite history the patrilocal joint-family household was an ideal that could not always be realized in practice, because of high mortality rates and the lack of resources (especially due to the scarcity of good land) that might force the members of certain households to attach themselves to wealthier families as dependent clients or slaves.

4. Clans and Households

For the Iron I period, Stager (1985a:18–23) has argued, on the basis of biblical, archaeological, and ethnographic data, that the pillared houses of the early Israelite villagers who settled the highlands of Palestine were arranged in “multiple family compounds,” each of which was inhabited by a single patrilineage or bêt ṭâb. According to Stager, the early Israelite bêt ṭâb was a joint-family household composed of smaller conjugal families which occupied individual dwelling spaces in small adjoining houses. He also suggests in passing that multiple family compounds of adjoining houses continued to exist in the more congested urban settlements of the Iron II period (ibid., p. 22). But it is more likely, in my opinion, that the largest Iron Age pillared houses were normally inhabited by an entire bêt ṭâb, a conclusion that is based on three lines of evidence: (1) demographic estimates of the mean size of joint-family households under preindustrial conditions in the Mediterranean region (i.e., 10 at the most, having generously added 3 to the maximal mean joint-family size of 7 to allow for extra wives, servants, or more distant kin); (2) analogies to the functionally similar dwellings of patrilineally extended families in later Mediterranean towns; and (3) estimates of the mean

\textsuperscript{23} The presence of a survivor in the inner part of the house indicates that it is ten "men" (ṭ’nahîm) who are in the house and who die, and not ten "people" (i.e., all the inhabitants of the house). The number “ten” is obviously hyperbolic; very few households would have been so large as to have ten men. But if this number referred to the entire household (including women and young children), the hyperbole would be lost. Shalom Paul’s interpretation (ibid., p. 216) that there are no survivors and the person in the rear of the house who answers “No one” is one of two men (both a kinsman and an “embalmer”) who go into the house to carry out the corpses, is unlikely. Even if we emend the singular verb with pronominal suffix nū ṭāṭâ to read nādū ṭâb, “they lift up,” instead of “he lifts him up,” it is hard to explain why one corpse-bearer would ask another corpse-bearer, “Is there anyone else with you?” (ha’ōdî ṭinnâhî). Moreover, the feminine form of “you” is used here. The person hidden in seclusion in the recesses of the house is apparently a woman, despite the use of the masculine form of the verb ʿamâr where I have translated “she will answer” above (and note that the use of an unmarked masculine verbal form with a feminine subject occasionally occurs elsewhere in biblical Hebrew; see GKC §145.7).

\textsuperscript{24} Although large numbers of sons are attributed to certain semilegendary patriarchs or judges and to polygamous Israelite kings, in various biblical stories we typically hear of two (surviving) sons, e.g., the two adult sons of such figures as Moses, Eli the priest of Shiloh, the wise woman of Tekoa, and the widow Naomi (Blenkinsopp 1997:52).
roofed dwelling area per person which have been derived from cross-cultural ethnographic research, discussed below (i.e., 8–10 m² per person).

In the Iron II period in Israel, the bet ‘ab was small enough and houses were large enough that joint families did not normally spill over to occupy a cluster of neighboring pillared houses. This makes sense insofar as the bet ‘ab was clearly the fundamental economic unit and the Israelite pillared house is an equally well defined architectural unit whose residents shared a walled space with one entrance and common facilities for cooking, eating, sleeping, stabling, storage, and craftwork. Thus the clustering together of large and small dwellings at Iron II sites like Tell Beit Mirsim, Tell en-Na‘eh, and Tell el-Fâr‘ah (North) does not indicate multiple family compounds of nuclear families comprising larger bet ‘abs, but rather indicates residential neighborhoods or “quarters” consisting of multiple bet ‘abs comprising broader clans or patronymic associations of related households (mišpâhôt).

A close inspection of the plans from stratum A at Tell Beit Mirsim that were published by Albright (plates 1, 3–7), in light of the later analyses of these plans by Yigal Shiloh (1970:figs. 3 and 4) and Frank Braemer (1982:figs. 7b and 9a), supports this view (see figure 5 above and figures 7 and 8 below). The arrangement of the houses uncovered in the northwestern quadrant of the tell, in particular, reveals a pattern of narrow, winding streets and blind alleys that is common in traditional Near Eastern cities. The entrances to several houses along the city wall opened onto a secluded plaza. Large houses were often situated next to smaller houses. In several instances, a pair of houses share a forecourt or alley with a single exit to the street, as Braemer has noted. Shiloh’s (1970:188) conclusion that the “two-, three- or four-room” pillared dwelling units found in Israelite walled towns “were joined in clusters, forming blocks and dwelling quarters in the city” is quite reasonable, therefore. Comparative Mediterranean evidence suggests that such a cluster of neighboring houses was inhabited by a clan or “patronymic association” headed by one of the wealthier householders, whose house and household were accordingly among the largest in the neighborhood. What must be corrected is the view of Shiloh and many others that individual pillared houses were occupied by nuclear families, a view which does not take into account the range of variation in house sizes and the relatively modest space requirements of even the largest third of households which were in the joint-family phase of the household lifecycle at a given time.

Figure 7. Northwestern area of Tell Beit Mirsim, Stratum A (after Albright et al. 1943:plates 6, 7) (Rooms with oil presses are shown in gray.)
The Israelite mišpāhâ or “clan” was an endogamous “protective association of extended families” that performed important social functions (Gottwald 1979:257–284; C. Wright 1992). It defined the circle within which a man could be required to serve as a ḡōḵ or “kinsman-redeemer,” a role which carried the obligation of avenging the murder of a clansman, marrying the widow of a deceased clansman to provide him with a male heir, redeeming the land of an impoverished clansman, and redeeming or otherwise supporting an indebted clansman himself and his dependents.

Not surprisingly, the mišpāhâ was also the basis of military conscription, as Gottwald (1979:270–276) has argued in some detail. He notes that the term ṣelep (pl. ṣalāpîm) which denotes a military unit, is occasionally used as a synonym for mišpāhâ, and it probably describes the fighting company of men mustered from a clan for military service. If this is so, the connection between the mišpāhâ and the ṣelep supplies an important clue concerning the size of a typical Israelite mišpāhâ. The Hebrew word ṣelep can also mean “one thousand,” but Gottwald, following George Mendenhall (1958), points out that originally the ṣelep as a military unit “did not contain one thousand men, nor indeed any fixed number, but rather a very much smaller but variable number of men actually mustered or promised for muster by a mishpāhâ” (p. 271). The size of a typical ṣelep can be determined from census lists preserved in Numbers 1 and 26, which enumerate the fighting men from each tribe “by their mišpāhôt and by their bêt ḏābôt.” Mendenhall, adopting an interpretation proposed long before by Flinders Petrie (1911:42–46), has argued convincingly that, instead of the traditional reading of these census entries as “X thousands plus Y hundreds” (e.g., 40,000 + 500 = 40,500 men from Ephraim in Numbers 1:33), which yields impossibly large numbers, we should read “X companies, totaling Y” (e.g., “40 ṣelep-units, 500 [men]” in Numbers 1:33). Numbers 1 therefore lists a total of 598 ṣalāpûm, consisting of 5,550 men (an average of 9 men per ṣalāpûm, with a range of 5–14); and Numbers 26 lists 596 ṣalāpûm, consisting of 5,750 men (an average of 10 men per ṣelep, with a range of 5–17).25

25 Mendenhall (1958:62) tabulates the numbers of ṣelep-units and men listed for each tribe in each of the two lists, and the individual averages (men per ṣelep) by tribe are: (1) Reuben 500/46 = 11 [Numbers 1] and 750/43 = 17 [Numbers 26]; (2) Simeon 300/59 = 5 and 200/22 = 9; (3) Gad 650/45 = 14 and 500/40 = 12; (4) Judah 600/74 = 8 and
As Mendenhall points out, the figures recorded in the census lists of Numbers seem both too random and too precise to have been the product of invention by the postexilic Priestly compilers of the book of Numbers. Moreover, Petrie demonstrated long ago that the numbers of “thousands” (רָפָם) and the numbers of “hundreds” in these lists are statistically correlated:

Now on looking at these lists a remarkable feature is that the hundreds of the numbers are mostly 4 or 5; 14 out of 24 numbers fall on these two digits, and there is not a single hundred on 0, 1, 8, or 9 . . . . There is here clearly some selective cause why the hundreds group together on 4 and 5, and around those digits, while avoiding both the extremes. This can only mean that the hundreds are independent numbers in some way, and not merely the odd amounts after the thousands. [Petrie 1911:42f.]

The striking correlation between the numbers of רָפָם and the numbers of hundreds can most easily be explained according to the interpretation outlined above. The census figures of Numbers therefore probably derive from actual administrative records, although the current twelve-tribe presentation is undoubtedly artificial and schematic, and at any one time many more clans must have existed in ancient Israel or Judah than are included in these lists. Numbers 26, for example, records a total of only 57 מִשְׂפָּחָה for the twelve tribes—an average of 5 per tribe. But the exhaustive twelve-tribe scheme is clearly secondary here, and earlier fragmentary or selected information has been inserted into it. This is not to deny, of course, that the Priestly editors of this material themselves understood the census entries to refer to actual “thousands” plus hundreds, nor were they reluctant to exaggerate numbers, for that matter. Nonetheless, the census figures of Numbers 1 and 26 do not appear to have been the product of pure invention, as a number of scholars have noted. Noth (1968:21–23), for example, defends the interpretation of הֶלֶם as “troop” in the census lists and argues that a typical military company of 9 or 10 men is reasonable in light of the Egyptian troop sizes recorded in the Amarna letters.

Gottwald (1979:275f.) has applied the interpretation of the term הֶלֶם as a clan-based military unit to the story of Gideon (Judges 7:2–8), arguing that the strange reduction of Gideon’s army from 32,000 to 300 men stems from the biblical writer’s misunderstanding of an original source that in one place spoke of 32 fighting units or רָפָם, and elsewhere referred to 300 men (yielding, again, an average of roughly 9 men per הֶלֶם). If this hypothesis is correct, the 32 רָפָם who were called out to fight by Gideon, who was a member of the tribe of Manasseh, correspond exactly to the 32 רָפָם of Manasseh recorded in Numbers 1:35 (although the number of men listed there is 200 rather than 300). Gottwald’s explanation therefore accounts for a curious feature of the Gideon narrative and provides further support for Mendenhall’s interpretation of Numbers 1 and 26.

Now, Mendenhall and Gottwald attribute both the census data in Numbers and the clan-based organization of the הֶלֶם to the premonarchic period (Iron I), or to the early part of David’s reign at the latest, as does Noth (1968:23). But there is no reason to assume that local levies (as opposed to professional troops) ever ceased to be mustered from individual מִשְׂפָּחָה throughout the monarchic period (Iron II), especially in view of the fact that many scholars regard the kin-oriented מִשְׂפָּחָה and בֵּית הָבָשָׁה as the fundamental socioeconomic units of the kingdoms of ancient Israel and Judah in all periods. Behind all such attempts to restrict functioning kin-groups to the premonarchic period lies the evolutionist assumption that kin-based social organization is incompatible with the territorial organization of a centralized “bureaucratic state.” But in my view, the very existence of such a state must be questioned for this period. In patrimonial regimes, such as the Iron Age polities (both “monarchic” and “premonarchic”) that emerged in highland Palestine, there is no conflict between the traditional organizing principle of “kinship” (actual or metaphorical) and a relatively centralized political administration, as I have argued above in chapter 4. Furthermore, if the postexilic Priestly editors of Numbers used reliable written sources at all, these will have come from “state” archives of the Iron II period.

A typical הֶלֶם-company of 9 or 10 men suggests that the מִשְׂפָּחָה itself, from which the הֶלֶם was mustered, was composed of only one or two dozen בֵּית הָבָשָׁה households. At any given time, as we have seen, approximately one-third of these households would have been in the full three-generation joint-family stage of the household lifecycle, and two-thirds would have been in the nuclear-family stage (consisting of married men whose fathers had already died, together with their wives and small children, and their widowed mothers or other dependent family members, in some cases). The relatively small size of the military company mustered from a clan results from the aspect of the census figures of Numbers 1 and 26.
from the fact that a typical joint-family bet ḏāb would have had only two men of an age suitable for military service (e.g., 20–50 years old), while a nuclear-family bet ḏāb would have had only one man in this age bracket (these figures are based on Saller’s computer-based kinship universe simulations, discussed in chapter 7.2). Furthermore, we can assume that not all men of military age were normally taken for service at any one time, so that some could remain to defend their families and to carry on the work of the household.

In this regard, Gottwald (1979:274f.) points to 1 Samuel 17:12–18, where we read that three of Jesse’s eight sons fought with Saul against the Philistines. They were part of the ‘ephelomustered from the “Ephrathite” miṣphāh, Ephrathah being the eponymous ancestress of a clan that lived in the town of Bethlehem in Judah.26 There are intriguing hints in 1 Samuel 17 of the actual operation of the clan-based fighting units of ancient Israel: Jesse sends his youngest son David to his brothers and their sar ḏelep (“company commander”) with an ephah of parched grain, ten loaves of bread, and ten cheeses—apparently an adequate amount to provision the small ten(?)-man troop in which his three eldest brothers served (see Gottwald 1979:275). This account is far from being a historically accurate report, of course, and the symbolic number of David’s seven older brothers is suspect; but the proportion of men from one bet ḏāb who were serving in their clan’s ḏelep is perhaps significant, assuming that the writer was aiming for literary verisimilitude. The original audience of the story would have understood that Jesse, the patriarch of a nine-man bet ḏāb, was too old for military duty (v. 12), and that David was too young (v. 14), leaving seven eligible males, of whom three went off to fight. Applying the same ratio to the family sizes which are demographically plausible in ancient Israel, a typical joint-family household with only two men who were eligible for military service (i.e., the adult sons, excluding their aged father and their own immature sons or younger brothers) would have sent only one man off to fight. Similarly, not all heads of nuclear-family households would have gone to war.

The fact that an ḏelep-company typically consisted of only 10 men rather than every man of the clan who was eligible for service implies that the census lists in Numbers were copied from rosters of those who actually performed military duty, rather than all of those who were eligible. Gottwald (1979:274) defends the latter hypothesis, but he proposes that a large bet ḏāb alone could have supplied an entire ḏelep-company, and that a miṣphāh might have consisted of “scores” of bet ḏāb households; thus for him the ḏelep-company represents only a tiny minority of the eligible men of a clan. Such a low rate of conscription seems unlikely, however. Gottwald here overestimates both the size of the typical bet ḏāb (having failed to allow for high mortality rates) and the number of households in a typical miṣphāh. It is more likely that most clans consisted not of “scores” but of only one or two dozen households in all, and that even the most fully extended three-generation joint-family households typically had only 10–15 members. If, for the sake of argument, we assume a figure of 18 households per clan on average, we obtain an estimate of 24 men of military age per clan, of whom only 40–50%, as we have seen, might normally go off to fight.27

The actual situation was obviously much more complex, with a greater variety of household types and household and clan sizes. But schematic though our estimates must be in the absence of detailed census data from ancient Israel, these estimates accord well with the notion that a small village of 100–150 inhabitants typically constituted a single endogamous clan (miṣphāh).28 Using plausible estimates of 18 households per miṣphāh on average, 10 persons per joint-family bet ḏāb (which are one-third of the total number of households, or 6 households), and 5 persons per nuclear-family bet ḏāb (two-thirds of the total number of households, or 12 households),29 we

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26 See Micah 5:1, “You, O Bethlehem Ephrathah, who are one of the little clans of Judah (‘alpē Yehūdā)...” using the term ḏelep poetically as a metonym for the entire miṣphāh; cf. also Ruth 1:2; 4:11; Genesis 35:19; and the genealogical information in 1 Chronicles 2:19 and 2:24, 50, where Ephrath(ah) appears as the second wife of Caleb and the mother of Hur, who is “the father of Bethlehem” in 1 Chron. 4:4.

27 Of 18 households, 6 (one-third) would have been joint and 12 (two-thirds) would have been nuclear. With 2 men of military age per joint household and 1 per nuclear household, this yields 24 men of military age on average, as compared to 10 per ḏelep. Compare the estimate of 20 pillared buildings surrounding a larger central pillared building (belonging to the clan leader/village headman?) at ‘IZ-bet Sartah (Finkelstein et al. 1986:21).

28 Gottwald (1979:316) suggests that “since the miṣphāh was an aggregation of extended families, it had a vital regional or neighborhood character which might be coextensive with a rural neighborhood, a cluster of small settlements, a single settlement and environs, or a segment of a large settlement.” His emphasis on clan coresidence is correct, but the range of clan sizes was probably not so wide as he supposes. Very few clans would have maintained their cohesion for long if they were spread over more than one settlement or consisted of “scores” of households (p. 274).

29 See chapter 7.2 for a defense of maximum mean household sizes of 10 (joint) and 5 (nuclear), a ratio of joint to
obtain an average of 120 persons per *mispâhâ*, of whom 60 lived in the 6 joint-family households and 60 lived in the 12 nuclear-family households.

5. Iron Age Villages and the Manassite Clans in the Samaria Ostraca

This estimate of clan size is in keeping with archaeological evidence which indicates that Iron Age villages had an average population of ca. 100–150. Few Iron Age villages have been excavated, but an estimate of 100–150 is plausible for those which have been excavated, such as Giloh (Mazar 1981:18), Khirbet Raddana (see Stager 1985a:23), and Izbet Şartah (Finkelstein et al. 1986). Settlement area is the best criterion for estimating the average population of Iron Age villages because most village sites have not been excavated and counting actual houses or rooms is therefore not feasible. Settlement area is easiest to determine for the single-period Iron I sites in highland Palestine which were surveyed in the 1970s and 1980s. The mean area of these settlements was no more than half a hectare, and few exceeded one hectare in size.

For example, from the detailed “Land of Ephraim Survey” of part of the West Bank, Israel Finkelstein (1988:192) reports that of 114 Iron I sites, 26 (23%) were “large villages” (≥ 0.5 ha), 32 (28%) were “small villages” (< 0.5 ha), and 56 (49%) consisted of only a “few houses.” Even if we disregard the last category on the grounds that those were seasonal encampments or ephemeral farmsteads rather than proper villages, we can see that there were more villages occupying less than 0.5 ha than occupied more than 0.5 ha. This is confirmed in the final report of the “Land of Ephraim Survey” (renamed the “Southern Samaria Survey”), in which Finkelstein and his colleagues estimate the sizes of Iron Age settlements (Finkelstein and Lederman 1997:20f. [vol. 1]; 894–902 [vol. 2]). The breakdown by period of settlements for which sizes were estimated is as follows:

<table>
<thead>
<tr>
<th>Size (ha)</th>
<th>Iron I</th>
<th>Iron II</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.1–0.3</td>
<td>89 (70%)</td>
<td>106 (48%)</td>
</tr>
<tr>
<td>0.4–1.0</td>
<td>26 (20%)</td>
<td>66 (30%)</td>
</tr>
<tr>
<td>1.1–2.0</td>
<td>12 (10%)</td>
<td>29 (13%)</td>
</tr>
<tr>
<td>2.1–3.0</td>
<td>0</td>
<td>13 (6%)</td>
</tr>
<tr>
<td>3.1–4.0</td>
<td>0</td>
<td>5 (2.3%)</td>
</tr>
<tr>
<td>4.1–5.0</td>
<td>0</td>
<td>1 (0.5%)</td>
</tr>
<tr>
<td><strong>Total:</strong></td>
<td>127</td>
<td>220</td>
</tr>
</tbody>
</table>

(There is also a group of sites labeled “Iron Age I–II,” with 84 sites that are 0.1–0.3 ha in size, and 8 sites 0.4–1.0 ha.) Although these survey data show that the number of settlements increased by 93 sites (73%) in the southern Samaria survey region between the end of the Iron I period and the end of Iron II, and that 19 (9%) of the Iron II settlements attained a size (> 2 ha) that did not exist in the Iron I period, the mean village size (i.e., unwalled settlements) did not increase greatly between the end of Iron I and the end of Iron II. Because Iron II settlements as small as 2–3 ha were fortified with walls (e.g., Tell en-Naṣbeh and Tell Beit Mirsim), it is likely that most unwalled settlements, conventionally called “villages,” were 2 ha or less in area, and it is most useful to compare the proportions of Iron I versus Iron II settlements in this size range.

Of the 220 Iron II sites, 201 were 2 ha or less, and 106 (53%) of these were less than 0.4 ha, 66 (33%) were 0.4–1 ha, and 29 (14%) were 1.1–2 ha. When these proportions are compared to the Iron I proportions (70% < 0.4 ha, 20% 0.4–1 ha, and 10% 1.1–2 ha), a slight increase in mean village size is discernible, but in both cases the large majority of villages (86% and 90%, respectively) did not exceed one hectare. The Iron I village-size estimates are similar to those for the Iron II period because the Iron I survey data do not refer to newly founded (and presumably smaller) villages of the early Iron I period, for the most part, but to long-established villages dating to the end of the Iron I period, ca. 1050–1000 B.C. (see Finkelstein 1988:193, 332), by which time most such settlements would have reached their full ecologically and sociologically sustainable sizes.

We can therefore assume a mean village area in highland Israel (after the initial settlement phase) on the order of 0.5 ha, with at least half of all villages occupying less than 0.5 ha. Assuming a density coefficient for unwalled villages of 200–250 persons per hectare, this yields a mean village population of ca. 100–125, which corresponds well to the estimate of 120 per *mispâhâ*. (This village density coefficient is discussed below, in relation to the higher density of 300–400 per ha that I believe is more likely for the larger walled towns of the Iron II period.) We can also compare the mean village area of half a hectare adopted here to the sizes of Iron II walled country towns like Tell Beit Mirsim, Tell en-Naṣbeh, and Beth-shemesh, which are on the order of 2–3 ha (excluding the city wall). In other words, such walled towns were four to six times the size of the average village, which seems plausible.
### Table 4. Clan and Village Names in the Samaria Ostraca

<table>
<thead>
<tr>
<th>Ostracan No.</th>
<th>Transliteration</th>
<th>Translation</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>bšt. hštr. šmryw. mb²rym. nbl. [yn.] yšn.</td>
<td>In the tenth year to Shemaryaw from Beerayim, a jar of old wine.</td>
</tr>
<tr>
<td></td>
<td>lgr ≠c. 交通枢纽. 2.</td>
<td>Gera (son of) Elisha, 2.</td>
</tr>
<tr>
<td></td>
<td>šl. 5. b.P. 1.</td>
<td>Uzza (son of) Q[i]b[a(?)], 1.</td>
</tr>
<tr>
<td></td>
<td>yšyw.</td>
<td>Baala (son of) Elisha [ . . . ].</td>
</tr>
<tr>
<td></td>
<td>yd. ym.</td>
<td>Yadayaw [ . . . ].</td>
</tr>
<tr>
<td>2</td>
<td>bšt. hštr. lgdyw. m²zh. g. mtr. 1.</td>
<td>In the tenth year to Gaddiyaw from Azzah.</td>
</tr>
<tr>
<td></td>
<td>nbl. l.</td>
<td>Abibaal, 2.</td>
</tr>
<tr>
<td></td>
<td>2.</td>
<td>Ahaz, 1.</td>
</tr>
<tr>
<td></td>
<td>šb. 1.</td>
<td>Sheba, 1.</td>
</tr>
<tr>
<td></td>
<td>mtr. 1.</td>
<td>Meribaal, 1.</td>
</tr>
<tr>
<td>3</td>
<td>bšt. hštr. lfl. P. mšmyd. nbl. [yn.] yšn. lbd. ≠c. qf.</td>
<td>In the tenth year to [. . . ] from Shemida, a jar of old wine. To Baala [(son of) ?]</td>
</tr>
<tr>
<td>4</td>
<td>[bšt. hštr. mq[sh. l]lgdyw. nbl. [yn.] yšn. ]</td>
<td>[In] the ninth year from Qo[soh] to Gaddiyaw, a jar of old wine.</td>
</tr>
<tr>
<td>5</td>
<td>bšt. hštr. mqsl. lgdyw. nbl. [yn.] yšn.</td>
<td>In the ninth year from Qosoh to Gaddiyaw, a jar of old wine.</td>
</tr>
<tr>
<td>6</td>
<td>bšt. [hštr. mqsl. lgdyw. nbl. [yn.] yšn.</td>
<td>In [the ninth year from Qosoh] to Gaddiyaw, a jar of old wine.</td>
</tr>
<tr>
<td>7</td>
<td>bšt. hštr. myst. lŒh.hšm. nbl. [yn.] yšn.</td>
<td>In the ninth year from Yasit to Ahinjoam, a jar of old wine.</td>
</tr>
<tr>
<td>8</td>
<td>[bšt. hštr. mqsl. lŒh.hšm. nbl. [yn.] yšn.</td>
<td>In the ninth year from Yasit to Ahin[joam], a jar of old wine.</td>
</tr>
<tr>
<td>9</td>
<td>bšt. hštr. myst. lŒh.hšm. nbl. [yn.] yšn.</td>
<td>In the ninth year from Yasit to Ahin[joam], a jar of old wine.</td>
</tr>
<tr>
<td>10</td>
<td>bšt. hštr. myst. lŒh.hšm. nbl. [yn.] yšn.</td>
<td>In the ninth year from Yasit to Ahin[joam], a jar of old wine.</td>
</tr>
<tr>
<td>12</td>
<td>bšt. hštr. mšptn. lŒlzmr. nbl. [yn.] yšn.</td>
<td>In the ninth year from Shiptan to Baalzemer, a jar of old wine.</td>
</tr>
<tr>
<td>13</td>
<td>bšt. hštr. m²br. šmryw. nbl. [yn.] yšn. lŒš[l]? mŒ [ntwl [. ]</td>
<td>In the tenth year from Abiezer to Shemaryaw, a jar of old wine to [Asa(?) . . . ] from Tawil.</td>
</tr>
<tr>
<td>14</td>
<td>bšt. [hštr. mŒ. prŒn. šmryw. nbl. [yn.] yšn.</td>
<td>In the ninth year from [Ga]t-Paran to Shemaryaw, a jar of old wine.</td>
</tr>
<tr>
<td>15</td>
<td>[hštr. mŒ. n]bl. yšn.</td>
<td>[. . . from Hašerot to [. . . , a jar of old wine.</td>
</tr>
<tr>
<td>16a</td>
<td>[hštr. mspr. lgdyw. nbl. šmn. rhš.</td>
<td>[In] the tenth year from Sepher to Gaddiyaw, a jar of fine oil.</td>
</tr>
<tr>
<td>16b</td>
<td>bšt. hštr. mspr. lgdyw. nbl. šmn. rhš.</td>
<td>In the tenth year from Sepher to Gaddiyaw, a jar of fine oil.</td>
</tr>
<tr>
<td>17a</td>
<td>bšt. hštr. m²zh. lgdyw. nbl. šmn. rhš.</td>
<td>In the tenth year from Azzah to Gaddiyaw, a jar of fine oil.</td>
</tr>
<tr>
<td>17b</td>
<td>[bšt. hštr. m²zh. lgdyw. nbl. šmn. rhš.</td>
<td>[In] the tenth year from Azzah to Gaddiyaw, a jar of fine oil.</td>
</tr>
</tbody>
</table>


**Nonbiblical clan/settlement/estate names are in bold type in the translation, gentilies in italics, and biblical clan names in bold italics.**
<table>
<thead>
<tr>
<th>Ostracon No.</th>
<th>Transliteration</th>
<th>Translation</th>
</tr>
</thead>
<tbody>
<tr>
<td>18</td>
<td>bš, h’reṣrt. mhšrt. lgdyw. nbl. šmn. rḥš</td>
<td>In the tenth year from Hāserot to Gaddiyaw, a jar of fine oil.</td>
</tr>
<tr>
<td>19</td>
<td>bš, h’reṣrt. myṣṭ. nbl. šmn. rḥš. Pḥḥ h’ḥm.</td>
<td>In the tenth year from Yāṣīt, a jar of fine oil to A[ḥi]noan.</td>
</tr>
<tr>
<td>20</td>
<td>bš, h’reṣrt. ynm. ḫm’un. ḫl[1. nbl. šmn. ḫ[r][ṣ. ]</td>
<td>In the [tenth year, wine] from Kerem-Hatt(el), a jar of fine oil.</td>
</tr>
<tr>
<td>21</td>
<td>bš, h’reṣrt. šmmyw. mlḥw L. nbl. šmn. rḥš.</td>
<td>In the tenth year to Shemaryaw from Ta[w]il, a jar of fine oil.</td>
</tr>
<tr>
<td>22</td>
<td>bš, 15 mlḥq. Pš’ prḥmlk. ḫš. mhšrt [. ]</td>
<td>In the 15th year from Heleq to Asa (son of) Ahimelek; Heles from Hāserot.</td>
</tr>
<tr>
<td>23</td>
<td>bš, 15 mlḥq. Pš’ prḥmlk. ḫš. mhšrt .</td>
<td>In the 15th year from Heleq to Asa (son of) Ahimelek; Heleš from Hāserot.</td>
</tr>
<tr>
<td>26</td>
<td>[ ḫš]. l’h[?]. n. mhšrt [. ]</td>
<td>[. . . from Heleq(?)] to Asa [. . .] from Ha[serot(?)].</td>
</tr>
<tr>
<td>27</td>
<td>bš, 15 mlḥq. Pš’ prḥmlk. b’ḥl. br’mn’y.</td>
<td>In the 15th year from Heleq to Asa (son of) Ahimelek; Heleq (the) Baalmeonite.</td>
</tr>
<tr>
<td>28</td>
<td>bš, 15 m’ḥḥr. Pš’ prḥmlk. b’ḥl. mh’ln.</td>
<td>In the 15th year from Abiezer to Asa (son of) Ahimelek; Baal from Elmatan.</td>
</tr>
<tr>
<td>29</td>
<td>bš, 15 mš[mšd. l’š’]. prḥmlk l’mnh. mspr.</td>
<td>In the 15th year from Shemida to Asa (son of) Ahimelek; [Gom?]er from Sepher.</td>
</tr>
<tr>
<td>30</td>
<td>bš, 15 mš[mšd. l’š’]. lḥš. gdyw. gr’q. ḫn’n.</td>
<td>In the 15th year from Shemida to Heleṣ (son of) Gaddiyaw; Gera (son of) Han[na].</td>
</tr>
<tr>
<td>31a</td>
<td>bš, h’15 mš[mšd. l’š’]. lḥš. ḫps’h. b’ḥl. zkr.</td>
<td>In the 15th year from Shemida to Heleṣ (son of) Apṣah; Baala (son of) Zeker.</td>
</tr>
<tr>
<td>31b</td>
<td>bš, h’15 mš[mšd. l’š’]. lḥš. ḫps’h. b’ḥl. z[kr. ]</td>
<td>In the [15]th year [from Shemid’a] to [Heleṣ (son of) Apṣah]; Baala (son of) Ze[ker].</td>
</tr>
<tr>
<td>32</td>
<td>bš, 15 mš&lt;md y’d’. lḥš. l’h’hm.</td>
<td>In the 15th year from Shemida to Heleṣ; Ahima.</td>
</tr>
<tr>
<td>33</td>
<td>[bšt. l]’5 mš[mšd. l’ḥš]. gdyw. n m[n].</td>
<td>[In the 1]5th year from Shemida to Heleṣ (son of) Gaddiyaw; [. . .?].</td>
</tr>
<tr>
<td>34</td>
<td>[bšl]. h’15 mš[mšd. l’ḥš]. gdyw. s’l</td>
<td>[In] the 15th year from Shemida to Heleṣ (son of) Gaddiyaw; [. . .?].</td>
</tr>
<tr>
<td>35</td>
<td>bšl. 15 mš[mšd. l’ḥš]. g’l[yw].</td>
<td>In the 15th year from Shemida to Heleṣ (son of) Gaddi[yaw]; Yaw[ . . .?].</td>
</tr>
<tr>
<td>36</td>
<td>[bšt. l]’5 mš[mšd] l’ḥš. yw[y].</td>
<td>[In the 15th year from] Shemida a . . .;</td>
</tr>
<tr>
<td>37</td>
<td>bšt. 15 mš[mšd] l’ḥš. b’h’m. b’h’m. b’ḥl. b’h’m.</td>
<td>In the 15th year from Shemida to Ahima; Asa (son of) Baalazker.</td>
</tr>
<tr>
<td>38</td>
<td>bšt. 15 mš[mšd] l’ḥš. b’h’m. b’h’m. b’h’m.</td>
<td>In the 15th year from Shemida to Ahima; [Ul]lah (son of) Ela.</td>
</tr>
<tr>
<td>39</td>
<td>bšt. 15 mš[mšd] l’ḥš. b’h’m. b’h’m. b’h’m.</td>
<td>In the 15th year from Shemida to Ahima; [A]sa (son of) [Baalazker(?)].</td>
</tr>
<tr>
<td>40</td>
<td>[mš[mšd]. b’h’m.</td>
<td>[. . . from] Shemida to [. . .]</td>
</tr>
<tr>
<td>41</td>
<td>l’ḥš. yw[yw].</td>
<td>[. . . shat(?)(son of) Egliyaw</td>
</tr>
<tr>
<td>Ostracon No.</td>
<td>Transliteration</td>
<td>Translation</td>
</tr>
<tr>
<td>--------------</td>
<td>----------------</td>
<td>-------------</td>
</tr>
<tr>
<td>43</td>
<td>bš[t]. h15 mbglh. [ ] lynn. [bš[r].]</td>
<td>In the [15th year from Hogla] to Hanan ([son of] Baara(?)); El[ . . . ].</td>
</tr>
<tr>
<td>44</td>
<td>[bš[t]. h15 mškm. [ ] hbdw. [ ] dhyw. gd[yw. mš[r][t].</td>
<td>[In] the 15th [year] from Shechem [ . . . ]; [ . . . ?].</td>
</tr>
<tr>
<td>45</td>
<td>bš[t]. h15 mbglh. [ ] lynn. [bš[r].]</td>
<td>In the 15th year from Hogla[h] to Hanan (son of) Ba[ar]a; [Meron(?)]yaw (son of) Nathan from Yāš[t].</td>
</tr>
<tr>
<td>46</td>
<td>bš[t]. 15 [ ] lynn. bʃ</td>
<td>In the 15th year [ . . ] to Hanan ([son of] Baara(?))</td>
</tr>
<tr>
<td>47</td>
<td>[bš[t. 15 m]lyd[w]. lynn. bʃ[r].]</td>
<td>[In the 15th year from] Hogla to Hanan (son of) Baara; M[eron]yaw (son of) Nathan(?) from Yāš[t].</td>
</tr>
<tr>
<td>49</td>
<td>bš[t. ] hbdw. [ . ] dyw. mš[r][t].</td>
<td>In the [?th] ye[ar . . . ] to He[leš . . . ]</td>
</tr>
<tr>
<td>50</td>
<td>bš[t. 15 lgm[r]. mn[C] h. ]</td>
<td>In the 15th year to Gomer from Noah; Obadyaw to[for(?)] U[ri]yaw.</td>
</tr>
<tr>
<td>51</td>
<td>bš[t. hš[r]. lʃ [ ] Ḥyd[yw. ]</td>
<td>In the tenth year to [ . . ]; Aha the Judah[ite].</td>
</tr>
<tr>
<td>52</td>
<td>b 15 lʃ[b] ʃ[yw. [ ]</td>
<td>In the 15th &lt;year&gt; [from] B[ . . ?]; Abiyaw [ . . . ].</td>
</tr>
<tr>
<td>53</td>
<td>bš[t. hš[r]. yn. kr[r]. hdl. bnl. ʃmn. rʃ.</td>
<td>In the tenth year, wine (from) Kerem-Hattel, instead of(?) a jar of fine oil.</td>
</tr>
<tr>
<td>54</td>
<td>bš[t. hš[r]. yn. kr[r]. hdl. ]</td>
<td>In the tenth year, wine (from) Kerem-Hattel, a jar of fine oil.</td>
</tr>
<tr>
<td>56</td>
<td>bš[t. 15 mṛt]. [ lnmʃ[. ] dl [. ] hbd.</td>
<td>In the 15th year from Hat[ . . ] to Nimsh<a href="?">i</a>; [ . . ?].</td>
</tr>
<tr>
<td>57</td>
<td>[ ] hbd[r]. yw. [ . ]</td>
<td>[ . . . ] Abdayaw(?)[ . . . ]</td>
</tr>
<tr>
<td>60</td>
<td>km[r. yh[yw[ly.</td>
<td>Kerem-Yehoeli</td>
</tr>
<tr>
<td>61</td>
<td>km[r. hdl. [. ] bš[t. 15</td>
<td>Kerem-Hattel [ . . . ] in the 15th year</td>
</tr>
<tr>
<td>62 (jar label)</td>
<td>[ ] ymr. ʃmy[d].</td>
<td>[ . . . ] wine (from) Shemid[a]</td>
</tr>
<tr>
<td>63 (jar label)</td>
<td>bš[t. ] lʃ[b]. [ ] mʃ[yw[.</td>
<td>In the [13th?] year [ . . . ] from Shemida [ . . . ]</td>
</tr>
<tr>
<td>3892</td>
<td>bš[t. hš[r]. yn. kr[r]. hdl. bnl. ʃmn. rʃ.</td>
<td>In the tenth year, wine (from) Kerem-Hattel, instead of(?) a jar of fine oil.</td>
</tr>
</tbody>
</table>
Shemida boam II (ca. 785–745 B.C.). They record the receipt of shipments of oil and wine in small amounts, one or two jars at a time, from a variety of settlements which were situated relatively close to the city, all but one being located in a zone 5–12 km distant from Samaria (the exception, Yashub, was 19 km south of Samaria, if its location has been correctly identified).30

The equivalence of the 100- or 150-person clan as a social unit to the village as its spatial manifestation is supported both by ethnographic parallels and by the occurrence in the eighth-century “Samaria ostraca” of place names that appear in the Hebrew Bible as the names of miśpāḥôt (see figure 9 and table 4 above; for general overviews of the Samaria ostraca and related bibliography, see Aharoni 1979:356–68 and Kaufman 1982). These 68 administrative documents (counting all the legible ostraca, including duplicates) were found in the Israelite royal city of Samaria and date probably from the reign of Jero-boam II (ca. 785–745 B.C.). They record the receipt of shipments of oil and wine in small amounts, one or two jars at a time, from a variety of settlements which were situated relatively close to the city, all but one being located in a zone 5–12 km distant from Samaria (the exception, Yashub, was 19 km south of Samaria, if its location has been correctly identified).30

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The Samaria ostraca show the correspondence between the clan as a social group—a group whose name is interpreted by later biblical writers, and probably also at the time in question, in genealogical terms, as the name of a founding ancestor—and the town or village it inhabited as a coresident community. This correspondence between clan and settlement was emphasized by C. H. J. de Geus in his ethnographically informed book The Tribes of Israel (1976:138ff.). He notes, in particular, that biblical texts often refer to the council of “elders” (zqēnîn) as the “elders of the town” (ziqnê hāʾārîr).31 The elders were the governing body of the clan by virtue of being its leading householders. As de Geus says, “It is a very old misunderstanding to make a contrast between this institution of the elders and the town. The
terminology does indeed derive from the ruling of the clan, but clan and town were parallel in the time of ancient Israel.” (ibid., p. 140). Note also that the Hebrew word "כָּל (kăl), which is often translated “city,” can refer to a permanent settlement of any size, walled or unwalled (see Deuteronomy 3:5; Esther 9:19), like Akkadian 𒈾𒇼—or possibly can refer even to a demarcated neighborhood or “quarter” within a larger settlement (e.g., "כָּל הָאָרֶץ (kăl hā’eretz) in 2 Samuel 5:9 and "כָּל הָהָרָה hammayim in 2 Samuel 12:27; in these cases, however, "כָּל may denote not an ordinary neighborhood but a fortified citadel within the town—McCartier 1984:310, 312).

De Geus points out the equivalence between the “elders of the town” (צִיץ הַאֲדֹנָי (țitzî ’ha’dônî) and those referred to elsewhere as the “masters” or “owners” of a given town (בָּעָל הַקָּנָה (bā‘al ḫānāh; Judges 9:51); that is, the independent heads of households who wielded influence as a collective governing body in a particular settlement, for example, the בָּעָלָּה (bā‘alāh, “house”) owners of Shechem” (Judges 9:2). In the case of smaller settlements, the “elders of the town” would have been equivalent to the elders of a single clan, but in larger settlements the elders of a particular clan must have been a subset of the entire group of householders (לְכָלָּה (lēkālāh), as in the case of the walled town of Shechem. Judges 9 recounts that the would-be king Abimelech, whose mother came from Shechem, spoke to “the whole clan of his mother’s בֶּט כִּיוֹם (bēt ‘iyyôm), which was small enough to be addressed as a group and to serve in turn as Abimelech’s agents, lobbying on his behalf throughout the entire town. This clan was obviously viewed by the biblical writer as a subset of the entire group of households resident in Shechem because its job was to propose to “all of the בָּעָלָּה (bā‘alāh)” that Abimelech be elected king of the whole town.

Thus the larger Iron Age walled towns like Shechem (Tell Balātāh), as well as Tirzah (Tell el-Fârîrah North), Mizpah (Tell en-Naṣîbeh), and Tell Beït Mirsim, whose domestic architecture is discussed in detail below, were probably inhabited by several המְגָפָה (mēgāphā), each occupying a residential quarter consisting of a patronymic (or sometimes a matronymic) association of neighboring households—like David’s Ephrathite clan which lived in Bethlehem, or like the clans which made up the town of Hebron. One such clan in Hebron is known in biblical tradition as the בֶּה דְת (bēh Déṭ, “sons of Heth” (usually translated “Hittites”), from whom the patriarch Abraham purchased the cave of Machpelah in which to bury his wife Sarah, who had died in Hebron (Genesis 23). The name “Hebron” itself means something like “congregation,” and the city was originally known by the name Qiryat-‘Arba‘ā, the “Fourfold City” or “City of Four (Clans?).” 33 The fact that this name for Hebron predates the Israelite period and survived from the Canaanite Bronze Age causes no difficulty for the “four-clan” hypothesis, because Canaanite towns were probably very similar to later Israelite towns in their clan-based neighborhood organization, which is attested also for much later Mediterranean cities (see chapter 6 above and the discussion of neighborhoods in Late Bronze Age Ugarit below in chapter 13.2).

Note also the recollection of the four clans of Hebron in the genealogical reference to four sons of Hebron in 1 Chronicles 2:43 (cf. the four המְגָפָה (mēgāphā) in the town of Qiryat-Yearim in 1 Chronicles 2:53). 34

The reference in Micah 5:1 to Ephrathah as a clan of Bethlehem suggests that familiarity with the clan

33 The name Ḥeḇrōn is probably related to the Hebrew root הָבָר, which means “to ally oneself, join forces”: cf. the noun הָבָר, “company, association.” The pre-Israelite name Qiryat-‘Arba‘ā is attributed in later biblical tradition to a man named, very oddly, “‘Arba‘ā” ("Four"), who is identified as one of the legendary giants of Canaan called “Anakim” (Joshua 14:15). But this is a patently etiological explanation written by a later (probably posttextic) editor who did not understand the meaning of the city’s name.

34 In light of the importance of clan-based subcommunities within walled towns, it is possible that the Hebrew term שָׂאָר, “gate,” could refer not just to a city gate but to a gate within the town which controlled access to a clan neighborhood, as in Ottoman Aleppo and Damascus (note what appears to be a gateway closing off a main street in Tell en-Naṣîbeh, locus 94; see figure 13 below). In that case, “gate” might have been a metonym for “clan” (cf. Old Babylonian bābtum, “city quarter = clan neighborhood,” versus bābum, “gate,” discussed in chapter 12.4 below). This seems to be the case in Ruth 3:11, where Boaz, an Ephrathite of Bethlehem and putative ancestor of King David, says to Ruth that “all the gate of my kindred (קֶלֶשָׂר אָם מְיָּוִת) knows what a fine woman you are.” The Septuagint translates σάραρ in this verse by φυλή, “clan” (note also the Old Latin translation tribus and Syriac ʿsrḥ, “family, tribe,” cited in Campbell 1975:124). A similar expression occurs in Ruth 4:10, where Boaz tells the witnesses that he is marrying Ruth, widow of Mahlon, so that Mahlon’s name will not be cut off from among his kinsmen (literally, “brothers”) and “from the gate of his place” (מִשְׂרָה אֲמִיתָן). The reference to Mahlon’s “brothers” and his “inheritance” (נָאָה לָא) in this verse suggest that σάραρ here is again a metonym for המְגָפָה—the primary social and legal body in ancient Israel—and not for the town as a whole, as it may also be in a number of Deuteronomic laws which enjoin the care of the propertyless “Levite” (cultic specialist), “alien sojourner” (גֵּר, widow, or orphan who is “within your gates” (e.g., Deuteronomy 5:14; 12:12; 14:21, 27–29; 15:7; 16:11, 14, 18, 17:2, 5, 8; 18:6; 23:17; 24:14; 26:12).
organization of ancient Israelite towns was not confined to the premonarchic period or the early monarchy, but survived until the end of the monarchy or later (depending on how late one wishes to date this text, whose final editing occurred after the Babylonian exile). The usual interpretation is that all of the residents of Bethlehem were a part of a larger Ephraimite group, but given the congruence between individual villages and clans in both ethnographic contexts and the Samaria ostraca (see below), it is more likely that the situation was reversed and the Ephraimite clan was a subset of the town of Bethlehem (cf. Gottwald 1979:269). The coresidence of clan members within a single town or village was necessary, or at least highly desirable, in order for the clan to function as an economic and cultic unit. The importance of the clan as a cultic community, in particular, is indicated in 1 Samuel 20, where David excuses his absence from Saul’s court by referring to an annual clan sacrifice (zebâh mišpâhâ) in Bethlehem he must attend—an explanation that was presumably still meaningful to the later authors of this episode.

In most smaller towns and villages, however, a clan would have occupied the entire settlement, as in the case of Gideon’s village of Ophrah which belonged to his Abiezrite clan (Judges 6:24; 8:32), and perhaps also Samuel’s village of Ramah (or Ramathaim), inhabited by the Zuphite clan (1 Samuel 1:1; 9:5), where a clan sacrifice was conducted that was attended by thirty men (1 Samuel 9:22). Thirty men is about what we would expect in a clan consisting of 18 households, of which one-third are three-generation joint-family households with three adult males per household on average (the senior male and two surviving adult sons), and two-thirds are in the nuclear-family phase of the household lifecycle, with only one adult male. The identification of the typical Israelite clan with a relatively small local community inhabiting one settlement is evident also in the story of the wise woman of Tekoa (2 Samuel 14), in which “the whole clan” (v. 7) confronts the woman to demand the death of her son who had killed his brother.

Baruch Halpern (1996:297) argues in connection with Samuel’s clan sacrifice in 1 Samuel 9 that: “Thirty adult males is too small a number to represent a full clan and too many, again, by a factor of 10, to represent a single housing compound [bêt ‘âb].” But although Halpern’s discussion of kinship in ancient Israel is quite informative and illuminating, we can be skeptical of his distinction between the “full clan” and the much smaller “clan section”—both called mišpâhâ, according to him (see also Halpern 1991:52–59). He regards the full clan as a large administrative district of which there were only five per tribe, on average, on the basis of the Priestly genealogy in Numbers 26. The clan section, on the other hand, corresponds to a village or a portion of a larger settlement. Halpern asserts this distinction in order to account for the cases in which the mišpâhâ is clearly a small coresident group, as in the story of the wise woman of Tekoa and the story of Abimelech, cited above. But in both these stories this small kinship group (Halpern’s “clan section”) is called explicitly “the whole clan” (kol-[ham]mišpâhâ). The idea that there were much larger clan districts, also called mišpâhôt, is supported only by the later genealogies, in which older kinship terms were reinterpreted and earlier source material was schematized in line with the Priestly writer’s concerns. And rather than confirming the completeness of the genealogical list of clans, close inspection of the Samaria ostraca shows that there is no need to posit a complex two-level clan structure between the tribe (šèbet) and the

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35 Samuel’s hometown is called “Zuph” rather than “Ramah” in 1 Samuel 9:5. The MT has Saul and his servant arriving in the “land of Zuph,” but as McCarter (1980:164, 168) observes, LXX has only “Zuph.” Ramah is not mentioned in this passage and the name Zuph is the antecedent of the subsequent reference to “this town” in v. 6. The expansion from “Zuph” to “land of Zuph” in the MT can be explained as an attempt to make sense of the verse by a later scribe who knew that Samuel lived in Ramah and did not realize that Zuph as a clan was equivalent to Ramah as a settlement, assuming instead that Zuph was the name of a larger district, perhaps under the influence of the preceding place names in v. 4, as McCarter suggests.

36 There are thus obvious diachronic differences in the meanings of terms such as mišpâhâ and bêt ‘âb (which became bêt ‘âbôt in postexilic texts, but this is not a warrant for saying that biblical kinship terminology is “very plastic” (Halpern 1996:296) or “very loose” (Lemche 1985:260). It is more likely that distinctions are being imposed here by modern scholars that were not meaningful to Iron Age Israelites. Lemche says that the biblical terminology is loose because he wants to attribute the meaning “lineage” (what I have been calling a “clan” or “patronymic association” and what Halpern calls a “clan section”) to the term bêt ‘âb, rather than to mišpâhâ, arguing that bêt ‘âb variously denotes a nuclear family, an extended family, or a lineage. But the nuclear family and extended (or joint) family are two phases of the same patrilocal household structure, dwelling in one house, as we have seen, and Lemche’s examples of the bêt ‘âb as a broader lineage are either unconvincing or are demonstrably postexilic (ibid., pp. 245–59). Following some anthropological writers, Lemche distinguishes the term “clan,” referring to a broader and largely nonfunctional kinship group, from the more practically effective “lineage”; but the equivalent of this kind of nonfunctional clan in Iron Age Israel was the so-called tribe (šèbet), which was relatively fluid and ill defined, as many have pointed out, whereas the local lineage in Israel was the mišpâhâ, as is shown by the texts cited above.
household (bêt ‘āb), as I will demonstrate below. The often cited story of Achan in Joshua 7 thus preserves an accurate picture of the kinship organization of Iron Age Israel, in which a man was identified by his sēbet, mîšpâhâ, and bêt ‘āb, with no hint that the mîšpâhâ was an ambiguous term which could refer either to a large clan district or to a small “clan section.”

De Geus (1976:138f), likewise, qualifies his position unnecessarily when he concludes that some clans, at least, were larger territorial units, akin to provincial districts, on the basis of the six sons of Manasseh listed in the Priestly genealogy in Numbers 26 and Joshua 17. Here he adopts the conventional interpretation of the Samaria ostraca that I will challenge below, which divides the territory of Manasseh (the central hill country south of the Jezreel valley in which Samaria was located) among the small number of clans mentioned in this genealogy. But if we dispense with the a priori assumption that the Priestly list of clans is complete, there is no basis in the ostraca themselves for distinguishing between large “clan districts,” corresponding to the names preserved in Numbers 26, and towns and villages located within those districts—places which are, by definition, not attested as clans in the Bible. Almost all of the place names in the ostraca can be treated as referring to clan-settlements of the same type, and the large number of these places mentioned in just the 68 preserved ostraca casts doubt on the commonly held notion that the typical Israelite clan was a large social group covering a wide territory and encompassing numerous settlements. That idea is derived solely from the small number of clans per tribe mentioned in Numbers 26 and Joshua 17, in what is, in all likelihood, a postexilic Priestly genealogy compiled hundreds of years after the eighth-century ostraca were written.

A more accurate estimate of the number of clans in Manasseh can perhaps be derived, not from the schematized genealogy of Manasseh in Numbers 26:29–33, but from the reference to 52 läpûm mustered from Manasseh in Numbers 26:34 (given as 32 läpûm in Numbers 1:35)—assuming that the word läpel in the underlying source document denoted a military company mustered from a clan, however the later Priestly writer might have understood this term (see the discussion of this issue above). But there is no guarantee that every clan sent an läpel-company to the troop-musters on which the census lists in Numbers are based. In these lists there is an average of 50 läpûm per tribe, and if these represented 50 clans per tribe, with an average of 100–150 persons per clan, a typical tribal population would have been only 5,000–7,500, with 60,000–90,000 in all twelve tribes. This seems too low a figure for the Iron II Israelite population. But even if 50 is an accurate estimate of the number of clans per tribe, there might well have been more than the twelve tribes assumed by the Priestly writer—in biblical tradition many more gentiles appear that are applied to sizable districts than just the twelve names of the traditional tribes. Furthermore, the highland Israelite population was not vastly greater than these calculations suggest: at the end of the settlement period it is estimated to have been about 150,000 (Stager 1998:134), including Transjordan but excluding Galilee, after which there was perhaps a very slow natural increase on the order of 0.1–0.2% per year.

The occurrence in the Samaria ostraca of a few Manassite clan names which are listed in Numbers 26 and Joshua 17 does show that the Priestly genealogy preserves some accurate information about the Iron Age, but it is unlikely that the genealogy provides a complete picture, for it is affected by the particular concerns and selective bias of the later writer. The genealogy states first that Joseph’s firstborn son Manasseh, the eponymous founder of the tribe of Manasseh, was the father of Machir, who was the father of Gilead, in recognition of the Manassite claim to the region of Gilead in Transjordan. Gilead was in turn the father of six sons who represent the western clans of Manasseh in Cisjordan: Abiezer, whose descendents were the clan (mîšpâhâ) of the Abiezrites; Heleq, founder of the clan of the Heleqites; Asriel and the Asrielites; Shechem and the Shechemites; Shemida and the Shemidaites; and Hepher and the Hepherites. Here we obviously have a later schematization and selection of earlier source material, interpreting traditional tribal, clan, and place names and their political affiliations in terms of a strict genealogical succession from the twelve sons of Jacob. 

37 Note also that the steplike poetic parallelism in the couplet in Jeremiah 3:14, “I will take you, one from each village (‘îr), two from each clan (mîšpâhâ),” implies not that “the clan is larger than the village” (Halpern 1991:52f.), as if a precise mathematical ratio were intended here, but that village and clan were basically equivalent. Halpern concludes correctly, however, that the term mîšpâhâ in Jeremiah 3:14 denotes something much smaller than a large clan district; it is a “clan section,” “a body numerically equivalent to one or more villages, or a part of a city” (ibid., p. 55f.).

38 Exceptions are Kerem-Hattel and Kerem-Yehoeli, which were probably royal vineyards (Aharoni 1979:366).
recorded here the average number of mishpahoth per 
shetev in old Israel is not one of the virtues with 
which we can credit the traditionist of Numbers 26. A 
comparision of this list with the list of Genesis 46:8– 
26 . . . where the mishpahoth of Numbers 26 appear 
only as banim, ‘sons,’ of the tribal ancestors, shows 
that these compendia of tribal subunits were variable 
both as to the names and numbers of subdivisions 
within each tribe.”

The existence of Manassite clan names which are 
feminine in form was handled by the Priestly gene-
alogist by stating that Zelophehad son of Hepher son 
of Gilead had no sons but only five daughters, whose 
names were Mahlah, Noah, Hoglah, Milcah, and Tir-
zah. When the Samaria ostraca were discovered, it 
was immediately noted that Abiezer, Heleq, 
Asriel,39 Shechem, and Shemida are mentioned as 
was immediately noted that Abiezer, Heleq, 
Asriel,39 Shechem, and Shemida are mentioned as 
the very same place at the beginning of the document 
where in other ostraca the seven known clan names 
appear—the names Azzah (nos. 2, 17a, 17b), Qoșoh 
(nos. 4–7), Geba (no. 8), Yaṣit (nos. 9, 10), Shiptan 
(no. 12), Tawil (nos. 13, 21), and Sepher (nos. 16a, 
16b), none of which is listed as a clan in the Bible. It 
might be objected that these are the names of settle-
ments rather than clans, as is more obviously the case 
(based on the meanings of the names) with Beerayim 
(no. 1), [Ga]-Paran (no. 14), and Haṣeret (no. 18). 
But the distinction between clan and settlement is 
what is in question here. Shechem (no. 44) was the 
name of a major town long before it appeared in the 
biblical genealogy as a clan and in the Samaria os-
traca as the source of a shipment—it was known by 
that name as far back as the Late Bronze Age (if not 
earlier) when it appears as “Ṣakmu” in an Amarna 
letter (EA 289; see Moran 1992:332). In the case of 
Shechem and Tirzah, according to the conventional 
interpretation, the clan name and the settlement name 
were the same; but we can apply the same reasoning 
to other place names in the Samaria ostraca, for there 
is no reason to think that the Priestly list of clans is 
complete. Indeed, the Priestly reduction of the clans 
of Manasseh to the six sons of Gilead fits a well-
known genealogical pattern in postexilic biblical lit-
erature that favors sets of six or twelve sons.

The key question is how to interpret the year 15 
ostraca in which not one but two place names appear. 
Two basic formats were used in the Samaria ostraca 
(see table 4 and figure 9 above). Ostraca of the king’s 
ninth and tenth years have the form “date, place of 
origin, recipient (or sender),41 commodity”; that is,

39 The name šriel, “<A>šriel,” in nos. 42 and 48 was origi-

nally read šag (or šeq), but Albright’s (1931) proposal to 
read this as the clan “Asriel” known in the Bible has been 
widely accepted (also Cross 1961).

40 Note also that in the genealogy of Manasseh in 1 Chroni-

cles 7:14–19, Mahlah is a sibling of Abiezer and not his 
brother’s great-granddaughter, and Abiezer himself is not 
the son but the nephew of Gilead (his mother is Gilead’s 
sister), while Shechem is the son (not the brother) of 
Shemida, and Asriel is placed in an earlier generation as a 
son of Manasseh and brother of Machir, father of Gilead. 
Hepher is not even mentioned.

41 Anson Rainey (1967b; 1979) is probably correct in his 
view that the men whose names follow the preposition l 
(the so-called l-men) were the recipients of the commodi-
ties and not, as Yadin (1959), Cross (1975:8–10), 
and Kaufman (1982:236f.) have argued, taxpayers who had sent 
the commodities to the king and to whom these shipments 
were being credited. In view of the small quantities being 
sent, and adopting the most common interpretation of the 
preposition l in this context, it is plausible that the recipi-
ents were landlords resident in Samaria, as Rainey pro-
poses, “eating at the king’s table,” but providing for them-
selves from the revenue of scattered clan-settlements (or 
clan-segments of larger settlements) which had been 
granted to them by the king. But whether the l-men were
(a) In the ninth/tenth year, (b) from GN (a settlement/clan), (c) to PN, (d) a jar of old wine/a jar of fine oil. Sometimes items (b) and (c) are reversed, and in two ostraca from the tenth year (nos. 1 and 2), secondary personal names are appended followed by numerical amounts. Ostraca of the fifteenth year, on the other hand, have the form “date, place of origin, recipient, secondary person, place of origin of secondary person (optional)”; i.e., (a) In the 15th year, (b) from GN (a settlement/clan), (c) to PN, (d) PN2, (e) from GN. The change in the format of these notations between year 10 and year 15 is also evident in the use of Egyptian hieratic numerals for the year instead of spelling it out, as was done in the ninth- and tenth-year ostraca, and in the fact that the commodity is not mentioned.

There is no need, however, to account for this change in scribal practice by concluding that the year 15 ostraca belong to the reign of a different king (see Kaufman 1982:235; cf. Aharoni 1979:366). And despite the change in format, it cannot be a matter of the introduction of a new system of clan-based reckoning between the tenth and fifteenth years, in which the “clan district” was now mentioned first and the village within this district (originally the only place given) was placed in a secondary position at the end of the document (see, e.g., Aharoni 1979:367). Biblically attested clans are listed as the source of the shipment already in year 10 (Shemida in no. 3 and Abiezer in no. 13), and no other place name is given to identify the village of origin within the supposed clan district. It is quite possible that biblically attested clan names would have appeared in ostraca of year 9 also, if our sample from that year were larger. Similarly, not all of the places of origin listed at the beginning of the year 15 ostraca are biblically attested clans. In two cases, although the text is damaged, nonbiblical names are given (b‘l in no. 52 and htl in no. 56; note that it is unlikely that the latter is the royal vineyard “Kerem-Hattel,” which in other ostraca is always written krm htl and not simply htl).

What, then, is the relationship between the place of origin of the shipment, given at the beginning of the year 15 ostraca, and the place name associated with the secondary person, which is given in 11 of the 19 year 15 ostraca where this information is legible (counting duplicates)? Whenever it is mentioned, this second place is different from the first, thus the conventional interpretation is that the second place name refers to the specific town or village within a larger clan district from which the shipment came. But in 8 of the 19 legible year 15 ostraca no second place name is given, indicating that this information was optional, which is very odd under the clan-district interpretation, because one would expect that the specific place of origin within a large district was an important piece of information to record, so that villages which had avoided payment could be identified. The situation can be explained, however, if the second place name was intended not to identify the place of origin of the oil or wine (indicated by the first place name), but rather the hometown of the man who delivered the shipment—the “non-l-man,” whom the scribes of year 15 were usually careful to identify either by patronym, gentilic, or village. A village name or gentilic was supplied whenever the deliveryman’s hometown was different from the place of origin of the shipment, which is why the second place name, if it occurs, always follows the name of the secondary person (the “non-l-man”) at the end of the document.

In other words, under a system in which an entire clan-community was responsible to send periodically a jar or two of oil or wine to Samaria, whether to an absentee landlord or to the king on behalf of that landlord, it was most efficient to entrust this small shipment to someone who was making the trip to Samaria on other business—perhaps with a spare donkey upon which an extra jar or two could be loaded. In two cases in the preserved ostraca this deliveryman was a foreigner who was identified only by his gentilic: Baala the Baalmeonite (no. 27), from the town of Baalmeon in Transjordan (modern Ma’in south of Madaba), and Aha the Judahite (no. 51). In other cases the deliveryman was from another clan-settlement in Manasseh, from which in many instances a shipment to someone else is recorded in other ostraca. In still other cases, the deliveryman took charge of the shipment on behalf of his own clan-settlement, and so is identified only by his name and (usually) his patronym.

\[\text{References} \]

42 It goes beyond the evidence in the ostraca themselves to suggest that: “The district (clan, village, or estate) is listed precisely or imprecisely since the district in question identifies the quality of the product, especially in the case of aged wine” (Cross 1975:10). This is an unlikely explanation for the omission of the second place name.

43 Aharoni (1979:360) interprets the gentilic b‘l’mn‘ny in no. 27 as a reference to an otherwise unknown village of Baalmeon in the clan-district of Helq in Manasseh. But it is more likely that the well-known Moabite town of Baalmeon was intended here, especially in view of the reference to another foreigner, Aha the Judahite, in no. 51.
The “administrative reform” between the tenth and fifteenth years therefore consisted in identifying more carefully this deliveryman (and often the recipient as well, whose patronym is frequently given), in case of a dispute about whether the delivery was actually made. The first three elements (date, place of origin, recipient) of the year 15 ostraca remained the same as in the year 9/10 ostraca, both in format and meaning, but the latter part of the document was expanded in order to specify the deliveryman. This is a simpler and more plausible explanation than the clan-district interpretation, which holds that the meaning of the first place name in this well-established formula changed from “village” to “clan-district.”

However we interpret the Samaria ostraca, these texts make clear the continuing importance in ancient Israel, well into the monarchic period, of cohesive clans as the basic economic and social units responsible for organizing resources and providing goods and services to their overlords. This arrangement was much simpler administratively than the alternative, which would have involved close supervision of dispersed settlements by a large corps of royal officials. It was also congruent with a longstanding mode of traditional patrimonial authority by which political leaders acquired surpluses and pooled resources in all periods in the Levant, regardless of the spatial scale of the overarching political regime, ranging from tiny “chiefdoms” consisting of a few villages to larger kingdoms or “states” spanning wide areas (if, indeed, this quantitative distinction had any qualitative social-structural significance in the Bronze or Iron Ages). It is only with the breakdown of traditional clan structures beginning in the late eighth and seventh centuries B.C. that the patrimonial mode of authority began to give way to more individualized and depersonalized social and economic relations, and a concomitant reinterpretation of the old Canaanite and Israelite “house of the father.”

6. House Plans and Roofed Living Space

Returning now to the evidence of domestic architecture, it should be noted that an urban pattern similar to that of Tell Beit Mirsim (see figures 5, 7, and 8 above) can be detected in the plans from Roland de Vaux’s excavations at Tell el-Fâr‘ah (North), biblical Tirzah, especially in stratum VIIb, where thirteen more-or-less complete pillared houses were uncovered near the city wall.44 Excavation did not progress far enough into the city to reveal much about the interrelationship of streets, culs-de-sac, and dwellings; but two houses in stratum VIIb (nos. 410A and 425) share a walled forecourt, and two other houses (nos. 149B and 180) open onto a secluded plaza (locus 178) near the city wall (see figure 10 below). De Vaux (1956:133) exaggerated the similarity in size among the stratum VIIb houses, which date to the early monarchic period in the tenth century B.C. (Iron IIA), arguing that “the very uniformity of the dwellings shows that there was no great social inequality among the inhabitants.” But the houses in stratum VIIb range in area from 52–121 m² with a mean area of ca. 77 m² and a relatively high standard deviation of 19 m² (see table 9 below, based on the internal surface areas of the VIIb houses reported in Chambon 1984:32, table 1).

Furthermore, the unusual size of the largest pillared building (no. 355), which was originally 108 m² in area and was later expanded to 121 m², is inadequate warrant for regarding it as “un bâtiment de caractère collectif, magasin ou entrepôt” rather than an ordinary dwelling, as Alain Chambon (1984:35) proposes. House 355 was built on the same plan as the other houses in stratum VIIb, and Chambon provides no evidence for his interpretation other than the unusual length of the central courtyard (which nonetheless occupies only 32–35% of the total area of the house—a typical figure, according to his own calculations), and the unusual size of the adjacent paved siderooms (p. 32). But the detailed drawing of house 355 in plate 17 (p. 164) and plan 3 shows that the pavement in the siderooms did not extend the full length of the courtyard, contrary to Chambon’s isometric reconstruction. This indicates that these “pièces ouvertes” beside the courtyard (which were almost certainly stable) were subdivided in house 355 and thus were not as disproportionately large as Chambon suggests; indeed, the paved and unpaved portions have different locus numbers on the detailed plan.

There was thus one exceptionally large dwelling (no. 355 = 121 m²) in stratum VIIb of Tell el-Fâr‘ah (North), as well as two exceptionally small dwellings (no. 425 = 52 m² and 180 = 53 m²), and a range of sizes in between. The supposed “social equality” among the tenth-century residents of the site accordingly finds no confirmation in the domestic architecture of the site. Furthermore, what de Vaux regarded as the contrasting “inequality” among the five dwellings that were uncovered in the later stratum VIIId, which were 53, 62, 78, 86, and 103 m² in area (see dimensions of this house are clear, however, as shown in the reconstruction of the wall lines in plan 3.

44 In his final report on the Iron Age strata, Alain Chambon (1984:22–38) gives detailed data for only twelve houses because the eastern wall of House 425, located immediately to the east of House 410A, is not well preserved. The basic
figure 11), accords quite well with the variation in the sizes of the VIIb houses—assuming, of course, that the three smallest VIId houses were actually con-
temporary with the two largest houses in that stratum (see de Vaux 1956:134 and cf. McClellan 1987:86).

Note that the mean house areas within the two strata (76.8 m$^2$ in VIIb and 76.4 m$^2$ in VIId) and their standard deviations (19.2 m$^2$ in VIIb and 19.7 m$^2$ in VIId) are strikingly similar. From this limited sample there is no evidence of an increase in social inequality.

Figure 10. Tell el-Fārā'ah (North), Stratum VIIb (after Chambon 1984:plan 3)
In my opinion, the plans from Tell Beit Mirsim, Tell el-Fàr’ah (North), and also Tell en-Naṣbeh (McCown et al. 1947; cf. McClellan 1984; Zorn 1994) indicate that Stager’s view of Israelite household organization—that the joint-family bêr ẓâb characteristic of the premonarchic Iron I period continued to be important even in the more congested urban settlements of the monarchic Iron Age II—is basically correct. Unfortunately, the published report on Beth-shemesh, another extensively excavated Iron II site, presents too many stratigraphic problems to be used here, although it confirms the general picture provided by Tell Beit Mirsim, Tell el-Fàr’ah (N), and Tell en-Naṣbeh, as Yigal Shiloh (1980:29 and p. 33 n.17) points out.

Stager (1985a:18) has suggested, however, that individual pillared houses were inhabited by nuclear families in both Iron I and Iron II (so also Holladay 1992:310; 1995:387). He assumes that each conjugal family occupied a separate house rather than a single room within one house; thus the entire joint family (bêr ẓâb) resided, not in one large house, but in a cluster of neighboring houses. This interpretation may be plausible for some of the village compounds of Iron I Palestine, where individual pillared houses were considerably smaller, on average, than in Iron II towns; but it is less likely for the later period. Stager’s calculations (p. 21, table 3) show that four houses occupied 164 m² in the Iron I village at ẓAi (mean size 41 m²) and six houses occupied 193 m² at Iron I Raddana (mean size 32 m²). If the main room or “courtyard” was not covered, the second-story living area in houses at these sites averaged only 30 m² and 17 m², respectively, although the living space on the upper floor might have been augmented by space on the ground floor that was not devoted to animals or storage. Iron Age I pillared houses were somewhat larger at Tel Masos, in the northern Negev, where six houses occupied 430 m² (mean size 72 m²), although still smaller on average than similar dwellings in the Iron II period. In contrast, as I will show below, the largest one-third of pillared houses found in Iron II walled towns could easily have accommodated entire joint-family households, assuming that such households consisted of 10 persons on average (see chapter 7.2).45

In some cases, of course, patrilinearly extended families might have been spread among neighboring pillared buildings in Iron II towns, as seems to have been the case in some Iron I villages. Indeed, there

Figure 11. Tell el-Fàr’ah (North), Stratum VIIId (after Chambon 1984:plan 5)

45 Note that the 2.2-ha fortified site of Khirbat al-Mudayna al-ʿAliya in the eastern Transjordanian highlands, recently investigated by Bruce Routledge (2000), now provides a large sample of relatively large pillared houses dating to the Iron I period. Approximately 35–45 dwelling units occupied ca. 1 ha of the site’s surface. Routledge concludes that “houses with ground plans of over 100 m² in area constitute at least 20–25% of all houses at the site and hence are far from a rare occurrence. By almost any standard, these houses could accommodate co-resident [joint] families in the uppermost size ranges that we would expect” (p. 61).
are examples in Tell Beit Mirsim in which two pillar-laid houses share a common forecourt or alley with a single exit to the street (see figures 7 and 8 above), a phenomenon also found in Tell el-Fârâ‘ah (North) VIIb (see figure 10 above). It can be argued, however, that the smaller dwellings adjacent to larger joint-family houses were normally occupied by nuclear families consisting of more distant kinsfolk or quasi-kin clients, rather than by members of the same patrilocal family. This was probably the case even in certain Iron I villages. In ‘Izbat Šârâth (stratum II), for example, a large pillar-laid house (Building 109b), which measures ca. 12 × 16 m (including the outer walls, which were up to 1.4 m thick), occupied the center of the village (Finkelstein et al. 1986:14–18; Finkelstein 1988:75–78). As many as 20 smaller houses (ca. 6 × 6 m, or 36 m², based on the two shown on the published plan) were arranged around the periphery of the site (Finkelstein et al. 1986:21). The internal measurements of the large house were 8.8 × 13.9 m, or 122 m². This building could easily have housed a large joint-family household of 12 or more persons, if we use Raoul Naroll’s (1962) rather high cross-cultural estimate of 10 m² of roofed living space per person. Even if we take a minimalist position and assume that the central room of the large house was an unroofed courtyard, thereby reducing its upper-floor living area to 90 m² (the central longitudinal room between the rows of pillars measures 3.5 × 10.8 m, or 38 m²), this structure could have housed a medium-sized joint-family household of 9 persons. And note that even with an unroofed courtyard the actual living space in ‘Izbat Šârâth Building 109b might still have been as much as 110 m², if we add to the upper floor the ground floor broadroom 119b, which measures 2.5 × 8.7 m (ca. 22 m²).

Stager (1985a:18) uses Naroll’s average in estimating Iron Age household size and settlement population, proposing 10 m² of roofed dwelling space per person for both Iron I and Iron II; and he cites Carol Kramer’s (1979:155) ethnoarchaeological study of a Kurdish village in support of this figure. But an average of 10 m² per person is more plausible for an unfortified Iron I village than for an Iron II walled town because it is reasonable to suppose that population density was greater in urban settings, as Kramer (1980:324f.) herself acknowledges. For the same reason, Holladay’s (1992:312) high estimate of 21 m² per person of total roofed area (including, however, wall thickness, stables, and storage space) seems unlikely. This estimate is derived from Watson’s ethnoarchaeological study of the “sprawling” village of “Hasanabad” in western Iran; but as Holladay notes, “the typical Iranian agriculturalist’s house included a stable area for traction and transport animals (cows, bullocks, and donkeys), together with folding space for an average of 21 sheep and goats” (p. 312), yielding a relatively low density of (human) occupation. By no stretch of the imagination could there have been room for so many animals in even the largest Israelite pillared houses which have been discovered in the crowded towns of Iron II Palestine, calling into question the suitability of this ethnographic analogy drawn from modern unwalled Kurdish villages in Iran. What is closely analogous to the Israelite situation, however, is the average roofed dwelling area per person reported by Watson (1979:291), which is 7.3 m². (See also Postgate 1994 on population density at Abu Salabikh, an Early Dynastic urban site in southern Iraq, where relatively high density estimates are given.)

Yigal Shiloh (1980), in contrast, assumed a rather high urban density coefficient of 40–50 persons per dunam (400–500 per hectare) in his Iron II population estimates, a figure which corresponds to an average of 8 persons per house at Tell Beit Mirsim (stratum A), according to his calculations, which in turn is equivalent to only 5–7 m² of roofed dwelling space per person, although Shiloh does not express it in these terms. In defense of his density coefficient of 400–500 per hectare, Shiloh cites the work of Magen Broshi (1980), who investigated the population of Palestine in the Roman-Byzantine era and proposed an urban density in that period of 400 per hectare based on early twentieth-century population data for Levantine walled cities such as Aleppo, Damascus, Tripoli, Acre, and Jerusalem. Broshi maintains, however, that 400–500 per hectare is a maximal figure for crowded, long-established cities, and in subsequent writings he has favored a much lower estimate of 250 per hectare in Palestine during the Bronze and Iron Ages (Broshi and Gophna 1984; 1986; Broshi and Finkelstein 1992). Broshi and others have accordingly argued that Shiloh’s estimate of 8 persons per dwelling is too high, preferring instead a mean household size of 4–5 (i.e., “nuclear-family households”; see Broshi and Gophna 1984:42 and 51 n.4; 1986).

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46 According to the data presented below in table 5, the total upper-story roofed area of the excavated houses at Tell Beit Mirsim was at least 1489 m², assuming that courtyards were entirely uncovered; it was 1793 m² if 50% of each courtyard was covered by the upper floor; and it was 2093 m² if the entire courtyard was covered. Assuming Shiloh’s figure of 8 per house, or 288 inhabitants in 36 houses, the mean roofed area per person was 5.2–7.3 m².
Stager 1985a:18; Finkelstein 1988:268f. n.22; 1990). This corresponds to a population density of no more than 270 per hectare in Iron II Tell Beit Mirsim, according to Broshi and Finkelstein.

But these three average measures—mean household size, density per hectare, and roofed space per person—can give a misleading picture of urban social organization, because they mask the wide variation both in individual house (and household) sizes and in the population densities within different types of settlements. I have detailed below the range of variation in the size of Iron II houses excavated at Tell Beit Mirsim, Tell el-Fârâh (North), and Tell en-Nâşbeh, together with arguments for an estimate of roofed living area per person ranging from 8 to 10 m², with an estimate at the low end of this range being more suitable for Tell Beit Mirsim, in particular. The variation in house size was produced not by disparities in wealth, for the most part, but by a system of patriarchal joint-family coresidence under preindustrial demographic conditions. Furthermore, my analysis of individual roofed areas, in conjunction with demographically plausible estimates of the average number of household members, leads to an estimate of population density for most Israeleite walled towns of ca. 300–400 per hectare (leaving aside a few specialized administrative centers such as Hazor and Megiddo in which a large proportion of the walled area was nonresidential and the overall density was accordingly much lower).

This density estimate is lower than Shiloh’s estimate but higher than Broshi’s, and it is supported by data from other preindustrial Levantine cities such as eighteenth-century Aleppo (see Marcus 1989, discussed above in chapter 7.3), which provides a better analogy than later Levantine cities of the early twentieth century. Note that a range of 300–400 persons per hectare in walled towns takes account of different uses of urban space: the more space devoted to nonresidential (e.g., administrative, commercial, or religious) purposes, the more likely is an average settlement density at the lower end of this range (thus eighteenth-century Aleppo had ca. 300 persons per hectare). In a small walled town like Tell Beit Mirsim, where there is no evidence that a large proportion of urban space was devoted to nonresidential uses, a higher estimate is more appropriate. Unwalled villages typically do not have large nonresidential structures, but they are also characterized by more open space with fewer constraints on peripheral expansion. Thus village population density is usually lower, on the order of 200–250 per hectare, as a number of ethnographic studies have shown (references are cited by Broshi and Finkelstein 1992:48).

Problems arise, however, when this ethnographically derived average of 250 persons per hectare is used by Broshi and others to estimate the population of specific walled towns in Iron Age Palestine. A density of 250 per hectare either pertains only to unwalled villages, or else it averages into one figure the quite different estimates one should apply to low-density villages as opposed to higher-density walled towns—and assumes, moreover, that a large proportion of urban space is devoted to nonresidential uses, which is not true of all towns. It is therefore incorrect to apply this density coefficient to the continuously built-up residential areas of a particular walled site such as Tell Beit Mirsim, as has been done by Broshi and Finkelstein (1992).47

Before examining the house sizes in detail, it must first be noted that Shiloh’s count of the houses in Tell Beit Mirsim is slightly inaccurate and his calculations must therefore be adjusted. He counted 36 “dwelling units of the ‘four-room house’ and its subtypes” in the areas excavated by Albright; and because 0.68 hectares or ca. 22% of the three-hectare tell was excavated, he calculated that the entire town had 164 houses or 1,312 people (assuming 8 persons per house), and that the population density was 437 per hectare, which suits his initial estimate of 400–500 per hectare (1980:29). Shiloh’s figure of 36 dwellings in Tell Beit Mirsim was also used by Broshi and Gophna (1984:42), although they estimated only 4–5 persons per house and thus arrived at a density of 270 per hectare. Frank Braemer (1982:182–98), on the other hand, in his detailed catalogue of Iron Age Levantine houses, identified only 27 houses in the plans of Tell Beit Mirsim, of which 15 are in the northwestern sector and 12 are in the southeastern (cf. Albright et al. 1943:plates 3–7). But Braemer’s catalogue is incomplete, because he failed to include the two northernmost houses near the city wall in the northwestern sector (rooms NW 22/17; 32/1, 3, 4, 15; 47 Gloria London (1992:74) takes note of the difference between urban and rural settlement densities; but she still uses a low overall density coefficient of 200 per hectare (or less) for both walled towns and unwalled villages. Note also that Bronze and Iron Age cities in which, as she says, 25–50% of the urban area is devoted to “public space and official buildings,” should not for that reason be characterized as “non-agricultural” (ibid.), which implies that the bulk of the population in these administrative centers did not subsist on agriculture. There is an implicit urban-versus-rural (“tell-versus-village”) economic dichotomy here, and in many other similar studies, which is unwarranted, although there was indeed a dichotomy in terms of settlement density that should be attributed not to a qualitative socioeconomic difference but simply to the physical constraint imposed by the existence of a city wall.)
33/1, 2; and rooms NW 33/10, 12, 13, 17)—although it must be said that one of these houses had solid internal partition walls rather than pillars and may therefore have been an administrative building rather than an ordinary dwelling, especially in light of its location directly opposite the “West Tower” (see figure 7 above where this structure is marked with a “?”). Shiloh himself counted 18 complete dwelling units in the northwestern sector, but he apparently treated the two small rooms NW 21/15 and 31/1E&W (marked “A” in figure 7) as a separate dwelling, as did Braemer, although there are no pillars here and the extremely small size (ca. 24 m² internal area) of this “house” suggests it was an annex to the adjacent dwelling (NW 31/10).

Furthermore, locus NW 32/12 (marked with a “V” in figure 7) was not a dwelling because it has no pillars or partition walls to support an upper story and its floor was taken up with “two shallow vats of masonry” measuring 1.4–1.6 × 0.8 m (Albright et al. 1943:62f.; also the plan in plate 6 and photographs in plate 49). Albright correctly identified this as an oil-pressing installation, noting the presence of “three high, narrow niches in the west wall of the locus,” about 2 m from the vats, and 18 “large perforated stones” (press-weights), which were “enough to weight two heavy beams.” Note that across the street from locus NW 32/12 is locus NW 32/13 (marked with a “P” in figure 7), which was clearly an outdoor olive-pressing area rather than a dwelling, because it has no pillars and is bounded only by a freestanding L-shaped wall that juts out at right angles from the wall of an adjacent house. An oil-press (a block-cut vat with a circular groove, which Albright identified incorrectly as a dye-vat) was situated 3 m from this L-shaped wall, which must have supported the beam of the press (see Albright et al. 1943:60, plates 6, 43[a]).

If we exclude the doubtful buildings NW 32/12 (“V”) and 32/13 (“P”) and the annex in rooms NW 21/15 and 31/1 (“A”), the total number of houses in the northwestern sector is 16, rather than 18, and may be as low as 15, if the building opposite the West Tower (“?”) was not a dwelling.

In the southeastern sector of Tell Beit Mirsim (see figure 8 above), Shiloh (1980:29; 1978:38) counted 15 complete dwelling units, and this figure appears to be accurate, judging by the published plans (cf. Albright et al. 1943:plates 3–5). Braemer (1982:190–98) describes only 12 houses here, but he ignored some houses (including two houses in SE 14/2–6, 9–12, and one in SE 24/3–5), and he listed separately three structures (SE 42/1, 50/3, 51/2) near the edge of the excavated area which are too small to have been separate dwellings—they should probably be treated as the equivalent of two complete dwellings. Table 5 above, which is based on Shiloh’s and Braemer’s work, together with the original plans, accordingly lists 16 houses in the northwestern sector and the equivalent of 15 houses in the southeastern sector. Shiloh also added 3 dwelling units to account for partly excavated structures, and this too seems reasonable. The best estimate of the number of dwellings excavated in Tell Beit Mirsim is therefore 16 + 15 + 3 = 34, which is slightly less than Shiloh’s 36, but is sufficiently different to affect the calculations.

More important than the exact number of houses, however, is the variation in house size. The minimum roofed living area which was available on the upper floor of each house in Tell Beit Mirsim (i.e., the overall house area, measured inside the walls, less the courtyard area) is shown in table 5 under the column headed “Less court.” In many cases, however, this minimal estimate seems too small (20–30 m²), even for a nuclear family. It is likely, therefore, that the upper floor entirely covered the “courtyard,” as Stager and others have argued (Stager 1985a:15f.; Holladay 1992; cf. Netzer 1992:195ff.), in which case the actual roofed living area was equal to the overall internal area of the house (listed in table 5 under the column headed “Overall”). Alternatively, the upper floor jutted out beyond the pillars, covering part of the courtyard while still permitting a certain amount of light and air to reach the ground floor. The figures shown in parentheses in table 5 after the minimal “Less court” figures represent this inter-

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48 Albright’s statement that the “isolated dye-vat” in NW 32/13 was not in situ stems from his belief that it was used for dyeing and that dyeing vats should occur in pairs (as they do elsewhere on the site) to permit successive treatment of the textiles. But similar semienclosed pressing areas with freestanding walls are known elsewhere, including Late Bronze Age Ugarit. Note, too, that the pair of Tell Beit Mirsim presses in SE 23/10 appear to have been in a semi-enclosed open yard as well (see plate 3), although Albright (p. 57f.) argued (rather unconvincingly) that this open-air “dye-plant” must have gone out of use in the last phase of stratum A because the structure that supposedly housed it had been entirely removed.

49 Note that the space occupied by a house’s internal partition walls is not deducted from the estimate of overall roofed living area, because it is assumed that these walls were much narrower in the upper story than they were in their ground-floor foundation courses (or pillars), being reduced to thin wooden partitions where they existed at all. The assumption here is that the lower story was normally devoted to stables, storage, and cooking or other household activities, whereas the upper story was devoted to human living space (see Holladay 1992).
mediate case and are calculated under the compromise assumption that 50% of the ground-floor courtyard was covered by the upper floor (see also the graph in figure 12).

Table 5 shows there was a minimum of 1,489 m$^2$ of total roofed living space on the upper floors of the 34 houses in Tell Beit Mirsim (stratum A). It is important to note that this is a bare minimum: no allowance is made for extra (human) living space in the ground-floor broadroom found at the rear of most houses, or for the possibility that the floor of the upper story extended past the ground-floor pillars to cover part or all of the central courtyard. Under the assumption that half of the courtyard was covered by the extension of the upper floor, we obtain a somewhat larger total roofed area for all of the houses of 1,793 m$^2$. And if the entire “courtyard” was covered by the upper floor, as Stager and others have suggested, then the roofed living area was equal to the overall internal house area, yielding a total for all of the houses of 2,093 m$^2$.

Assuming a generous 10 m$^2$ of roofed living area per person, there were thus approximately 150–210 persons living in the 34 houses (0.68 ha) that were excavated, depending on the extent to which the courtyards were covered (see table 6). The lower figure holds if the courtyards were entirely uncovered and the higher figure holds if they were entirely covered. These figures are equivalent to 4.4–6.2 persons per house, or a density of approximately 220–310 per ha (264 per ha if courtyards were half-covered). The densities per hectare given in table 6 apply only to a continuously built-up urban area, but they do take into account a certain proportion of nonresidential space, because the 0.68-ha area excavated at Tell Beit Mirsim encompassed some nonresidential structures, including the city wall and “West Tower,” as Shiloh noted. Thus the density figures calculated above are probably applicable to the town as a whole, although the overall density coefficient for the town would have been lower to the extent that the nonresidential proportion of the town was considerably larger than in the area excavated (which is difficult to determine without more extensive excavation of the site). But this is unlikely in light of the fact that no large public structures or open spaces were discovered in the course of extensive excavations at the comparable Iron II site of Tell en-Naṣbeh, even though two-thirds of that settlement were exposed.

In any case, the overall averages of persons per house and persons per hectare mask the variation among individual households. Taking only the case in which courtyards were entirely uncovered, the minimum roofed living area within individual houses ranged from 22–96 m$^2$ (see table 5), with a rather high standard deviation of 17.9 m$^2$; and household size therefore ranged from ca. 2–10 persons—still assuming 10 m$^2$ per person. In my opinion, however, the most probable assumption is that these houses had upper floors which entirely covered the ground floor; thus their roofed living areas ranged from 35–120 m$^2$, corresponding to 3–12 persons at 10 m$^2$ per person. This range encompasses the smallest sizes expected for joint-family households and the largest sizes expected for joint-family households, with a plausible overall mean of 6.2 persons per house and 308 per ha, or 923 people in the entire 3-ha town (assuming that the proportion of nonresidential space was the same throughout the site). A mean of 6.2 persons per house matches reasonably well the mean household size of 6.7 predicted by my earlier estimates (based on Mediterranean demographic models) of 10 persons on average in the one-third of the households which are joint and 5 persons on average in the two-thirds which are nuclear. Indeed, the proportion of houses which were large enough for joint-family households (with 70 m$^2$ or more of roofed living space, assuming 10 m$^2$ per person) is close to the expected one-third (9 out of 31 = 29%), and the proportion of the total population which lived in such households was 45%, which is close to the 50% which is predicted demographically.\(^5\) Also, the density estimate of 308 per ha fits Marcus’s (1989) estimate of 300 per ha in eighteenth-century Aleppo, a densely populated city of two-story houses (remembering that this estimate takes account of nonresidential areas occupied by suqs, mosques, and administrative buildings).

\(^5\) The cut-off for a joint-family household as opposed to a nuclear-family household is here set at 7+ persons, who would require a roofed living area ≥ 70 m$^2$ at 10 m$^2$ per person and ≥ 56 m$^2$ at 8 m$^2$ per person. Recall that comparative Mediterranean census data show that mean nuclear family size is only 3.5 and mean joint family size is 7 persons or fewer, although in predicting household sizes I have increased these figures quite generously to 5 and 10, respectively, to take account of polygamous marriages, more distant kin, and nonfamily household members such as servants or lodgers. For the sake of simplicity, but in line with empirically supported demographic models, the working assumption here is that a household of 7 or more is joint and a household of 6 or fewer is nuclear. In other words, I am positing discrete ranges of ca. 4–6 (mean=5) for nuclear-family households and 7–13+ (mean=10) for joint-family households, noting that very few houses would have accommodated more than 13 persons (≥ 104 m$^2$ at 8 m$^2$).
Table 5. House Areas at Tell Beit Mirsim, Stratum A

<table>
<thead>
<tr>
<th>House</th>
<th>Overall (m x m = m²)</th>
<th>Courtyard (m x m = m²)</th>
<th>Less court (m²)</th>
</tr>
</thead>
<tbody>
<tr>
<td>NW Quadrant</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3/1</td>
<td>10 x 12 = 120</td>
<td>3 x 8 = 24</td>
<td>96 (108)</td>
</tr>
<tr>
<td>11/1</td>
<td>5 x 8 = 40</td>
<td>3 x 6 = 18</td>
<td>22 (31)</td>
</tr>
<tr>
<td>11/2</td>
<td>6 x 8 = 48</td>
<td>3 x 5 = 15</td>
<td>33 (40)</td>
</tr>
<tr>
<td>11/4</td>
<td>5 x 9 = 45</td>
<td>3 x 6 = 18</td>
<td>27 (36)</td>
</tr>
<tr>
<td>12/3</td>
<td>8 x 9 = 72</td>
<td>3 x 7 = 21</td>
<td>51 (62)</td>
</tr>
<tr>
<td>13/9</td>
<td>5 x 9 = 45</td>
<td>3 x 6 = 18</td>
<td>27 (36)</td>
</tr>
<tr>
<td>21/9</td>
<td>8 x 9 = 72</td>
<td>3 x 7 = 21</td>
<td>51 (62)</td>
</tr>
<tr>
<td>21/11</td>
<td>6 x 9 = 54</td>
<td>3 x 7 = 21</td>
<td>33 (44)</td>
</tr>
<tr>
<td>22/5</td>
<td>8 x 9 = 72</td>
<td>3 x 6 = 21</td>
<td>51 (62)</td>
</tr>
<tr>
<td>22/11</td>
<td>4 x 11 = 44</td>
<td>2 x 7 = 14</td>
<td>30 (37)</td>
</tr>
<tr>
<td>31/6</td>
<td>6 x 9 = 54</td>
<td>3 x 7 = 21</td>
<td>33 (44)</td>
</tr>
<tr>
<td>31/8</td>
<td>5 x 10 = 50</td>
<td>2.5 x 7 = 18</td>
<td>32 (41)</td>
</tr>
<tr>
<td>31/10</td>
<td>87</td>
<td>3 x 8 = 24</td>
<td>63 (75)</td>
</tr>
<tr>
<td>32/10</td>
<td>8 x 8 = 64</td>
<td>3 x 7 = 21</td>
<td>43 (54)</td>
</tr>
<tr>
<td>32/4</td>
<td>8 x 8.5 = 68</td>
<td>3 x 6 = 18?</td>
<td>50 (59)</td>
</tr>
<tr>
<td>33/12</td>
<td>77 x 9 = 63</td>
<td>2 x 5? = 10</td>
<td>53 (58)</td>
</tr>
</tbody>
</table>

| Total: | 998 | 303 | 695 (849) |
| Mean:  | 62.4 | 18.9 | 43.4 (53.1) |
| S.d.:  | 20.3 | 3.7 | 18.4 (19.3) |

| SE Quadrant |
| 4/1   | 4 x 10 = 40         | 2 x 7 = 14             | 26 (33)        |
| 13/12 | 7 x 8 = 56          | 2 x 7 = 14             | 42 (49)        |
| 13/16 b | 13/16 b            | 2 x 6 = 12             | 36 (42)        |
| 14/3 c | 7 x 9 = 63          | 2 x 6 = 12             | 51 (57)        |
| 14/6 c | 7 x 8 = 56          | 2 x 4 = 8              | 48 (52)        |
| 22/4  | 7 x 14 = 98         | 3 x 7 = 21             | 77 (88)        |
| 23/5  | 10 x 11 = 110       | 3 x 9 = 27             | 83 (96)        |
| 23/13 | 8 x 9 = 72          | 3 x 6 = 18             | 54 (63)        |
| 24/4 c | 6 x 8 = 48          | 3 x 5 = 15             | 33 (40)        |
| 33/10 | 7 x 9 = 63          | 3 x 7 = 21             | 42 (52)        |
| 33/12 | 8 x 10 = 80         | 3 x 7 = 21             | 59 (70)        |
| 42/1  | 5 x 6 = 35          | 3 x 6 = 18             | 17 (26)        |
| 50/3  | 4 x 7 = 28          | 2 x 5 = 10             | 18 (23)        |
| 51/2  | 6 x 6 = 36          | 2 x 6 = 12             | 24 (30)        |
| 51/5  | 5 x 7 = 35          | 2 x 5 = 10             | 25 (30)        |

| Total: | 910 | 247 | 663 (786) |
| Mean:  | 60.7 | 16.5 | 44.2 (52.4) |
| S.d.:  | 21.4 | 5.2 | 18.0 (19.6) |

Combined NW and SE quadrants (n = 31): | Total: | 1908 | 550 | 1358 (1635) |
| Mean: | 61.5 | 17.7 | 43.8 (52.7) |
| S.d.: | 20.5 | 4.6 | 17.9 (19.1) |

Note: Houses are identified according to the locus number of their main room, following Braemer 1982:182–98; cf. Albright et al. 1943:pls. 3–7. Dimensions are measured inside the walls, but rectangular dimensions are estimated to compensate for the irregularity in house shapes. Where Braemer and Shiloh differ concerning house boundaries, Shiloh’s plans are followed because they are more inclusive (see Shiloh 1970:187; 1978:38).

Italics The three houses in italics (SE 42/1, 50/3, 51/2; total area 99 m²) are listed separately by Braemer but were probably part of larger dwellings, as he acknowledges for SE 42/1 and SE 51/2. They are therefore treated here as the equivalent of two houses (thus n = 15, not 16). In calculating the means and standard deviations for the southeastern sector, the following figures were used for these two reconstructed houses in place of the three sets of italicized figures: (1) overall—50 & 49 m²; (2) courtyard—20 & 20 m²; (3) roofed—30 (40) & 29 (39) m².

a Contrary to Braemer 1982:188f., House NW 31/10 (6 x 10.5 = 63 m²) here includes the adjacent “House” 31/1 (3 x 8 = 24 m²), which has no pillars or courtyard and is too small to be a separate family dwelling.

b Contrary to Braemer 1982:191 and Shiloh 1978:38 (fig. 2), House SE 13/16 here includes the adjacent rooms 13/6 (3 x 6 = 18 m²) and 12/7 (2 x 3 = 6 m²) because it would otherwise be too small (4 x 6 = 24 m²).


d Including 3 partly excavated houses (n = 34): Total: 2093 | 603 | 1489 (1793)

Note: The three houses in italics (SE 42/1, 50/3, 51/2; total area 99 m²) are listed separately by Braemer but were probably part of larger dwellings, as he acknowledges for SE 42/1 and SE 51/2. They are therefore treated here as the equivalent of two houses (thus n = 15, not 16). In calculating the means and standard deviations for the southeastern sector, the following figures were used for these two reconstructed houses in place of the three sets of italicized figures: (1) overall—50 & 49 m²; (2) courtyard—20 & 20 m²; (3) roofed—30 (40) & 29 (39) m².

a Contrary to Braemer 1982:188f., House NW 31/10 (6 x 10.5 = 63 m²) here includes the adjacent “House” 31/1 (3 x 8 = 24 m²), which has no pillars or courtyard and is too small to be a separate family dwelling.

b Contrary to Braemer 1982:191 and Shiloh 1978:38 (fig. 2), House SE 13/16 here includes the adjacent rooms 13/6 (3 x 6 = 18 m²) and 12/7 (2 x 3 = 6 m²) because it would otherwise be too small (4 x 6 = 24 m²).


d Partly excavated houses are not analyzed individually but are treated as the equivalent of three “average” houses (i.e., the additional area = mean house area x 3). Note that the means and standard deviations are unchanged.
Figure 12. Second-story roofed areas at Tell Beit Mirsim A based on percentage of courtyard covered

(* SE 51/2, 42/1, 50/3 are part of larger dwellings and constitute the equivalent of two 49–50 m² houses; see table 5.)
If it were argued that not all of the courtyards in Tell Beit Mirsim would have been completely roofed, so that we must lower the maximal estimates of roofed living area (with courtyards fully covered) which yielded 6.2 persons per house and 308 per hectare, it is equally possible to argue that Naroll’s widely cited figure of 10 m$^2$ of roofed living space per person is too high for Tell Beit Mirsim. We can therefore plausibly reduce both the overall living space estimate and the space per person estimate and arrive at roughly the same result. Naroll’s estimate was based on ethnographic data from eighteen nonurban “prehistoric” societies, but living space in an Iron II walled town was undoubtedly more restricted than in an unwalled village. Moreover, Naroll’s coefficient does not take account of differences in family- or dwelling-type. Samuel Casselberry (1974:119) has therefore argued that Naroll’s average is overgeneralized, and he presents New World ethnographic data which show that in “multifamily” dwellings, in particular, the number of inhabitants approximates “one-sixth the floor area of the dwelling as measured in square metres”; that is, 6 m$^2$ per person (see also the discussion of population estimation in Renfrew and Bahn 1991:399f.). Charles Kolb (1985:590) reports the same result in a much more detailed study in which he reviews previous attempts at demographic estimation in archaeology and defends a figure of 6.12 m$^2$ per person, based on ethnographic data from Mesoamerican peasant households. A figure of 5–6 m$^2$ per person in ancient Near Eastern urban settings is also supported by Elizabeth Stone’s (1981:32) analysis of the textual and archaeological data from Old Babylonian Nippur, which produced an estimate of 5.3 m$^2$ per person.

But many will think that an estimate of 6 m$^2$ per person is too low as an overall measure since it is based on rural peasant households or nonelite urban households and thus fails to take into account the fact that more spacious living quarters were probably enjoyed by the wealthiest segment of the population—compare Watson’s (1979:291) average of 7.3 m$^2$ of roofed dwelling space per person, with a large standard deviation of 4.1 m$^2$, derived from her ethnoarchaeological work in Iran. So let us increase our lower limit generously to 8 m$^2$ per person, in order not to bias the results in favor of what I am arguing for here, namely, that a typical Israelite joint family (bêt ‘āb) could have occupied a single dwelling. Our upper limit will continue to be Naroll’s widely used figure of 10 m$^2$. If we assume 8 m$^2$ of roofed living space per person in Tell Beit Mirsim, there were between 186 and 261 persons living in the 34 excavated houses, which works out to an average of 5.5–7.7 persons per house, or 274–385 per hectare (see table 6). Taking as a compromise the middle estimate of upper-floor living space (50% of courtyard covered), we obtain a plausible average of 330 per hectare and 6.6 per house, with individual household sizes ranging from ca. 4–13 persons (i.e., 31–108 m$^2$ at 8 m$^2$ per person).

More importantly, the proportion of houses which were large enough for joint-family households is 39% (12 out of 31)—again, close to the one-third predicted by the demographic models discussed above in chap-

### Table 6. Population Density and Household Size at Tell Beit Mirsim, Stratum A

<table>
<thead>
<tr>
<th>Percentage of “courtyard” covered:</th>
<th>10 m$^2$</th>
<th>8 m$^2$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Total second-story roofed area (m$^2$)</td>
<td>1489</td>
<td>1489</td>
</tr>
<tr>
<td>Population in 34 houses = 0.68 ha</td>
<td>149</td>
<td>186</td>
</tr>
<tr>
<td>Persons per house</td>
<td>4.4</td>
<td>5.5</td>
</tr>
<tr>
<td>Persons per hectare</td>
<td>219</td>
<td>274</td>
</tr>
<tr>
<td>$a$ Joint-family households (≥7 persons)</td>
<td>3</td>
<td>5</td>
</tr>
<tr>
<td>Joint-family households (%)</td>
<td>10</td>
<td>16</td>
</tr>
<tr>
<td>Joint-family population (%)</td>
<td>18</td>
<td>28</td>
</tr>
<tr>
<td>Population in entire 3-ha site</td>
<td>657</td>
<td>821</td>
</tr>
</tbody>
</table>

$a$ Only the 31 complete houses listed in table 5 are considered here; the 3 houses that were added to account for partly excavated dwellings do not enter into the calculation of the proportion of joint-family households. Note that a “joint-family household” is defined as having ≥7 members (10 on average), in a house whose second-story roofed living area is ≥70 m$^2$ at 10 m$^2$ per person or ≥56 m$^2$ at 8 m$^2$ per person.

$b$ The percentage of the population living in joint-family households is calculated under the assumption that the mean size of joint-family households is 10 and the mean size of all other (nuclear-family) households is 5.

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**Mediterranean Houses and Households**
The findings from Tell Beit Mirsim are supported by data from Iron II houses excavated at a similar site, Tell en-Naṣbeh, which is probably to be identified as biblical Mizpah in the territory of Benjamin (McCown et al. 1947). The plans of these houses have been analyzed in some detail by Thomas McClellan (1984) and Jeffrey Zorn (1994). Zorn identified 23 buildings as having “plans sufficiently clear to determine both total area for each structure (including walls) and actual floor areas” (pp. 36, 39 [table 3]). Of these, he regards 19 as dwellings and four as nonresidential buildings with specialized functions (e.g., olive pressing). The usable sample from Tell en-Naṣbeh is larger than this, however. McClellan (1984:fig. 13) identified 41 separate buildings, although 11 of these, mostly in the northwestern part of the tell (nos. 341, 361, 360, 354, 345, 344, 346, 664, 662, 656, 625), were not excavated sufficiently or are too poorly preserved to determine their full extent and so are excluded from consideration here. Similarly, four of the houses used by Zorn in his sample, but not by McClellan, are too poorly preserved to be used here (Zorn’s nos. 142.07 [rooms 630, 648, 658]; 142.08 [rooms 649, 650]; 142.10 [rooms 621, 660, 666]; and 142.11 [rooms 613, 614, 618]). All of these fragmentary houses appear to be of typical size, though, so excluding them does not bias the results substantially.

One wedge-shaped structure (no. 607 = Zorn’s no. 142.02) was clearly not a dwelling but a specialized oil-pressing establishment, as was noted by both McClellan and Zorn. This building (= rooms 607, 609) is comparable to locus NW 32/12 at Tell Beit Mirsim, which I excluded from consideration above because it was a special-purpose oil-pressing establishment and not a dwelling. In both cases, there is a large room with oil-making equipment and no pillars.
or partition walls to support a second story. Still another building (no. 445) is identified by McClellan as a “workshop” rather than a dwelling. This leaves 28 distinct dwellings whose basic dimensions can be determined, even though some of them are badly preserved or are otherwise confusing (see figure 13 below, where I have followed McClellan’s stratigraphic analysis and the house boundaries shown in his figures 4, 7, and 9). In table 7 below I have listed these 28 houses, specifying which rooms belong to each dwelling. For each one I have given the maximum possible second-story roofed area (the “Overall” area based on average rectangular dimensions measured inside the exterior walls), the courtyard area, and the overall area less the courtyard area, showing in parentheses the roofed living area available if only 50% of the courtyard was covered (see also the graph in figure 14). These areas are analogous to those calculated for Tell Beit Mirsim in table 5 above.

Note that in his detailed analysis of Iron II house sizes at Tell en-Naṣbeh, Zorn does not calculate courtyard areas or the range of possible roofed living space on upper floors, based on the proportion of the courtyard which was covered, because he treats the ground floor as the main living space; thus his “Floor Area” calculations pertain only to ground-floor rooms (including the courtyard) and they also exclude the internal ground-floor partition walls (p. 39, table 3). But as I have noted above, the space occupied by internal walls is negligible in calculating roofed living area since we can assume that such walls were much narrower in the upper story than they were in their ground-floor foundation courses (or pillars), being reduced to thin wooden partitions where they existed at all. Again, as Holladay, Stager, and others have argued, it is highly likely that the ground floor was largely devoted to stables, storage, and domestic work, while the upper floor was used for human living space. As a result, Zorn’s lower-story “Floor Area” underestimates the upper-story roofed living space. Insofar as I have overestimated the upper-story area by ignoring internal partition walls, this is compensated for by the possibility that additional ground-floor living space was used in certain cases (e.g., in the rear broadroom, where there is one).

There is some question as to whether several large “four-room” style building units on the east side of the tell, which are not included in table 7, should be dated to the Iron II period. Zorn (1994:36) attributes them to the Babylonian and early Persian periods, but Ze’ev Herzog (1997:237) disputes this, defending the more common view that these buildings were erected in the second major phase of Iron II construction when the town was enlarged by relocating the city wall, being thus converted from a 1.9-hectare walled settlement into a more strongly fortified 3.2-hectare military and administrative center (in which a large part of the urban area was taken up by an outer ring of fortifications and storage facilities). This enlargement probably occurred fairly early in the Iron II period, during the ninth century B.C., when the division of the “United Monarchy” of David and Solomon turned Mizpah into a northern border town of the kingdom of Judah (cf. the account in 1 Kings 15:22 that King Asa of Judah fortified Mizpah). The houses added at that time, especially on the east side of the tell near the city gate, were unusually large and well built, suggesting a specialized administrative function—but on behalf of the king of Judah, not a later Babylonian or Persian governor, according to this interpretation. In any case, like Zorn, I have restricted my sample to the Iron II houses excavated in a more-or-less contiguous block of architecture on the southwest side of the tell, recognizing that the few additional houses near the city gate on the east side that might also have been considered would not have housed typical domestic groups, whatever their date, because they presumably belonged to the city’s governor and other high-ranking officials.
Figure 13. Tell en-Naṣbeh, first Iron II phase (after McCown et al. 1947 [survey map] and McClellan 1984)
Table 7. House Areas at Tell en-Naṣbeh, first Iron II phase

<table>
<thead>
<tr>
<th>House No. (rooms, including courtyard rooms) (n = 28)</th>
<th>Overall (m²)</th>
<th>Courtyard (m²)</th>
<th>Less court (m²)</th>
</tr>
</thead>
<tbody>
<tr>
<td>51 (51, 61, 73, 74, 76, 78, 79, 80, 81)</td>
<td>9×10.5 ± 94</td>
<td>3×9 = 27</td>
<td>67 (80)</td>
</tr>
<tr>
<td>60 (50, 52, 59, 60, 65, 72)</td>
<td>8×10 = 80</td>
<td>3×7 = 21</td>
<td>59 (70)</td>
</tr>
<tr>
<td>64 (42, 44, 58, 63, 64, 66, 67)</td>
<td>7×10.5 ± 74</td>
<td>3×7 = 21</td>
<td>53 (64)</td>
</tr>
<tr>
<td>386 (385, 386, 387, 392)</td>
<td>6×10 = 60</td>
<td>2×7 = 14</td>
<td>46 (53)</td>
</tr>
<tr>
<td>390 (389, 390, 391)</td>
<td>5×12 ± 60</td>
<td>2.5×9.5 ± 24</td>
<td>36 (48)</td>
</tr>
<tr>
<td>393 (393, 395, 396, 397)</td>
<td>7×11 = 77</td>
<td>2×9 = 18</td>
<td>59 (68)</td>
</tr>
<tr>
<td>433 (427, 429, 430, 432, 433)</td>
<td>6×11 = 66</td>
<td>2×7 = 14</td>
<td>52 (59)</td>
</tr>
<tr>
<td>434 (404, 409–10, 412–13, 434, 435, 506, 511–12, 515)</td>
<td>10×13 = 130</td>
<td>4.5×8 ± 36</td>
<td>94 (112)</td>
</tr>
<tr>
<td>437 (415, 431, 437, 442)</td>
<td>5×13 = 65</td>
<td>2.5×8 ± 20</td>
<td>45 (55)</td>
</tr>
<tr>
<td>440 (440, 463, 464, 468, 520)</td>
<td>6×9 = 54</td>
<td>1.5×5 ± 8</td>
<td>46 (50)</td>
</tr>
<tr>
<td>441 (87, 417, 418, 438, 441a, 443) a</td>
<td>6×11.5 ± 69</td>
<td>2×7 = 14</td>
<td>55 (62)</td>
</tr>
<tr>
<td>446 (444, 446, 448, 449, 450, 451, 455)</td>
<td>6.5×10.5 ± 68</td>
<td>1.5×7 ± 10</td>
<td>58 (63)</td>
</tr>
<tr>
<td>519 (518, 519, 529, 569) b</td>
<td>5×9 = 45</td>
<td>2×5.5 ± 11</td>
<td>34 (40)</td>
</tr>
<tr>
<td>536 (530, 531, 536, 566a)</td>
<td>6×8 = 48</td>
<td>2.5×6 ± 15</td>
<td>33 (40)</td>
</tr>
<tr>
<td>537 (537, 538, 566b)</td>
<td>6.5×7.5 ± 49</td>
<td>2×6 = 12</td>
<td>37 (43)</td>
</tr>
<tr>
<td>576 (576, 577, 578, 579)</td>
<td>5×6.5 ± 32</td>
<td>2.5×4.5 ± 11</td>
<td>21 (26)</td>
</tr>
<tr>
<td>580 (580, 584, 612)</td>
<td>5×7.5 ± 38</td>
<td>2×5 = 10</td>
<td>28 (33)</td>
</tr>
<tr>
<td>581 (513, 575, 581)</td>
<td>4×6 = 24</td>
<td>2×4 ± 8</td>
<td>16 (20)</td>
</tr>
<tr>
<td>583 (497–8, 503–5, 508, 509, 510, 581, 583, 585, 587)</td>
<td>7×14 = 98</td>
<td>2.5×10 ± 25</td>
<td>73 (86)</td>
</tr>
<tr>
<td>588 (588, 606, 608, 610) c</td>
<td>5×8.5 ± 42</td>
<td>2.5×6 ± 15</td>
<td>27 (34)</td>
</tr>
<tr>
<td>590 (500, 586, 590, 595, 596)</td>
<td>4.5×12 ± 54</td>
<td>2.5×9 ± 22</td>
<td>32 (43)</td>
</tr>
<tr>
<td>594 (527, 592, 593, 594)</td>
<td>46 d</td>
<td>2×6 = 12</td>
<td>34 (40)</td>
</tr>
<tr>
<td>599 (526, 534, 535, 539, 540, 597, 598, 599, 601, 604)</td>
<td>8×12 = 96</td>
<td>2×7 = 14</td>
<td>82 (89)</td>
</tr>
<tr>
<td>605 (605, 617)</td>
<td>5×5.5 ± 28</td>
<td>2.5×5.5 ± 14</td>
<td>14 (21)</td>
</tr>
<tr>
<td>616 (616, 619, 622, 623)</td>
<td>4.5×8 ± 36</td>
<td>2.5×6 ± 15</td>
<td>21 (28)</td>
</tr>
<tr>
<td>628 (615, 620, 624, 626, 628, 629)</td>
<td>6×8 = 48</td>
<td>1.5×6 = 9</td>
<td>39 (44)</td>
</tr>
<tr>
<td>640 (640, 645, 646)</td>
<td>5×7 = 35</td>
<td>2.5×5.5 ± 14</td>
<td>21 (28)</td>
</tr>
<tr>
<td>642 (642, 651)</td>
<td>5.5×7 ± 38</td>
<td>3×5 ± 15</td>
<td>23 (30)</td>
</tr>
<tr>
<td>Total:</td>
<td>1654</td>
<td>449</td>
<td>1205 (1429)</td>
</tr>
<tr>
<td>Mean:</td>
<td>59.1</td>
<td>16.0</td>
<td>43.0 (51.0)</td>
</tr>
<tr>
<td>Standard deviation:</td>
<td>24.6</td>
<td>6.5</td>
<td>20.2 (22.4)</td>
</tr>
</tbody>
</table>

*Note:* The house numbers in this table are those used by McClellan (1984:fig. 13), based on the room numbers assigned by the original excavators in the “Survey Map” in McCown et al. 1947. Usually the number of the main “courtyard” room is chosen to identify the entire house (courtyard rooms are shown in bold italics).

- a House 441 does not include rooms 82, 83, and 84, which are probably an alley between House 441 and House 51.
- b The central courtyard in House 519 is not numbered in the site plan.
- c House 588 has an oil-press in the rear corner of its courtyard, as Zorn (1994:39) points out, but it otherwise appears to be an ordinary dwelling and thus is included here.
- d House 594 is irregularly shaped and has been measured using two rectangles with dimensions 4×9 + 2×5 = 46 m².
Figure 14. Second-story roofed areas at Tell en-Našbeh based on percentage of courtyard covered
The maximum “Overall” upper-story roofed living areas of these 28 dwellings at Tell en-Naṣbeh have a mean of 59.1 m² and a large standard deviation of 24.6 m², which are similar to the same statistics at Tell Beit Mirsim (61.5 and 20.5 m²). Again, demographically plausible estimates are produced under the assumptions that 100% of the courtyard was covered and each person had 10 m² of roofed living space, or, alternatively, that 50–100% of the courtyard was covered, on average, and each person had 8 m² of roofed living space (see table 8). The estimated household sizes are somewhat smaller than at Tell Beit Mirsim and the densities per hectare are greater, because of the slightly smaller average size of the houses and the greater density of residential structures in the sample area at Tell en-Naṣbeh (cf. Zorn 1994:37). Household sizes and densities per hectare are, however, within the expected ranges derived from comparative Mediterranean evidence.

Furthermore, the proportion of joint-family households and the proportion of the population living in such households are close to what we would expect under a system of patrilocal postmarriage coresidence.

The picture of urban domestic architecture presented by Tell Beit Mirsim and Tell en-Naṣbeh is confirmed by the data from Tell el-Fār’ah (North), strata VIIb and VIIId (see tables 9 and 10 below and figures 10 and 11 above). At that site—probably biblical Tirzah, at one time the capital of the northern kingdom of Israel—the houses were substantially larger than in Tell Beit Mirsim (mean overall area ca. 77 m² versus 62 m²); thus the roofed areas and corresponding population estimates are larger (6.6–9.6 per house, assuming 8 m² per person). Perhaps living conditions were less crowded, in which case a higher estimate of roofed area per person is appropriate. Note also that the sample is considerably smaller (13 houses in VIIb and 5 houses in VIIId), and might be biased toward larger elite houses situated near the city gate. In any case, the general pattern exhibited at Tell Beit Mirsim and Tell en-Naṣbeh is duplicated at Tell el-Fār’ah (North). Even though several of the houses excavated at the latter site were unusually large, only 31% of the stratum VIIb dwellings (4 out of 13) were large enough to house joint-family households of 7 or more, assuming a fairly dense occupation of 8 m² per person and a minimum of

---

**Table 8. Population Density and Household Size at Tell en-Naṣbeh, first Iron II phase**

<table>
<thead>
<tr>
<th>Roofed area per person:</th>
<th>$10^2$</th>
<th>$8^2$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Percentage of “courtyard” covered:</td>
<td>0%</td>
<td>50%</td>
</tr>
<tr>
<td>Total second-story roofed area (m²)</td>
<td>1205</td>
<td>1429</td>
</tr>
<tr>
<td>a Population in 28 houses = 0.4 ha</td>
<td>120</td>
<td>143</td>
</tr>
<tr>
<td>Persons per house</td>
<td>4.3</td>
<td>5.1</td>
</tr>
<tr>
<td>Persons per hectare</td>
<td>301</td>
<td>357</td>
</tr>
<tr>
<td>Joint-family households (≥ 7 persons)</td>
<td>3</td>
<td>5</td>
</tr>
<tr>
<td>Joint-family households (%)</td>
<td>11</td>
<td>18</td>
</tr>
<tr>
<td>b Joint-family population (%)</td>
<td>19</td>
<td>30</td>
</tr>
<tr>
<td>c Population in entire 1.9-ha site</td>
<td>572</td>
<td>679</td>
</tr>
</tbody>
</table>

---

a The area occupied by the 28 houses in the sample, including adjacent streets, nonresidential buildings, and the city wall (i.e., the city wall of the first Iron II phase), but excluding badly preserved dwellings which are not in the sample, is here estimated to be ca. 4,000 m² or 0.4 ha, as measured from the site plan.

b As in table 6, a “joint-family household” is defined as having ≥ 7 members, in a house whose second-story roofed living area is ≥ 70 m² at 10 m² per person or ≥ 56 m² at 8 m² per person, and the percentage of the population living in joint-family households is calculated under the assumption that the mean size of joint-family households is 10 and the mean size of all other (nuclear-family) households is 5.

c In its latest Iron II phase Tell en-Naṣbeh was 3.2 ha in area, but in its earlier Iron II phase (10th century?) it was only about 1.9 ha, fortified by a casemate wall into which the outer ring of houses was built, as at Tell Beit Mirsim—see the plan in Herzog 1997:238, fig. 5.26, although Herzog’s estimate of 1.5 ha seems too low, judging by the published site plan, as does Zorn’s (1994:36) estimate of 1.7 ha. Even with the later expansion of the city, the total population probably did not increase much (although the site-wide density coefficient would have decreased substantially) because much of the additional area on the periphery of the town was taken up by the massive new offset-inset wall and specialized administrative or storage structures.
upper-story roofed living area, or assuming 10 m² per person but allowing for the middle estimate of roofed living area.

In all three sites we find proportions of house sizes of the magnitude expected when patrilocal coresidence is the ideal but high mortality rates shorten the three-generation joint-family phase of the household lifecycle. The smaller houses would have been occupied by families in the nuclear phase of the household lifecycle, or in some cases by landless families which could not maintain patrilocal coresidence and attached themselves to wealthier households as clients or servants. Over time, of course, many of these smaller families in turn would have become “joint,” and the irregular architectural pattern of sites like Tell Beit Mirsim, Tell el-Fâr’ah (North), and Tell en-Naṣbeḥ presumably reflects the constant rearrangement of living space—clearly demonstrated in the architectural changes observed in successive Iron II strata at Tell el-Fâr’ah (North)—that was necessary to accommodate the changing sizes and compositions of interrelated households that made up these towns.

It might be objected that the variation in house size in Iron II Israelite towns was caused by differences in wealth rather than family size. Indeed, archaeologists working in the Near East have traditionally identified larger houses as those of the “rich” and smaller houses as those of the “poor.” But wealth is highly correlated to family size under premodern conditions, because richer householders can afford polygamous marriages or more numerous servants. Moreover, as Wilk and Rathje (1982) point out, land is the most important form of wealth in agrarian societies, and scarcity of land in turn fosters joint-family households, impartible inheritance, and a diversity of household sizes, because certain potentially large but land-poor households must effectively disband, distributing their members as servants or clients among wealthier (often kin-related) households (see the discussion of this phenomenon in chapter 7.1 above).

Table 9. House Areas at Tell el-Fâr’ah (North)

<table>
<thead>
<tr>
<th>Stratum VIIb</th>
<th>Stratum VIIId</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>House</strong></td>
<td><strong>Overall</strong></td>
</tr>
<tr>
<td><em>(n = 13)</em></td>
<td><em>(m²)</em></td>
</tr>
<tr>
<td>149b a</td>
<td>68</td>
</tr>
<tr>
<td>161</td>
<td>95</td>
</tr>
<tr>
<td>163 a</td>
<td>61</td>
</tr>
<tr>
<td>176</td>
<td>61</td>
</tr>
<tr>
<td>180</td>
<td>53</td>
</tr>
<tr>
<td>187 a</td>
<td>77</td>
</tr>
<tr>
<td>355</td>
<td>121</td>
</tr>
<tr>
<td>384</td>
<td>70</td>
</tr>
<tr>
<td>410a</td>
<td>82</td>
</tr>
<tr>
<td>425 c</td>
<td>7×7.5 = 52</td>
</tr>
<tr>
<td>436</td>
<td>87</td>
</tr>
<tr>
<td>440</td>
<td>81</td>
</tr>
<tr>
<td>442</td>
<td>90</td>
</tr>
</tbody>
</table>

| **Total:**   | 998          | 316           | 682 (839)      | **Total:**  | 382          | 123          | 259 (321)     |
| **Mean:**    | 76.8         | 24.3          | 52.5 (64.5)    | **Mean:**   | 76.4         | 24.6         | 51.8 (64.2)   |
| **S.d.:**    | 19.2         | 7.8           | 13.4 (16.2)    | **S.d.:**   | 19.7         | 6.9          | 13.0 (16.7)   |

**Note:** The “Overall” and “Courtyard” areas are taken from Chambon 1984:32 (table 1) and 44 (table 2), which give surface areas measured inside the exterior walls of each house.

* a Chambon does not give courtyard areas for Houses 149b, 163, and 187, so these areas have been measured from the plan, assuming the maximum possible courtyard space.

* b The courtyard area for House 180 is difficult to determine from the plan, so this area is estimated based on the fact that courtyards occupy an average of 32% of the total house area in the other houses in stratum VIIb.

* c House 425 is not included in Chambon’s table 1 (p. 32), but plan 3 shows that loci 423, 425, and 427 constitute a separate dwelling.
Figure 15. Second-story roofed areas at Tell el-Fârîah (North) VIIb based on percentage of courtyard covered

Table 10. Population Density and Household Size at Tell el-Fârîah (North), Stratum VIIb only

<table>
<thead>
<tr>
<th>Roofed area per person:</th>
<th>10 m²</th>
<th>8 m²</th>
</tr>
</thead>
<tbody>
<tr>
<td>Percentage of “courtyard” covered:</td>
<td>0%</td>
<td>50%</td>
</tr>
</tbody>
</table>

Total second-story roofed area (m²)

| 682 | 839 | 998 | 682 | 839 | 998 |

a Population in 13 houses = 0.3 ha

| 68 | 84 | 100 | 85 | 105 | 125 |

Persons per house

| 5.2 | 6.5 | 7.7 | 6.6 | 8.1 | 9.6 |

Persons per hectare

| 227 | 280 | 333 | 284 | 350 | 416 |

Joint-family households (≥ 7 persons)

| 1 | 4 | 8 | 4 | 9 | 11 |

Joint-family households (%)

| 8 | 31 | 62 | 31 | 69 | 85 |

Joint-family population (%)

| 14 | 47 | 76 | 47 | 82 | 92 |

Population in entire 6-ha site

| 1364 | 1678 | 1996 | 1705 | 2098 | 2495 |

a The total area excavated in stratum VIIb (excluding the parts of Area II disturbed by other strata and adjacent partly excavated houses not included in the sample) is ca. 3,000 m² or 0.3 ha, according to Chambon 1984:plan 3.

b As in tables 6 and 8, a “joint-family household” is defined as having ≥ 7 members, in a house whose second-story roofed living area is ≥ 70 m² at 10 m² per person or ≥ 56 m² at 8 m² per person, and the percentage of the population living in joint-family households is calculated under the assumption that the mean size of joint-family households is 10 and the mean size of all other (nuclear-family) households is 5.
The situation of land scarcity did not change significantly after the first "pioneering" phase of Israelite settlement in the highlands of Palestine during the twelfth and eleventh centuries B.C. It is not surprising, therefore, that the combined influence of this continuing external constraint and the deeply rooted patrilocal cultural ideal with which it interacted served to perpetuate the joint-family household structure until the end of the Iron Age and beyond, as both the archaeological and the biblical data suggest. There is no evidence that the emergence of a more centralized political regime during the tenth century B.C. significantly affected household size or structure.

If anything, the average size of Israelite dwellings, and their corresponding capacity for housing joint families as coresident domestic groups, increased during the Iron II period. Similarly, there is no archaeological or textual evidence of any diminution in the importance of the joint-family household or ʾbēt ḫāb as the basic social and economic unit of Israelite society—the basic institution by means of which land, labor, and other resources were organized for production, consumption, and transmission from one generation to the next.

These observations call into question the sociological interpretations of legal, prophetic, and historical biblical texts advanced by earlier generations of biblical scholars, as they pertain to the relations between rich and poor, the degree of "bureaucratic" economic and political specialization found in Iron Age Israel, and the degree of breakdown of traditional forms of social organization during the pre-Assyrian period. From the perspective of demography and domestic space, there is no support for interpretations which posit a radical qualitative shift in Israelite social, political, or religious traditions prior to the catastrophic Assyrian and Babylonian conquests and deportations late in the Iron Age—after which, indeed, the Israelite ʾmispāḥāt, if not the ʾbēt ḫāb, suffered a radical transformation as both fact and symbol. The investigation of that transformation, using both textual and archaeological sources, will be reserved for my discussion of "Tradition and Rationalization in the Axial Age," which is forthcoming in a second volume on The House of the Father as Fact and Symbol.

This concludes my discussion of the main issues provoked by the "Interpretation of Ancient Social Action," which have occupied Part One of this book. Much more could be said on this topic, but at this point we are equipped with enough comparative Mediterranean data and a sufficiently well articulated theoretical background to study some of the key social phenomena of the Bronze Age Levant, and of the kingdom of Ugarit in particular. My approach in Part Two will be "interpretive" or hermeneutical, in the sense that archaeological and textual data will be interpreted in terms of what we know of the linguistically mediated symbolic world of the West Semitic-speaking Canaanites (or "Amorites") and their Bronze Age Near Eastern neighbors. The symbolism of social relations, whether in international diplomacy, economic exchange, or religious cult and mythology, will not be dismissed as epiphenomenal or euphemistic. There is no underlying layer of "real" social life deeper than what is expressed in language by the social actors themselves, because any social order consists of inherited types of traditional social behavior which are symbolically mediated and constrained. I have therefore rejected materialist and functionalist models of social causation of a kind that is still common in Near Eastern archaeology; but I have not done this simply a priori, on theoretical grounds, for I hope to show that the available evidence can be more persuasively explained in terms of nonfunctionalist motivational models, in which typically subjectively meaningful motivations are seen to operate in the context of particular material circumstances.

In any case, it is clear that Near Eastern textual and archaeological evidence is always interpreted in light of some larger model of ancient social interaction, functionalist or nonfunctionalist, whether this is acknowledged or not. Thus arguments about the meanings of particular socially significant words, in Ugaritic or any other ancient Near Eastern language, and about the meaning of particular house plans and domestic assemblages, will be most convincing if they are not offered piecemeal, in a disconnected fashion, but can be seen to fit into a broader framework of mutually reinforcing interpretations which can account for all the data. And accounting for all the data requires, in my view, close attention, within a single conceptual framework, to all aspects of ancient social experience—economic, political, and religious—pects which are too often prematurely isolated for separate consideration by balkanized groups of specialists.
PART TWO

THE PATRIMONIAL HOUSEHOLD IN UGARIT
AND THE BRONZE AGE NEAR EAST
IV. MODELS AND EVIDENCE

Chapter 9. Models of Bronze Age Near Eastern Society

There are two principal models that scholars have adopted to explain the evidence from Late Bronze Age Ugarit, a small kingdom on the Mediterranean coast of Syria, which forms the primary case study in our consideration of the role of the “house of the father” in the Bronze Age Near East. For the sake of convenience, I will call the first of these two models the “feudal” model, and the second the “two-sector” model. Historians of the ancient Near East had already made extensive use of both of these models before they were applied to Ugarit. Until the 1960s, concepts and terminology derived from medieval European feudalism were pressed into service to describe, for example, Old Babylonian society under Hammurapi, Kassite Babylonia, Middle Assyrian society, Nuzi and the Hurrian kingdom of Arrapha, the Hittite empire, and Middle and Late Bronze Age Canaan.1 Erich Ebeling’s (1957) article on “Feudalismus” in the Realelexikon der Assyriologie well represents the view, widely held during the first half of the twentieth century, that: “Diese Gesellschaftsform findet man im Alten Orient besonders in Staaten, bei denen eine dünne Herrenschaft, die aus der Fremde zugewandert ist, festgestellt werden kann” (p. 54). The feudal analogy has been criticized in recent years, but for a long time many scholars favored some form of the feudal model as a description of what appear to have been relatively decentralized ancient Near Eastern regimes, while more centralized governments were pictured according to the bureaucratic model of “state socialism” or the “temple economy,” for example in third-millennium southern Mesopotamia.

At first glance, the feudal model and the bureaucratic “state socialism” model appear to be quite different, but they diverge chiefly with respect to the degree of administrative centralization that is thought to have existed in the various societies they describe.2 More fundamentally, they are alike in excluding a significant role for private land ownership—a situation that many scholars hitherto believed to be characteristic of all ancient Near Eastern states. In this respect, at least, both the feudal and the bureaucratic interpretations that were proposed by non-Marxist scholars resemble the classical Marxist model of an ancient “slave society” populated by nonfree producers who do not own the means of production, a description that Marxist writers have applied not only to the Hellenistic and Roman empires but to earlier Asiatic societies as well. (Note that in Marxist theory, feudal dependents do own the means of production and are therefore not “slaves,” in the Marxist sense. Here, however, I am comparing what non-Marxist scholars understand by “feudalism,” i.e., that all land is owned by the ruler, with the Marxist idea of a “slave society.”)

Since the 1950s, however, the Russian scholar Igor Diakonoff—reacting to “orthodox” Marxist analysis, on the one hand, with its monotonous stress on slavery, and to Western scholarship on the other, with its fondness for feudal interpretations—has developed a broad “two-sector” model as a general description of

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1 For statements concerning feudalism (or the lack of it) in various ancient Near Eastern states, see the following. Old Babylonian: The feudal model is assumed by Meek (1946:66 n.19); note that the feudal analogy led Meek to translate avilum as “seignior” and ikum as “feudal obligations” in the laws of Hammurapi (ANET, pp. 166ff.). Kassite: Balkan (1986; orig. Turkish 1943) strongly supports the feudal model; by contrast Brinkman (1974), in a more recent study of the government of Kassite Babylonia, eschews feudal terminology, holding the question of feudalism in abeyance until more evidence is available (p. 408), while Sommerfeld (1995a; 1995b) notes certain similarities to feudal phenomena in Kassite Babylonia but rejects the use of the term as an overall description of Middle Babylonian society. Middle Assyrian: Garelli (1967) discusses and criticizes the notion of Middle Assyrian feudalism. Nuzi: The feudal model is defended by Koschaker (1928; 1944) and especially by Lewy (1942). Hittite: Goetze (1964) supports the feudal model; see Haase’s (1983) review of the literature and of Goetze’s arguments, in particular. Diakonoff (1967) was one of the first to reject the feudal model of Hittite society in favor of his “two-sector” approach (discussed below), in which he is followed by Archi (1973). Canaan: Alt (1959b) and Gray (1952a; 1952b), among others, employ the feudal model (their work is discussed in more detail below).

2 The classic statement of the “temple economy” hypothesis is that of Anton Deimel (1931); see also Falkenstein (1974 [1954]). Diakonoff attacked this approach in a volume published in Russian in 1959 (English summary in Diakonoff 1974). Similar criticisms were voiced by Gelb (1971), who also objected to the kindred notion of a “state economy” or “state socialism” in the Ur III period.
Asiatic Near Eastern society from the beginnings of urban civilization until the end of the second millennium B.C. According to Diakonoff, not all producers were propertyless slaves or dependents, nor were they feudal serfs; rather, there was a substantial group of private landowners forming a kin-based “free” sector of society outside the great royal or temple households of the bureaucratic “state” sector. He argues that these two sectors existed not just in third-millennium Mesopotamia but also in the Old Babylonian empire, the Middle Assyrian kingdom, Hittite Anatolia, and Bronze Age Syria and Palestine—and probably in the rest of the ancient Orient as well, including China. Obviously, there were differences among societies so widely separated in time and space, but Diakonoff contends that the basic two-sector structure was a feature common to all of them. His approach has won a number of adherents, including the two leading writers of recent years on the society and economy of Ugarit, namely, Diakonoff’s former student Michael Heltzer and the Italian historian Mario Liverani. Their two-sector model of Ugaritian society has in turn become the standard view of the social and economic organization of the kingdom of Ugarit accepted by many scholars (see, e.g., van Soldt 1995).

1. Feudalism as a Comparative Concept

Although Diakonoff’s two-sector model is prominent today, earlier scholarship on Ugarit was dominated by the feudal model. This can be seen, for example, in works published in the 1950s and 1960s by Albrecht Alt, John Gray, Georges Boyer, and Anson Rainey. Of course, none of these last-mentioned scholars contends that Ugarit, or any other ancient Near Eastern state, exhibits all of the features of medieval European feudalism. What they have stressed through the use of feudal terminology is the role of the king as, in some sense, proprietor of all of the land in his domain. The king grants land (“fiefs”), usually on a hereditary basis, to men (“vassals”) who owe him service (“fealty”) in return. The recipients of land may in turn parcel out plots to their own servants (“serfs”). The result is a hierarchical, somewhat decentralized, political and economic system. This stripped-down “feudalism” is a long way from the military feudalism of the high Middle Ages in Western Europe, with its ethic of knighthood, oaths of fealty, and complex contractual formulae. But it must be admitted that there are advantages in the use of familiar terms that call upon analogies in well-known historical phenomena, provided that the use of these terms is not misleading and does not give a false impression of what was going on under very different conditions in an ancient Near Eastern setting.

Unfortunately, the use of feudal terminology frequently does give rise to misunderstanding and confusion. This is because the essence of feudalism has been defined in two quite different ways. Some have seen it as basically a method of government and not as a distinct socioeconomic system. This was the approach taken by the American medievalist Joseph Strayer, who argued that:

the basic characteristics of feudalism in Western Europe are a fragmentation of political authority, public power in private hands, and a military system in which an essential part of the armed forces is secured through private contracts. Feudalism is a method of government, and a way of securing the forces necessary to preserve that method of government. [Strayer 1979:13; see also Strayer 1956; Strayer and Coulborn 1956]

By defining feudalism strictly in terms of political and legal institutions, Strayer rejected the interpretation of medievalists of Marxist orientation like Marc Bloch (1941; 1961) and his followers in the French Annales school, who have stressed the economic aspects of feudalism. Bloch, for example, focused on peasant life at the expense of a careful analysis of the thought and behavior of the feudal elite, implicitly treating the manorial system of dependent agriculture as the central feature of feudalism. Strayer, however, distinguished feudalism as a political system from the economic institution of manorialism. Manorialism was no doubt the system of land tenure most naturally congruent with feudal political institutions; but even though manorialism coincided with political

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4 Cantor (1993:195ff.) provides a useful capsule summary of the debate among medievalists on this topic (see also Cantor 1991, esp. pp. 140ff. and 278ff., on the lives and works of Bloch and Strayer). The situation is not helped by the fact that “feudalism” itself is not a medieval term but was coined by seventeenth-century lawyers to describe the vestigial laws and customs emanating from a then-vanished society. The term was used pejoratively in the eighteenth century by bourgeois French political philosophers in their criticisms of the French aristocracy, and it was eventually adopted by Karl Marx, who freighted it with additional meaning by using it to describe a universal precapitalist stage in human social evolution. Even etymologically “feudalism” is an awkward term, because it can be argued that it is not the fief (feudum) but vassalage that was central to feudal society.
feudalism at various times and places in medieval Europe, it was not coterminous with it. Feudal lordship and vassalage, growing out of late Roman clientage and Germanic war bands, existed for centuries prior to the emergence of manorialism. The manorial system, on the other hand, developed slowly throughout the early Middle Ages as vassals gradually moved out of their lords’ households and were given land of their own in the form of fiefs worked by serfs. Conversely, in some places—such as Italy—manorialism thrived while feudal vassalage took hold only feebly, if at all. Moreover, in the later Middle Ages feudal political institutions survived the dissolution of the manorial system and its replacement by a money economy.

These temporal and regional variations in medieval European society must be taken into account in arriving at a definition of the essential characteristics of feudalism that will be suitable for comparative analysis. Strayer’s point is well taken: unless one takes the classical Marxist position of assuming that legal and political institutions are secondary and ultimately derivative of economic relationships, one should beware of conflating the political aspects of feudalism with the economic system of manorialism, especially in light of clear evidence to the contrary. Obviously, the debate over the essence of feudalism is ultimately a matter of definition, of deciding which phenomena should be included under the rubric “feudal.” But precise definitions are essential in comparative historical work, and historians of the ancient Near East have often failed to examine carefully the concepts they employ. In Strayer’s terms, for example, much of what Near East specialists have called “feudal” is comparable not to feudalism as a political system but to medieval manorialism, which is a very widespread type of economic system. The use of feudal terms like “vassal” and “serf” therefore creates confusion because they call to mind legal, political, and military institutions that are foreign to ancient Near Eastern societies. As a result, the tendency develops to dismiss feudal analogies outright as misleading and anachronistic when there may well be some value in drawing attention to similarities with medieval manorialism.

In my opinion, there are parallels between the European manorial system of dependent agriculture and ancient Near Eastern systems of land tenure—not to mention the multitude of other similar examples to be found throughout world history. At many times and in many places the granting of land in return for rent in the form of goods and services was a simple and effective solution to the problem of organizing production and consumption in a nonmonetized economy. The “feudal” (i.e., manorial) system of land tenure is therefore a viable comparative concept. At the same time, however, it must be noted that the legal and political thinking that underlay medieval feudalism in Europe is alien to the ancient Orient.

Essential to European feudalism was the notion of a contract between free men, which is very different from the traditional Near Eastern concept of political subordination modeled on the relationship between masters and slaves. It is necessary, then, to separate the economic structures usually associated with medieval feudalism from the ideology of feudalism as a political system.

This has implications for the use of feudalism as a comparative concept. Contrary to current trends in ancient Near Eastern studies, I believe that earlier generations of scholars had a more-or-less accurate perception of the nature of the social and economic arrangements that they described as “feudal.” But the aspects of ancient Near Eastern society that are truly comparable to feudal Europe can be better described in terms of what Max Weber called “patrimonialism”; that is, a political organization understood as an extension of the ruler’s household. This model allows for a hierarchy of land grants and a system of dependent agriculture without imposing an alien concept of political legitimation based on the formal legal rationalism of a contract between free men. The patrimonial household model (PHM) was introduced in chapter 3.1 above, and its application to the Bronze Age Near East will be treated in greater detail below in chapter 12; indeed, the present work is devoted to arguing for its value as an aid to understanding the society of Ugarit and other ancient Near Eastern states. At this point, however, it is necessary to examine the chief alternative to the feudal model in current scholarship on the ancient Near East: Diakonoff’s two-sector model.

2. Igor Diakonoff’s Two Sectors and the “Asiatic Mode of Production”

Diakonoff and other Marxist historians contend that feudal terminology is inappropriate for the ancient Near East, but their dislike of the feudal model is due in large part to the special role played by “feudalism” as a stage in the universal evolutionary progression pictured by Marxist historical materialism, which in its simplest form proceeds in five stages from primitive communalism (based on kinship), to slave societies, to feudalism, and thence to bourgeois capitalism and, finally, communism. What distinguishes the various stages in this scheme are the changing relations between rulers and producers (the “exploiters” and the “exploited”) that are based on the ownership
or lack of ownership of the means of production. For this reason these stages are called “modes of production”—an extremely important concept in Marxist thought. Marx himself gave a concise summary of the relationship between economic modes of production and other social institutions in his famous preface to A Contribution to the Critique of Political Economy, first published in 1859:

The general conclusion at which I arrived and which, once reached, became the guiding principle of my studies can be summarised as follows. In the social production of their existence, men inevitably enter into definite relations, which are independent of their will, namely, relations of production appropriate to a given stage in the development of their material forces of production. The totality of these relations of production constitutes the economic structure of society, the real foundation, on which arises a legal and political superstructure and to which correspond definite forms of social consciousness. The mode of production of material life conditions the general process of social, political and intellectual life. It is not the consciousness of men that determines their existence, but their social existence that determines their consciousness. At a certain stage of development, the material productive forces of society come into conflict with the existing relations of production or—this merely expresses the same thing in legal terms—with the property relations within the framework of which they have operated hitherto. From forms of development of productive forces these relations turn into their fetters. Then begins an era of social revolution. The changes in the economic foundation lead sooner or later to the transformation of the whole immense superstructure. [Marx 1970:20ff.]

In light of this theory of social evolution, Marxist historians of the ancient Near East regard feudality as a stage in the development of human society—a total mode of production with broad social and ideological consequences—and not simply as a particular method of government or a form of land tenure. They assign special meanings to feudal terminology, giving it greater theoretical weight and applying it more broadly than do non-Marxist scholars, who use it (admittedly) rather casually. In Marxist thought, for example, feudal vassals and serfs, unlike slaves, do own the means of production, even though they are personally dependent on a feudal lord and collectively form (at the lowest levels) an exploited class of producers.5 Because private ownership of land was generally thought to have been rare or absent in the ancient Near East, Marxist historians have traditionally rejected the feudal interpretation in favor of some other model—usually the concept of an ancient slave society, which is the precursor to feudalism in the standard Marxist scheme.

Diakonoff himself does not agree with the orthodox Marxist idea of an ancient Oriental slave society, at least in its traditional form. But in rejecting the feudal model he follows Marxist analysis to the extent that he sees in ancient Near Eastern societies a substantial class of “slaves”; that is, nonfree producers who do not own the means of production but subsist on rations or, at best, temporary land allotments. This situation does not characterize feudalism as he understands it, but neither does he believe that slavery was total and generalized before the first millennium B.C., as some Marxists have suggested. Diakonoff’s model differs from both the feudal and slave-society approaches by positing two sectors, one of which was the locus of slavery (namely, the great bureaucratic urban organizations of temple and palace), while the other reflected the continuance, in rural villages, of the earlier stage of primitive communalism based on kinship, and so was characterized by “private” (albeit communal) ownership of the means of production. In neither sector, however, is there any place for feudal-style relations between rulers and producers.

In other words, Diakonoff believes that the labor force in ancient Near Eastern states did not consist exclusively of state dependents (i.e., “slaves,” in the Marxist sense), but was divided into a “free” sector composed of persons who owned and worked their ancestral land (collectively, in his view) and a “non-free” sector of state dependents who performed specialized services or worked the king’s land in return for rations but did not themselves own any of the means of production. In itself the state sector was a centrally organized bureaucracy, as in the older

5 This understanding of feudalism has influenced Soviet studies of the ancient Near East. For the benefit of non-Marxist readers, Diakonoff (1969a:15f.) defines several terms, e.g., “slave,” “serf,” “slave society,” and “feudal society,” that have special theoretical importance within the framework of historical materialism as it has been applied to ancient Mesopotamia. It is worth quoting here his definition of feudal society: “A feudal society is one based on the extra-economic coercion of an exploited class of producers owning means of production but personally dependent on individual members of the governing class, or on a body representing it; typical of such a society is a division into estates (the members of each estate having a different degree of legal capacity), and ownership of the producers in at least some of the means of production. The term does not necessarily imply a hierarchic structure of society based on vassalage, a phenomenon which occurs also in other socio-economic formations.” Notice that this definition is the exact opposite of Strayer’s (cited above), in the sense that for Strayer vassalage was the essential feature of feudalism, as opposed to any particular system of production.
“temple economy” or “state socialism” model, and it was characterized by a complex redistributive apparatus. The free sector possessed a different structure, however, and the king exploited it only indirectly, through taxation and conscription. Contrary to the feudal model, as Diakonoff understands it, producers who were personal dependents of the ruler (in the state sector) did not own the means of production, and those producers who did own the means of production (in the free sector) were not dependents but independent proprietors. Contrary to the feudal model also, the king did not own all of the land. According to Diakonoff (1982:18f.), it was in his capacity as “sovereign” and not as “proprietor” that the king demanded goods and services from the free sector; thus, these were “taxes,” as in modern capitalist states, and not “rent” paid by tenants, as in medieval feudalism. The king was also a proprietor in his own right, however, and he personally owned a great deal of land which his enslaved dependents in the state sector cultivated for him in return for rations or a share of the crop.

Diakonoff admits that documentation for the free sector is generally sparse, a fact he attributes to the bias in our sources toward matters of direct concern to the state. But in Mesopotamia during the Old Babylonian period, he argues, members of the two sectors were distinguished terminologically as awilî (free) and muškênû (nonfree); and everywhere in the ancient Near East, he asserts, free men were identified by their patronyms while royal dependents were usually identified only by their professions (ibid., p. 28). Moreover, one of the most important features of the two-sector model, from Diakonoff’s point of view, is the difference in family structure between the two sectors. The free sector was characterized, in earlier periods at least, by “hierarchic structures of extended families and clan communes” (p. 30) that provided mutual aid and served to spread the risk of a bad harvest or other misfortune. Workers in the state sector, on the other hand, had small families; they had no need of extensive kinship ties because they were already part of a large “household” in which food was redistributed in the form of rations and from which they received protection, at the cost of their independence.

The extended-family organization of the free “communal” sector receives repeated discussion in Diakonoff’s writings. It is best demonstrated, he believes, in Middle Assyrian documents and in the Nuzi texts from the kingdom of Arrapha, which were studied in detail by his colleague Ninel Jankowska (1969a; 1969b; 1986). This aspect of Diakonoff’s model has attracted criticism, however. Nicholas Postgate (1982:309f.), for example, sees no evidence in Middle Assyrian texts for collective ownership of land by extended-family communes. W. F. Leemans (1983; 1986) has also questioned the existence of extended families and rural communes in ancient Mesopotamia, especially during the Old Babylonian period and later. One could argue, however, that the basic two-sector model need not specify the internal structure of the free sector. It can dispense with extended-family communes, provided that Diakonoff’s main tenet is preserved: that land was owned privately outside the state sector and there was no private land ownership within it. In light of the Marxist theory of social evolution, however, it is clear why Diakonoff is reluctant to jettison this aspect of his model, for it provides the link with the earlier primitive-communal mode of production from which the two-sector society presumably developed.

It is necessary to digress here briefly to discuss the intellectual climate in the Soviet Union and the vicissitudes of Marxist-Leninist historical analysis in the twentieth century in order to understand the genesis of the orthodox Marxist model of the ancient Near East and Diakonoff’s variant of it. In the writings of Marx and Engels, human social evolution is usually presented in terms of five successive modes of production occurring in a more-or-less unilinear sequence. These are the primitive-communal, slave, feudal, capitalist, and communist modes of production that were mentioned above. Each mode gives rise to the next according to internal dynamics in a dialectical process that is explicated in the theory of historical materialism (although Marx explained this mechanism fully only for the last three stages). Marx derived these evolutionary stages almost entirely from his study of European history, however; so later Marxist theorists, faced with greater knowledge about the ancient Near East and the Orient in general, have attempted to modify the standard sequence.

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6 It is worth noting that this does not hold true in Ugarit: royal servicemen dependent on the palace are usually identified by patronym, and in many cases by patronym alone.

7 O’Leary (1989:162–75) summarizes the modern neo-Marxist debate about unilinear versus multilinear readings of Marx and the addition or subtraction of various stages. The writings of Marx and Engels are sometimes ambiguous, but in their mature works there is strong textual warrant for a basic five-stage model, and many Marxists have subsequently taken this view, although others are uncomfortable with such a simple, universal, unilinear sequence and appeal to some of Marx’s early writings in support of their “multilineal” interpretation.
In the Soviet Union in the 1920s and 1930s, for example, several historians made much of the concept of an “Asiatic mode of production” (AMP) that appears a few times in Marx’s writings. The AMP is derived from the idea of “Oriental society” that Marx picked up from the works of the British political economists James Mill, Richard Jones, and John Stuart Mill. As Brendan O’Leary (1989) and others have pointed out, the concept of “Oriental despotism” has had a long history in Western political thought, but Marx derived his idea of it most directly from these nineteenth-century British writers (see O’Leary 1989:73ff. on the influence of the Utilitarian economists on Marx and Engels, with relevant references).

According to the British political economists, Oriental society is characterized by isolated, self-sufficient peasant communities dominated by a despotic state organization that owns all of the land, extracts rent from the peasants, and coordinates and controls large-scale irrigation projects and other public works. These authors believed that the lack of private property and the economic autonomy of the villages hindered trade and capital accumulation and stifled individual initiative, rendering Oriental society stagnant and underproductive. Marx agreed that village autarky explained the lack of development in this system, but he held that the absence of private property was only apparent and was not the distinguishing feature of Oriental society. Most discussions of Marx’s understanding of the AMP have missed this point, but Eric Hobsbawm, in his introduction to the English edition of Marx’s rambling notes on Pre-Capitalist Economic Formations, argues that:

Marx evidently held that the fundamental characteristic of this system was “the self-sustaining unity of manufacture and agriculture” within the village commune, which thus “contains all the conditions for reproduction and surplus production within itself” (pp. 70, 83, 91), and which therefore resists disintegration and economic evolution more stubbornly than any other system (p. 83). The theoretical absence of property in “oriental despotism” thus masks the “tribal or communal property” which is its base (pp. 69–71). [Hobsbawm in Marx 1965:33]

In other words, in the AMP the primitive-communal mode of production, involving communal ownership of the means of production, persists in semiautonomous “closed” villages even though the state—“the despotic government which is poised above the lesser communities” (Marx 1965:71)—appropriates their surplus production. Thus Marx here (in a work not published in his lifetime) integrated the “Oriental society” of the classical economists into his sequence of modes of production. In contrast to the simple five-stage sequence of modes of production advanced in Marx’s other works, his notes on precapitalist economic formations suggest that there were three or four alternative routes out of the primitive-communal mode of production (see Hobsbawm’s remarks in Marx 1965:32ff.). One of these, of course, was the “ancient” or slave system of Greece and Rome that Marx discusses frequently; but in Asia there emerged an unchanging system (the AMP) that persisted in the Orient until it finally succumbed, not to internal decay but to an external force, namely, the onslaught of Western capitalism in the nineteenth century.

Marx also speaks of “Germanic” and “Slavonic” modes of production, although these concepts are developed in less detail. But it was the AMP that caught the attention of Soviet Orientalists in the 1920s (see Dunn 1982:7–37 and O’Leary 1989:8ff.). This concept was seen as an aid to Marxist analysis not only of ancient Near Eastern societies but also of modern Asian conditions, including recent developments in Russia itself. As such it could serve as a guide to revolutionary strategy in the Far East. But the AMP lacked an internal dynamic and therefore fitted awkwardly into the framework of the Marxist theory of inexorable social evolution; furthermore, it apparently met with opposition from Stalin himself for reasons of practical politics. Consequently, the concept of the AMP was censored and in the 1930s Soviet discussion of it ceased. Stalin’s party catechism of 1938, Dialectical and Historical Materialism—

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8 E.g., Marx (1970:21), in the preface to A Contribution to the Critique of Political Economy (cited above), lists the “Asiatic, ancient [slave], feudal and modern bourgeois [capitalist] modes of production . . . as epochs marking progress in the economic development of society.” See also Marx 1965 [written in 1857–58], esp. pp. 69–71, 83, 91–95. The latter work consists of notes by Marx entitled Formen der kapitalistischen Produktion vorhergehend that were first published in Moscow in 1939–41 and later re-published in Berlin in 1952–53 (in their first widely accessible edition) as part of the Grundrisse der Kritik der politischen Ökonomie. Dunn (1982) discusses the Soviet debate on the AMP that began in the 1920s and was renewed in the 1960s, while O’Leary (1989) provides a broader and more detailed analysis of the AMP and its relationship to Marxist theorizing in the West as well as in Soviet circles, together with an extensive bibliography.

9 It is true that some of Marx’s scattered comments elsewhere appear to reject the existence of “private” property in the AMP, but the notes on Pre-Capitalist Economic Formations contain his most extended discussion of Asiatic society, and I believe that Hobsbawm is correct to stress the importance of Marx’s reference there to “communal property.”
ism, recognized only the five standard modes of production: the “primitive communal, slave, feudal, capitalist and Socialist” (Stalin 1972:323). Brendan O’Leary outlines the reasons for this “conceptual purge”:

From then [1938] on the five-stage model of history was decreed correct Marxism. The AMP had entered into doctrinal limbo. The stabilization of the USSR was felt to require a rigid codification of Marxism, and the excision of the AMP formed one significant part of a wider vulgarization and simplification of Marxist doxology under Stalin’s aegis. The AMP created genuine problems for historical materialism but it also had problematic ideological connotations. Moreover, it allowed far too much Marxist legitimacy to opponents of Stalin’s policies in the East and the USSR. [O’Leary 1989:9]

The chief representative of Stalinist orthodoxy among Soviet Orientalists was Vasilii Struve, who promoted the “slave society” interpretation of the ancient Near East (see Struve 1969 (orig. Russian 1933) and Dunn’s commentary in Dunn 1982:43–61). He was not deterred in this even by the publication in Moscow, in 1939, of Marx’s Pre-Capitalist Economic Formations, where Asiatic society is discussed in some detail. Struve maintained that, for Marx, the Asiatic system was merely a variant of the slave mode and not a separate mode of production. This view prevailed until the late 1950s, when Soviet discussion of the AMP revived after Stalin’s death.

One of the instigators of the renewed debate was Igor Diakonoff, who published a paper in 1963 in which he argued that: “Alongside slave production, there always existed small-scale subsistence production by independent free producers” (Diakonoff 1963:33). In this paper Diakonoff went out of his way to harmonize his assertions with those of Struve (who was still living), but it is clear that his view of ancient Near Eastern society was actually quite different. Although he presented his two-sector model as a variant or early form of the slave mode of production, it really describes an entirely different system—one that bears a strong resemblance to the AMP, even though Diakonoff himself does not use this term. In his later writings during the 1960s and 1970s, Diakonoff claimed to oppose the concept of the AMP, but his two-sector model contains all of the elements of the Oriental system described by Marx in Pre-Capitalist Economic Formations, including self-sufficient villages, “private” communal property, and a ruler who accordingly does not own all of the land.

It is not clear why Diakonoff has refused to use the term “Asiatic mode of production” when his own model is so close to it, and in view of the fact that the concept began to be defended with impunity in the post-Stalinist era by other Soviet scholars. Mario Liverani (1972:228f.) attributes this to Struve’s continuing prestige and influence. Or it may be that Diakonoff was reacting to the original Soviet version of the AMP, put forward in the 1920s before the publication of Marx’s Pre-Capitalist Economic Formations—a version of the Asiatic model in which the ruler owned all of the land, implying that “taxes” were identical to “rent” (as in the traditional Western view of “Oriental despotism”) and that the members of the village communes therefore constituted an exploited class. Diakonoff’s two-sector model necessarily distinguishes taxes from rent; that is, the ruler’s income from the free village sector is structurally and conceptually different from the yield of his own lands. He also rejects the notion that the tax-paying members of the free village communes, as owners of the means of production, formed an exploited class. But as we have seen, Marx (1965:70) himself argued that the Asiatic system was characterized by communal ownership of the means of production on the part of free agricultural producers. It is true that Marx was inconsistent in his writings on the AMP, because he followed the British political economists in failing to distinguish taxes from rent in Oriental society. In terms of his own theory, this is contrary to his insistence on the villagers’ ownership of the means of production—as proprietors, they ought to pay taxes rather than rent. Nonetheless, the similarities between Diakonoff’s two-sector model and the AMP are too great to ignore. Like the AMP, Diakonoff’s two-sector model is a modification of the standard Marxist sequence of modes of production that provides room for the persistence of “earlier” forms of kin-based communalism within an urban-based “state” society dominated by an elite of unproductive exploiters.

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10 This discussion was stimulated in part by the publication and wide dissemination of Karl Wittfogel’s views on “Oriental despotism” (see Wittfogel 1957). Wittfogel was a Sinologist and former Marxist who used his analysis of the Asiatic system to attack what he saw as the parallel phenomenon of modern Communist totalitarianism. I will not treat Wittfogel’s views here, but see the reviews by Eisenstadt (1958) and Pulleyblank (1958a; 1958b), and the chapter on Wittfogel in O’Leary 1989:235–61. These critics point out that Wittfogel makes use of the same kind of deterministic monocausal model favored by his Marxist opponents. He employs a variant of Marx’s concept of the AMP, lamenting its suppression under Stalin, but he places greater emphasis on the management of large-scale hydraulic projects as the vehicle of despotic state control.

11 Dunn (1982:71–76) describes the impact of Diakonoff’s paper in Soviet circles and the lively debate that followed its publication.
In a long article, first published in 1981, the Italian scholar Carlo Zaccagnini (1989) defended the use of the AMP as a "heuristic model" that is useful for describing a number of ancient Near Eastern societies (cf. Komoróczy 1978). In this he follows Mario Liverani, whose version of the two-sector model, which is essentially the same as Diakonoff's, is correctly identified by Zaccagnini (1989:21f.) as equivalent to the concept of the AMP (see Liverani 1984). Zaccagnini also points out the obvious influence on Diakonoff of Marx's statements about the AMP (p. 12). Unlike Diakonoff, however, Zaccagnini exempts from the AMP the intensively irrigated regions of the Nile valley and the Mesopotamian alluvial plain, arguing that the independence of the free village sector, with its communal ownership of the means of production, was preserved much longer—indeed, until the end of the Bronze Age—in the dry-farming areas of northern Mesopotamia, Anatolia, and the Levant (p. 55). Even in those areas, however, the AMP gave way to something else during the first millennium B.C.: presumably some variant of the "slave mode of production," in which the peasants had lost their ownership of the means of production, in the period of the Neo-Assyrian, Achaemenid, and Hellenistic empires. In like fashion, Liverani (1975:163f.) speaks of the emergence of "l'esclavage généralisé" by 500 B.C.

Liverani's use of the two-sector or AMP model for Ugarit, and for Bronze Age Syria and Palestine in general, will be discussed in some detail below (see also Gottwald 1983 for an application of the AMP to Late Bronze Age Canaan). It is worth noting here, however, that the defects of the AMP concept have not been remedied by its current advocates among historians of the ancient Near East; namely, its disagreement with historical data and (what is a serious defect from a classical Marxist perspective) its ultimate lack of congruence with the Marxist theory of social evolution adopted by its proponents. But its present popularity among Marxist scholars can be explained in light of developments in Marxist theory over the last several decades that have occurred in response to the accumulation of historical knowledge about parts of the world of which Marx and Engels were ignorant. Presumably, the concept of the AMP, by whatever name, is attractive to such scholars because of its provenance in the works of Marx and its apparent utility in explaining features of social organization peculiar to the ancient Orient.

The adoption of the two-sector model by non-Marxist scholars is more puzzling, however, for few of them would subscribe to the overarching theory of social evolution in which the AMP concept is situated. But the wide appeal of Diakonoff's model should probably be attributed to the continuing influence of the positivist-functionalist sociohistorical paradigm in ancient studies (discussed above in chapter 3.1). The functionalist paradigm shares the AMP's emphasis on a fundamental urban-rural dichotomy in premodern civilizations, including the Bronze Age Near East. Indeed, Marx himself inherited this assumption from leading exponents of positivism such as John Stuart Mill, in a line of thought that goes back to the ahistorical functionalism of Adam Smith in The Wealth of Nations (see chapter 6.1 above). Here we can see that a similar intellectual outcome is produced by the positivist theoretical assumptions that underlie both Marxism and functionalism, in contrast to the quite different patrimonial model of Bronze Age Near Eastern society produced by a hermeneutical approach.

3. The Functionalist View of Ancient Near Eastern Urbanism

In a 1977 essay entitled "The 'Urban Revolution' in a Socio-Political Perspective," the Assyriologist and archaeologist Giorgio Buccellati concisely presented the functionalist view of ancient Near Eastern urbanism. Although it is expressed in different ways within different communities of researchers, the functionalist paradigm has underpinned most interpretations of ancient urbanization and state formation over the past few decades, especially the interpretations proposed by Near Eastern archaeologists. Buccellati is typical in holding that an increase in settlement size alone, brought about somehow by technological, ecological, and demographic factors, was sufficient to produce a qualitative change in social organization; thus he claims that "large human settlements are not only proof of incipient urbanism but also of a new political order—the bureaucratic state." (Buccellati 1977:37). He argues that greater size implies internal differentiation in terms of social stratification and professional specialization. Fellow-residents of a city no longer knew one another personally but only

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12 O'Leary (1989:152–202) provides a detailed discussion of the theoretical problems that the AMP creates for historical materialism, while Adams (1988:26ff.) notes the flaws in the AMP as an interpretation of ancient Mesopotamian evidence.

13 Buccellati has also defended the functionalist paradigm more recently, in a 1996 essay on "The Role of Socio-Political Factors in the Emergence of 'Public' and 'Private' Domains in Early Mesopotamia," in Hudson and Levine (eds.) 1996.
functionally, as members of various professions and status groups. They were therefore held together not by personal ties but by means of what Durkheim called “organic solidarity,” which is based on the interdependence of persons who perform complementary specialized functions (on Durkheimian functionalism see chapter 3 above).

Buccellati then argues that the existence of disparate social functions necessitated the emergence of other specialists whose function was to coordinate the activities of everyone else. These were the first bureaucrats, whose management role gave them considerable authority over the other inhabitants of the city. A chain of command developed, and finally, taking functionalist logic one step further, an abstract system of hierarchical offices that existed independently of the personal characteristics of individual office-holders and subsequently formed an important tool in the exercise of political power. According to Buccellati, this developing bureaucratic system became self-sustaining because it conferred a functional advantage, in terms of efficiency and productivity, on the city as a whole. Thus the mere quantitative expansion of the size of settlements produced a qualitative transformation entailing the depersonalization of social life and the emergence of an impersonal, abstract, functionally oriented system that underlay urban social organization, namely, bureaucracy.

Buccellati presents a widely held view of the development of ancient Near Eastern urban society, but in my opinion his model rests on questionable assumptions and it is not supported by the available evidence. It must be discussed here in some detail, however, because it is so widely accepted. Buccellati employs an externalizing, functionalist approach to sociohistorical reconstruction—a frequently used paradigm, but one which has been severely criticized in recent years, as I have noted in chapter 3.1. In functionalist social theory there is a tendency to reify externally observed patterns of motivated social behavior as self-existent entities, and there is a corresponding neglect of social actors’ understanding of their own situation.

On the subject of the existence of a “public” versus “private” dichotomy in urban Mesopotamia, for example, Buccellati admits that:

The polarity public/private was never articulated in the social and political conceptualization of even the latest historical periods in Mesopotamia, so it is a fair assumption that it was not an operative concept in the earlier stages of urban development. It was however an operative reality, inasmuch as decisions were taken and actions implemented that presupposed a real distinction between the two spheres. [Buccellati 1977:35]

But from a Weberian interpretive perspective, Buccellati wrongly ignores the native understanding of social action in favor of an alien functional logic that conforms, not to the Mesopotamian data, but to the modern conception of urbanism. He attributes any activity that involved or affected all or most of the social group, such as the construction of a monumental building, to the “public” sphere, because: “These functions . . . were provided by the leadership on behalf of the human group as a whole, and were in the nature of a public service” (ibid.). He cites no textual evidence for this interpretation; indeed, there can be none, because the people concerned did not view it this way, as he points out. The fact of the matter, however, is that in Mesopotamian texts the building of a temple (for example) is not presented as a “public service,” or even as a communal “public” endeavor, but as the personal, pious deed of a ruler on behalf of his god, accomplished with the personal, dutiful assistance of his people.

Of course, we can attach any interpretation we wish to such an event from an external point of view; but an interpretation that neglects the native understanding of social behavior risks serious misunderstanding of the social relationships and motivations that produced it. According to the hermeneutical method of sociohistorical reconstruction, native conceptions, far from being dismissed as forms of “false consciousness” that mask “real” economic relationships, constitute the only valid basis for sociohistorical generalization (see Part One). This is because social actors do not experience “society” as an abstract whole; instead, they experience a set of meaningfully constituted social relationships in which the shared meanings attributed to social actions involving authority and obedience constrain (and explain) patterns of social behavior—patterns that functionalists mistakenly reify as structures which are independent of the subjective conceptions revealed to us in the native forms of expression used to describe them.

In contrast to functionalism, therefore, the interpretive approach shifts attention to native forms of expression; and the first thing to note is that ancient Near Eastern texts do not exhibit any conception of impersonal bureaucracy, as Buccellati admits. The functionalist-bureaucratic model advanced by Buccellati and others must accordingly rely on unproven inferences about ancient Near Eastern urbanism that are based on prior assumptions concerning the import of certain external features of social organization having to do with size and complexity. But there is nothing in the mere existence of large walled settlements, complex administrative hierarchies, the knowledge of writing, or monumental architecture that
requires a formally rationalized impersonal bureaucracy and a corresponding distinction between public and private spheres. The necessary lack of face-to-face contact among the inhabitants of a large city does not imply that they had no mutual bond of personal attachment through, for example, common membership in the patrimonial “household” of their ruler (human or divine). This is the sort of personalized “mechanical” solidarity operative in the large tribal groups (rural and urban) known from Middle Eastern history and ethnography, whose members are not all acquainted with one another.

Furthermore, Bucellati and others who adopt the functionalist paradigm exaggerate the extent of “professional” specialization and functional interdependence among the inhabitants of ancient Near Eastern cities. The widespread system of land allotments known from various periods suggests that most urban specialists were self-subsistent in basic commodities. This is especially likely if professional duties were only part-time for many, rather than full-time, as the work of Piotr Steinkeller and others suggests was the case (for example) even during the highly bureaucratized Ur III period in Mesopotamia at the end of the third millennium B.C. (see chapter 11.2 below). Even among urbanites, therefore, a common agrarian lifestyle could lead to mechanical as opposed to organic solidarity (to use Durkheim’s terms); hence the mere numerical expansion of settlements need not have effected a qualitative social transformation in the Bronze Age Near East. Indeed, the absence of a qualitative urban-rural dichotomy from the native perspective is reflected in many ancient Near Eastern languages, in which the same word is used to denote a settlement of any size, be it a large walled city or an unwalled village; for example, Sumerian *uru, Akkadian *alum(m), Egyptian *niwt, Ugaritic *qr(y)t, and Hebrew *qtr.¹⁴

A more nuanced (but also theoretically less coherent) treatment of ancient Mesopotamian urbanism has been published recently by Marc Van De Mieroop (1997). His work may perhaps be taken as typical of the current approach of documentary historians, who are aware of the critiques of functionalist systems theory and the dangers of anachronism, and so are more cautious in assessing the role of craft specialization, rationalized bureaucracy, and market exchange in ancient Near Eastern society. Thus Van De Mieroop (ibid., pp. 13ff.) acknowledges the arguments made by antiformalist Assyriologists such as Johannes Renger, who claim that market exchange was relatively unimportant during the Bronze Age, in particular, because economic exchanges were embedded in redistributive networks of personalized social relationships. Similarly, anthropologically oriented archaeologists who work in the Near East now pay more attention to the role of “ideology” in structuring social relations, calling into question the de-personalized systemic economic models which were once thought to explain major social change (e.g., Wattenmaker 1998; Stein 1999), and even questioning the validity of certain aspects of the urban-rural dichotomy that have so often been assumed in previous research (e.g., Schwartz and Falconer 1994).

But many of these studies, by archaeologists and documentary historians alike, have not rid themselves of the ahistorical functionalist assumption that, in any period of human history, regardless of the prevailing symbolism of social order, differences in the quantitative scale of human settlements necessarily correspond to qualitative differences in the structure of social relations. Thus Van De Mieroop (1997:10–12), after noting that ancient Mesopotamians did not seem to think it worth making a lexical distinction between cities and villages, and after noting that various externally observable objectivist criteria for a cultural definition of the “city” have proved to be inadequate, proceeds to take as his fundamental criteria of urbanism four “commonly accepted quantifiable dimensions” by which historians distinguish European cities from noncities in the early modern period (A.D. 1500–1800). These criteria are: (1) population size; (2) density of settlement; (3) share of nonagricultural occupations; and (4) diversity of nonagricultural occupations. Having accepted this objectivist approach, Van De Mieroop has no qualms about adopting Adams’s (1966) now rather dated evolutionist model of Mesopotamian urbanism as a qualitatively innovative social phenomenon that arose in a society in which hierarchical class structures gradually replaced kinship and cities became functionally necessary as nodes of exchange among classes (Van De Mieroop 1997:27ff.).¹⁵

¹⁴ For ancient Mesopotamia, Van De Mieroop (1997:10) notes that: “The Akkadian term *alum was used for anything from the metropolis of Babylon in the sixth century to a farmstead with seven inhabitants in the area of Harran in the seventh century. It was used for the entire city of Nineveh as well as for a section of it. The Akkadian language had a few other terms to allude to permanent settlements, such as *kaprum, a farmstead, *dūrum, a fortified settlement that could be the size of a large city like Dur-Sharrukin, and *mīhazum, which could be anything from a small enclosure to a large town. But in the great majority of cases *alum was used.” On the Egyptian term *niwt, see Bietak 1979.

¹⁵ It is odd, then, that Van De Mieroop elsewhere follows I. J. Gelb in stressing the role of households and household
Thus the positivist inference persists that there was an urban-rural dichotomy which generated a basic bipolar social dynamic as early as the third millennium B.C. 16 More generally, the same assumption underlies the prevailing archaeological concept of a “complex” society as opposed to a “simple” society, where social complexity can be measured by an external observer using such criteria as settlement size and regional settlement pattern, architectural style and urban plan, and artifact manufacture and distribution (e.g., Tainter 1988). Yet for reasons I have elaborated in Part One, the burden of proof should be on those who posit this sort of dichotomy in the Bronze Age Near East, in particular, rather than on those who reject the analogy with postmedieval Western urbanism.

Even granting that ancient Near Eastern cities were less agrarian than villages (which in many cases is doubtful, in my view, especially in the Levant), the sociological significance of Van De Mieroop’s four criteria is questionable from a hermeneutical perspective, to the degree that the social actors in question did not pay attention to these criteria. The enormous sociohistorical significance of the city and of the urban-rural dichotomy in late medieval and modern Europe and America is not, in fact, directly related to these sorts of quantifiable criteria. The city is a significant concept because of the new and distinctively urban (i.e., “bourgeois”) mode of social organization and capitalist production which these external criteria reflect, especially the obviously most important criteria of the share and diversity of nonagricultural occupations. European economic historians jump directly from these externally observable and quantifiable criteria of urbanism to an economically and sociologically meaningful classification of social phenomena, bypassing the question of the native symbolism of social order. But they can do this only because they can assume the presence in the periods they study of an influential symbolic system (in which they themselves participate) characterized by a degree of individualism, rationalism, and egalitarianism that is not attested in antiquity.

In other words, an objectivist, functionalist analysis will often work in this case because the society has already been symbolically ordered along formally and instrumentally rational (i.e., functionalist) lines (see chapter 4.4 above). But for ancient historians, the symbolic ground of social action cannot be ignored in favor of “objective” criteria which may have had a very different meaning for ancient peoples, or no meaning at all.

Another way of putting the issue is to ask whether economic specialization and systemic integration must necessarily correspond to political specialization and integration. The growth of large settlements as a political phenomenon in which large numbers of people are brought under the rule of one person—he the king of a single city and its hinterland or an emperor who dominates many cities—might well be unrelated to economic specialization and trade, or the lack of it. The Bronze Age evidence can be explained according to a model of the city as an overgrown agricultural village, or rather an agglomeration of similarly structured villages, each corresponding to a “clan,” within an economic context in which most urban households remained self-sufficient in basic commodities. The settlement hierarchy which results is the product of political and military factors rather than economic interdependency, in contrast to the anachronistic predictions of Central Place Theory. Indeed, few Bronze Age urban centers attained a size

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16 Among Syro-Palestinian archaeologists, Steven Falconer (1994), for example, makes agricultural self-sufficiency the basic criterion of rural as opposed to urban settlements already in the Early Bronze Age. He adopts a threshold of 35 ha as the size beyond which a settlement could not be self-sufficient and must therefore be classed as “urban.” By definition, therefore, no urban settlements existed in Early Bronze Age Palestine, and there were very few even in the “highly urbanized” Middle Bronze period. This leads Falconer to emphasize “rural complexity” as opposed to “urban preeminence” in the Levant, indicated by the presence of “urban” features such as temples in quite small settlements (see also Falconer and Savage 1995). But “rural complexity” is just a way of saying that there was no meaningful urban-rural dichotomy. We should therefore abandon objectivist economic criteria of urbanism altogether.
that required them to be provisioned by the surplus of surrounding villages, as opposed to subsisting on what their own inhabitants produced. This was especially true in Syria-Palestine, as we shall see.17

Some commodities were brought as taxes into palace and temple storerooms, possibly from quite far afield. This was done in order to feed the members of royal and priestly establishments, who numbered perhaps in the hundreds in a given city; but the volume of such shipments in proportion to overall production should not be exaggerated in the absence of direct evidence. Furthermore, it is not clear whether such deliveries from outlying villages were a matter of economic necessity, or even of economic efficiency, as opposed to being symbolic of political authority. In any case, we can conclude that the majority of Bronze Age urbanites in the Levant were involved in subsistence agriculture simply on the grounds that a large proportion of the total population was resident in walled towns.

The best analogy for this is the large “agro-town” found in Sicily, Sardinia, and parts of Spain until the first half of the twentieth century, in areas of highly nucleated settlement (Chisholm 1979:47–52). These towns were, in effect, overgrown villages with as many as 10,000 mostly peasant inhabitants, who traveled every day to the surrounding fields and orchards. Around the town within a radius of 3–4 km was a zone of intensive cultivation in which labor-intensive crops such as grapes and olives were located. Beyond 4 km the land was largely devoted to wheat and barley, and beyond that to pasture.18 The fields farthest from the town were the most likely to be left fallow, but in the inner zone of intensive cultivation manure was employed to increase soil productivity and to reduce fallowing, making it possible to sustain large populations from local agricultural production (for Bronze Age Near Eastern analogues to this, see Wilkinson 1982; 1989; 1994). These recent Mediterranean examples, characterized by similar crops in a similar ecological environment, show that there was no functional necessity for large-scale importation of food into even quite densely populated urban centers of the size we encounter in the Bronze Age Levant.

The main difference between a city and a village in the Bronze Age lay not in a basic economic differentiation leading to systemic interdependency, of the type envisioned by Adam Smith in *The Wealth of Nations*, but in the presence in the city of a monumental regal-ritual center which, together with military fortifications, served as the practical and symbolic focus of political power. But even this was a quantitative rather than a qualitative difference, because it is likely that in the smallest villages there was a venue for the local cult and for public appearances of the village chief, although these manifestations of a divine-human political hierarchy are far less visible archaeologically. In general, there is no evidence that “archaic” or “pre-Axial” urban centers were the home of a qualitatively different kind of social group, imposing its own program or vision on the rural periphery, as Eisenstadt has pointed out (see chapter 4.2 above). Instead, there was a symbolic homology between the social center and its periphery, so that political elites were not “autonomous” but were “embedded” in traditional networks of ascribed status.19 The growth and maintenance of large settlements can therefore be explained in terms of highly personalized “patrimonial” social relations without the need to invoke systemic economic interdependencies involving the

17 Dever (1987b; 1995b) has defended a “modified” version of Central Place Theory in discerning an economically specialized three-tier settlement hierarchy in Palestine during urbanized periods in the Bronze and Iron Ages. But in both Late Bronze Ugarit and Iron Age Israel, to take two examples, there is no evidence for a basic economic differentiation between “central places” and peripheral villages (see chapters 8 and 14).
18 This arrangement of crops was typical of Sicily and Sardinia but was inverted in La Mancha in Spain, where the grain-fields were placed closest to the town. But under the more arid conditions there grain was a more labor-intensive crop than olives or grapes (Chisholm 1979:51f.).
19 Alexander Joffe’s (1998) recent survey of “disembedded capitals” in the Near East fails to distinguish between pragmatic as opposed to symbolic disembodedness. Bronze Age rulers occasionally founded new cities for practical political reasons. These were indeed the symbolic focus of political power, as were “old” capitals, but the political elite remained embedded in a traditional ascriptive framework and there was a symbolic homology between center and periphery—with the possible exception of the Egyptian New Kingdom capital of Akhetaten (modern el-Amarna, discussed below in chapter 12.9), the fruit of Pharaoh Akhenaten’s “Atenist heresy.” Normally, the founding of a city was merely one of an arsenal of pragmatic political moves rather than an embodiment of a utopian ideological vision that established a symbolic distance between center and periphery. Joffe acknowledges that the so-called disembodied capitals he cites were necessarily “reembedded” into traditional types of political relationship (p. 573), and that the term “disembodied capital” is thus a misnomer (p. 552). But a chronological distinction must be made here, for “reembedding” is more characteristic of Joffe’s earlier than his later examples. His lack of attention to the specific symbolic history of the region leads him to misleadingly conflate pre-Axial and post-Axial examples; e.g., Old Akkadian Agade versus Islamic Baghdad and Samarra.
exchange of large volumes of subsistence goods or of productive tools and resources.

Some goods did circulate among households, of course, and in so doing they served as vehicles of political influence and social prestige. In this category we can include both regular “taxes” or “duties” consisting of goods and services (rental payments to human and divine landlords) and ad hoc “gifts” of various kinds, especially of high-value luxury items, which incurred a reciprocal obligation on the recipient, whether he was of higher status than the giver, of lower status, or a social equal. In this kind of prestige economy the demand for specialized craft products may well have increased in urban settings with the increasing size of the social group, because such goods can communicate social identity and serve as marks of distinction, as Patricia Wattenmaker (1998) has argued. But available archaeological and textual evidence indicates that specialized production often remained a part-time, home-based occupation.

Wattenmaker emphasizes the symbolic meanings of material goods in a way that the previous generation of Near Eastern archaeologists did not. She discusses economic specialization in terms of prestige and nonverbal communication rather than functional or adaptive necessity, proposing that “two distinct but closely related factors contributed to the shift to specialized production in complex societies, in both the nonelite and elite sectors of society. These include (1) the increased importance of goods to signal social identity, consequent efforts to control access to goods expressing prestige, as well as attempts to gain access to, or imitate, them; and (2) the need for greater standardization within the categories of goods used to convey information as sociopolitical complexity increased. The need for standardization resulted from greater social scale and complexity and the consequent need for more distinctions in social identities to be made by goods (used as communicators). The greater efforts to control access to goods stemmed from increased status differentiation in hierarchical societies and the increased role and effectiveness of materials in signaling status in large-scale societies” (Wattenmaker 1998:11).

The delayed impact of hermeneutics in social anthropology, beginning in the 1970s, can be seen, after a further delay, in work published in the 1990s by Near Eastern archaeologists such as Wattenmaker; cf. the earlier work by anthropologists such as Schneider (1977), Brumfiel (1987), and others.

By way of comparison to the Bronze Age Near East, it is worth noting Brumfiel’s (1987) conclusion that in Aztec society specialization in the production of subsistence goods was largely a part-time endeavor, because of the limitations of market exchange, whereas “[t]he Aztec capital, Tenochtitlan, became a center of artistic production where highly skilled, full-time craft specialists manufactured a wide array of goods for elite consumption” (p. 117). But in the Bronze Age Near East (and Ugarit in particular) there is evidence that even the production of luxury items was part-time, at least for the many craft specialists who were not directly supported by a palace or temple establishment and who headed their own large landholding households that were self-sufficient in basic commodities. Indeed, it is not clear how we can be sure that the craft specialists serving the elite in Aztec society, however skilled, were “full-time.” Note also the critiques of V. Gordon Childe’s emphasis on the role of full-time craft specialists in Bronze Age Europe and the Near East in Wailes 1996 (summarized by the editor on p. 10), esp. the essays by Gil Stein and Richard Zettler.

The influence of the political approach can be seen in a recent book on urbanism in ancient Israel by Ze’ev Herzog (1997), who adopts a political model, emphasizing the role of the city as a “container of power” (p. 13). According to Herzog, centralized political institutions emerge for a variety of reasons when nonurban communities are “driven to establish central institutions in order to cope with dire situations. Once such organizations were formed, the elite achieved power over the common population and used the city as a power base and as a means to communicate that power” (ibid.). This is plausible as far as it goes, but in his brief theoretical remarks Herzog does not comment on the question of symbolic homology versus symbolic disjunction between city and village; i.e., the issue of an urban-rural dichotomy.

Furthermore, in Wattenmaker’s model and in the similar prestige-economy models proposed by archaeologists working elsewhere, the political changes that lead to an increase in social scale are thought to precede craft specialization, because increasing scale is what creates the demand for prestige items (see Earle 1987). Older “commercial development” and “adaptationist” models of specialization, exchange, and sociopolitical complexity have increasingly been rejected in favor of “political” models (see Brumfiel and Earle 1987; Stein 1999a). Specialized production may well be an externally observable effect of prior political changes and associated symbolic innovations, but it is not systemically related to political phenomena in an overarching causal framework in which “real” economic relationships generate and maintain “ideological” political hierarchies. Rather, it is ideology itself (and thus hermeneutical understanding of the meanings of social actions, including
Among hermeneutically sensitized anthropologists, however, there is a tendency to reintroduce objectivist systemic metaphors which capture within a familiar positivist perspective the new emphasis on the social meanings of material objects. Thus Jane Schneider (1977) speaks of the “world economic forces” and “energy flows” mediated by luxury exchange, and her goal is “to discover laws which apply to pre-capitalist as well as capitalist social change” (p. 27). Likewise, Timothy Earle (1987) relates the circulation of prestige goods to a protocapitalist system of “wealth finance.” Earle’s concept of “wealth finance” involving prestige goods (as opposed to “staple finance” involving redistribution of subsistence commodities) assumes the existence of a retail market in foodstuffs in which luxury items serve as a medium of indirect exchange; in effect, money. But the symbolic ideological preconditions of depersonalized market exchange are not considered in his model; in particular, the role of formal rationality (see chapter 4.4 above). The movement from staple finance to wealth finance (i.e., from redistribution to market exchange, in Polanyi’s terms) is not a “natural,” systemic outcome of the specialization brought about by the prior development of political stratification, operating without regard to the symbolic ordering of society. To posit an automatic, cross-culturally applicable systemic link between prior political stratification and subsequent economic effects in this manner simply reverses the direction of causality of older adaptationist models, as Earle (1987:67) himself acknowledges, producing an equally objectivist causal model. But the ideological structure of market exchange itself makes it difficult to generalize about where and when that sort of exchange will be effective on a broad scale, permitting us to use terms like “finance” and “market.” What is clear, however, is that the existence of a depersonalized financial “system” cannot be assumed but must be demonstrated in each case.

More generally, archaeologists such as Kent Flannery and Joyce Marcus (1993) reject the processualist view that ideas are epiphenomenal, but they continue to make a questionable distinction between “cognitive” social phenomena (which they in turn separate rather too neatly into the distinct spheres of cosmology, religion, political ideology, and iconography) and “subsistence-settlement behavior”—as if such behavior is an underlying layer of “real” life that does not itself embody symbolically mediated practices and so is amenable to a nonhermeneutical systemic analysis. But the danger of depersonalized systemic models is that they tempt us to overvalue material exchange, whether of subsistence commodities or prestige goods, to the extent that we neglect the importance of intangible and unpredictable political assets such as rhetorical skill, physical beauty and strength, strategic acumen, diplomatic subtlety, and a sense of timing—the components of a leader’s personal charisma, which is communicated and celebrated (or perhaps even manufactured) by political followers via language, in gossip, story, and song. When what counts is “symbolic capital” there is no simple correlation between material production, even of prestige items, and the establishment and maintenance of centralized political authority.
Chapter 10. Canaanite Feudalism as Seen from Ugarit

Ugarit was a small kingdom on the Mediterranean coast of north Syria ruled from the city of that name (see the maps in figures 16 and 17 below). It flourished during the Middle Bronze Age (ca. 1900–1500 B.C.) and the Late Bronze Age (ca. 1500–1180 B.C.). The mound site called in modern times Ras Shamra, where the ancient city of Ugarit was located, was inhabited even earlier, during the Neolithic, Chalcolithic, and Early Bronze periods, but little is known about these earlier phases of occupation. In any case, it seems that the kingdom and city of Ugarit as we know them in the Middle and Late Bronze Ages were founded early in the second millennium by West Semitic-speaking “Amorite” rulers who had migrated from the region of the Euphrates River in inland north Syria. It is not clear how rapid this migration was or in how many stages it occurred, but some sort of migration is the most plausible explanation for the founding of numerous similar kingdoms or city-states along the eastern coast of the Mediterranean early in the Middle Bronze Age, from Alalah on the Orontes River in the north to Ashkelon in the south, on the coast of southern Palestine.

Most of these small kingdoms were ruled from a strongly fortified capital city which was the center of royal power and featured a large palace and one or more temples. Available textual evidence indicates that most of their rulers had West Semitic names and spoke one dialect or another of the language family traditionally called “Northwest Semitic” (i.e., the Levantine branch of the “Central” branch of West Semitic; see Huehnergard 1995). To this linguistic similarity correspond archaeologically observed similarities in architectural and ceramic styles and in cultural and mortuary practices. Finally, the striking resemblance between the mythology and religion of Ugarit and what we know of the mythology and religion of Syria-Palestine from the Hebrew Bible and a few other textual sources indicates a basic cultural uniformity along the eastern Mediterranean seaboard.

1. Who Is a Canaanite?

This well-attested complex of cultural features in the second-millennium Levant calls for a name, and there is a long-established convention of referring to it as “Canaanite.” In this sense, then, Late Bronze Age Ugarit is a prime example of “Canaanite” society. It is true that, strictly speaking, Late Bronze Age Canaan as a geopolitical entity did not extend north of Lebanon, because it was coterminous with the Egyptian imperial domain in Asia, as Anson Rainey (1996) has emphasized (see also Na’aman 1999). Likewise, in linguistic terms the Canaanite languages of the southern Levant must be distinguished from Ugaritic, the Northwest Semitic language spoken in Ugarit and perhaps elsewhere in north Syria. But there is no harm in using the term Canaanite more loosely to denote the West Semitic culture of coastal Syria and Palestine in general.

Undoubtedly, there were linguistic and cultural differences within this region, differences that might well have become more pronounced over the course of the second millennium. Moreover, Ugarit and its neighbors in the north, which became part of the Hittite empire after 1340 B.C., had a political experience during the Late Bronze Age that differed from that of the Egyptian-controlled kingdoms in the south (see table 11 below and the historical overview in Liverani 1979c). The northern kingdoms were also larger, on average, than those in Canaan proper. During the Hittite period the king of Ugarit, for example, controlled roughly 2,000 km², a territory measuring approximately 70 km from north to south and 30 km from east to west; and the kingdom of Ugarit had as many as 150 secondary towns and villages (see van Soldt 1996; 1997; 1998). Most scholars would agree, however, that the various Levantine city-states of the Middle and Late Bronze Ages were alike in their basic social and economic organization. It would perhaps be more accurate to refer to them collectively as “Amorite” or “second-millennium Levantine,” but the features they had in common deserve recognition in a convenient term, which in this book is “Canaanite” (in this vein, see also Pardee 2001).

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1 The evidence for earlier occupation comes from a few deep soundings dug by the excavators of Ras Shamra (see Contenson 1992). The vast majority of archaeological finds at the site date to the Middle and Late Bronze Ages, and especially to the final phase of the Late Bronze Age in the early twelfth century B.C.

2 There is no reason to doubt the old hypothesis of an Amorite migration from inland Syria to the Mediterranean coast and then southward to Palestine. Linguistic evidence points to a third-millennium homeland for West Semitic speakers in the semiarid steppe region surrounding the middle and upper Euphrates (Buccellati 1992; but cf. Gelb 1961). Whatever the geographic origin of the West Semitic languages, key elements of second-millennium urban Levantine material culture can be traced to Early Bronze Age antecedents in inland Syria, indicating that this is the most proximate origin of the “Amorites” as a Middle Bronze Age political phenomenon along the Mediterranean coast.
Figure 16. Map of Syria and Upper Mesopotamia in the Bronze Age
Figure 17. Map of the kingdom of Ugarit (after Yon 1997:21, fig. 6)
Table 11. Major Events Affecting the History of Ugarit

<table>
<thead>
<tr>
<th>Period</th>
<th>Event</th>
</tr>
</thead>
<tbody>
<tr>
<td>ca. 1900–1800</td>
<td>Amorite dynasty established in Ugarit; construction of fortifications, palace, and temples</td>
</tr>
<tr>
<td>ca. 1800–1600</td>
<td>Domination of Alalah and much of north Syria by the Amorite “great kingdom” of Yamhad based</td>
</tr>
<tr>
<td></td>
<td>in Aleppo; Ugarit probably remained independent</td>
</tr>
<tr>
<td>1776–1761</td>
<td>Reign of Zimri-Lim of Mari; Ugarit mentioned in texts found in palace at Mari</td>
</tr>
<tr>
<td>16th cent.</td>
<td>Mittanian domination of much of north Syria; spread of Hurrian influence</td>
</tr>
<tr>
<td>1504–1492</td>
<td>Reign of Thutmose I of Egypt; at least one Egyptian campaign in north Syria</td>
</tr>
<tr>
<td>1492–1479</td>
<td>Reign of Thutmose II of Egypt</td>
</tr>
<tr>
<td>1479–1425</td>
<td>Reign of Thutmose III of Egypt; Egyptian conquest of Palestine and Syria</td>
</tr>
<tr>
<td>1428–1397</td>
<td>Reign of Amenhotep II of Egypt; campaigned in north Syria in his 7th year</td>
</tr>
<tr>
<td>1397–1388</td>
<td>Reign of Thutmose IV of Egypt</td>
</tr>
<tr>
<td>1388–1351</td>
<td>Reign of Amenhotep III of Egypt</td>
</tr>
<tr>
<td>1351–1334</td>
<td>Reign of Amenhotep IV (Akhenaten) of Egypt</td>
</tr>
<tr>
<td>mid-14th cent.</td>
<td>Reign of Ammittamru I of Ugarit (precise dates unknown)</td>
</tr>
<tr>
<td></td>
<td>Ugarit mentioned in several letters found at el-Amarna (ancient Akhetaten)</td>
</tr>
<tr>
<td>1344–1322</td>
<td>Reign of Šuppiluliuma I of Hatti</td>
</tr>
<tr>
<td>? – 1315</td>
<td>Reign of Niqmaddu II of Ugarit</td>
</tr>
<tr>
<td>ca. 1340</td>
<td>Hittite conquest of north Syria under Šuppiluliuma I; Niqmaddu II of Ugarit sides with</td>
</tr>
<tr>
<td></td>
<td>the Hittites against Mukиш and Nuhhašše and is rewarded by the enlargement of his territory</td>
</tr>
<tr>
<td></td>
<td>at the expense of Mukиш</td>
</tr>
<tr>
<td>1321–1295</td>
<td>Reign of Muršíli II of Ḫatti</td>
</tr>
<tr>
<td>1315–1313</td>
<td>Reign of ḡArhalba of Ugarit (who apparently rebelled against the Hittites)</td>
</tr>
<tr>
<td>1313–1263</td>
<td>Reign of Niqmepa of Ugarit (installed by Muršíli II when ḡArhalba was deposed)</td>
</tr>
<tr>
<td>1295–1272</td>
<td>Reign of Muwattalli II of Ḫatti</td>
</tr>
<tr>
<td>1279–1213</td>
<td>Reign of Ramesses II of Egypt</td>
</tr>
<tr>
<td>1274</td>
<td>Egyptian-Hittite “Battle of Qadeš” under Ramesses II and Muwattalli II, eventually followed</td>
</tr>
<tr>
<td></td>
<td>by a treaty (under Hattušili III) confirming imperial boundary in S. Syria</td>
</tr>
<tr>
<td>1272–1267</td>
<td>Reign of Urhi-Tešub of Ḫatti</td>
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<tr>
<td>1267–1237</td>
<td>Reign of Ḫattušili III of Ḫatti</td>
</tr>
<tr>
<td>1263–ca. 1220</td>
<td>Reign of Ammītamru II of Ugarit</td>
</tr>
<tr>
<td>1237–1209</td>
<td>Reign of Tudhaliya IV of Ḫatti</td>
</tr>
<tr>
<td>ca. 1220–1210</td>
<td>Reign of ḡIbirānu of Ugarit</td>
</tr>
<tr>
<td>ca. 1210–1195</td>
<td>Reign of Niqmaddu III of Ugarit</td>
</tr>
<tr>
<td>ca. 1208–?</td>
<td>Reign of Šuppiluliuma II of Ḫatti</td>
</tr>
<tr>
<td>ca. 1195–1180?</td>
<td>Reign of ḡAmmurapi of Ugarit</td>
</tr>
<tr>
<td>1183–1152</td>
<td>Reign of Ramesses III of Egypt</td>
</tr>
<tr>
<td>early 12th cent.</td>
<td>Destruction and abandonment of the city of Ugarit due to Sea Peoples invasion</td>
</tr>
<tr>
<td></td>
<td>Breakup of the Hittite empire and demise of its capital Ḫattuša</td>
</tr>
<tr>
<td>1175</td>
<td>Egyptian battle against invading Sea Peoples in the eighth year of Ramesses III</td>
</tr>
</tbody>
</table>


Note: There is still much debate about the absolute chronology of Middle Bronze Age Mesopotamia and Syria. Although there are arguments in favor of a “low” chronology (see Gasche et al. 1998), for the sake of convenience I have adopted the conventional “middle” chronology here, placing the reign of ḡAmmurapi of Babylon at 1792–1750 B.C. Either way, the Late Bronze Age dates are unaffected.
Figure 18. The site of Ugarit (Ras Shamra) showing excavation areas (after Callot 1994:231, fig. 1)
2. The City of Ugarit and Its Archives

Since 1929, teams of French archaeologists have conducted many seasons of excavation at the site of Ras Shamra, ancient Ugarit, near the modern city of Latakia on the coast of northern Syria, due east of Cyprus. The site is more than 20 hectares in area and at its highest point rises 20 meters above the surrounding plain. A quarter of the present surface of the site has been excavated, revealing a large palace, two temples, and extensive residential areas (see figure 18 above). The earlier architectural phases of the site are not well known because the excavators, although they have uncovered wide areas, have probed deeply in only a few places. Still, it is clear that the city of Ugarit flourished throughout the second millennium until its destruction at the hands of the Sea Peoples, after which it was never rebuilt. Many buildings, notably the major temples, were founded during the first half of the second millennium, although they underwent renovation thereafter. In their present form, however, most of the architectural remains and other artifacts so far recovered from the site (including the inscriptive material) date to the Late Bronze Age—in many cases, indeed, to the last days of Ugarit at the beginning of the twelfth century B.C.

Ugarit is justly famous for its archives of clay tablets inscribed in both syllabic and alphabetic cuneiform scripts. Some 1,800 syllabically written texts and fragments—mostly Akkadian and Sumerian, although Hurrian and Hittite are also represented—were known as of 1986 (see TEO 1 and 2). Even more numerous are the texts written in Ugaritic, the local Northwest Semitic language, using an indigenous consonantal alphabetic script. More than 1,900 Ugaritic tablets and fragments were discovered up to 1986. A number of Akkadian and Ugaritic tablets have also been found at a neighboring site located on a small promontory, Ras Ibn Hani, four kilometers southwest of Ras Shamra, and approximately 400 new texts (ca. 80% Akkadian) were discovered from 1988 to 1999 in the “House of ‘Urtēnu,” in a newly excavated area in the southern part of the tell (Yon 1995; 1997:95ff.; Malbran-Labat 1995).

The syllabic and alphabetic tablets differ quite markedly with respect to the types of texts represented in the two scripts. Legal documents are almost all written syllabically, in Akkadian. Letters, too, are more common in the syllabic corpus, as might be expected given the role of Akkadian in international correspondence (although for letters known to have been written by natives of Ugarit the proportions are reversed: ca. 80 Ugaritic letters against fewer than 20 in Akkadian). Economic or administrative docu-

ments, on the other hand, were more likely to have been written in the alphabetic script. Approximately 200 Sumero-Akkadian economic texts have been discovered, but the total of Ugaritic economic texts and fragments is now approaching 1,000, including those from recent discoveries—by far the largest single category of alphabetically written documents. The native myths and rituals, numbering in excess of 170 tablets and fragments, were written almost exclusively in Ugaritic.3

The texts from Ugarit are not dated by the regnal year of the current king or by any other device. Some chronological information, however, is contained in legal texts documenting acts of the reigning king or transactions witnessed by him. The kings of Ugarit themselves can be assigned absolute dates with the aid of synchronisms derived from international letters, edicts, and treaties in the archives that name rulers of neighboring lands, especially Hittite kings (see table 11 above). Many administrative texts in turn can be dated in relation to the letters and legal texts (very roughly, to be sure) on prosopographic or stratigraphic grounds. The dating of the texts from Ugarit has been thoroughly discussed by Wilfred van Soldt in a lengthy work published in 1991. None of the texts that have come down to us appears to have been written earlier than ca. 1340 B.C., when Hittite rule began at Ugarit.4 The chronological limit on the other side lies around 1180 B.C., when the texts and the city itself came to an end. The tablets found at Ugarit therefore span a period of approximately 160 years. But there is reason to believe that the letters and the economic texts (both syllabic and alphabetic), in particular, should be confined to a much shorter period, perhaps the last fifty years of the life of Ugarit, from 1230 to 1180 (van Soldt 1991:230f.). Unlike the legal, literary, and lexical texts, there was appar-

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3 Texts are listed by genre in TEO 2 and in CAT/KTU (for Ugaritic alphabetic texts). Approximate percentages are as follows: (1) syllabic—economic, 22%; legal, 39%; letters, 16%; scholarly, 16%; literary and religious, 4%; other, 3%; (2) alphabetic—economic, 68%; legal, 1%; letters, 7%; scholarly, 2%; literary and religious, 15%; sealings, labels, and inscribed objects, 7%.

4 It has often been assumed that the extant literary and religious texts are copies of much older compositions, but there is no direct evidence for this, as I have been reminded by my colleague Dennis Pardee. Indeed, these “copies” themselves may be much younger than has been supposed, because the scribe ‘Illimilkhu whose name appears on a number of mythological tablets probably lived during the reign of Niqmaddu III (ca. 1210–1195) as opposed to the reign of Niqmaddu II in the fourteenth century (Pardee 1997b:241, n.3; cf. van Soldt 1991:28f.).
ently no reason to keep letters or administrative records for long periods after they were written.

In addition to his work on dating, van Soldt has analyzed the spatial distribution of the texts found at Ugarit. This sort of investigation is possible because over the years the excavators recorded the findspots of most of the tablets. This information, along with brief descriptions of the texts, was made available in a full and corrected form in a catalogue that appeared in 1989 (Bordreuil and Pardee 1989 = TEO 1). Happily, too, almost all of the tablets were recovered during controlled excavations; very few were purchased on the antiquities market. Even before the publication of the full catalogue van Soldt was able to undertake a detailed study of the locations of the texts in relation to their contents and dates. By and large his findings confirm the opinions of the original excavator, Claude Schaeffer. Most of the tablets fall into one of 16 major collections on the basis of their findspots. Six of these archives were found in various places in the royal palace—a huge building of 90 rooms, about a hectare in area, situated near the northwestern corner of the site. The palace archives are differentiated by genre, more or less, as Schaeffer observed long ago (see, e.g., PRU 3:xi–xxx). The so-called Western, Eastern, and Southwestern archives of the palace contain mostly economic documents, with very few legal texts. Letters are concentrated in the Eastern archive, international treaties and legal documents in the Southern archive, and domestic legal texts in the Central archive. An important group of Ugaritic letters and economic texts, about 75 in number, was discovered in what the excavators took to be the remains of a kiln in a courtyard of the palace. It was long thought that these tablets were being baked when the city was destroyed; if that were so, they could be dated to the last days of Ugarit around 1180 B.C. More recently, however, the current team of excavators at Ras Shamra has concluded that there was no kiln in the courtyard and any baked tablets found at the site were fired accidentally during the destruction of the city (see Calvet 1990:39f.; Margueron 1995; Yon 1997:50, 54f.).

On the whole, it may be said that the palace archives contain most of the legal and administrative documents found at Ugarit, but by no means all of them. In certain private houses such texts are quite numerous. In fact, the excavators have so far discovered at least nine “private” archives at Ugarit, most of them apparently in the houses of high officials or cult personnel (van Soldt 2000; 1991:226f.). Some of the private archives contain letters and legal documents, and almost all of them contain economic texts similar to those found in the palace. Indeed, economic texts usually constitute a substantial fraction of the whole archive, ranging from 10% to 30% of the tablets, although the absolute number of such texts is not large—fewer than 20 in most cases. In one large house, however, 86 economic texts were found, forming 90% of the archive. But lexical, literary, and religious texts are even more common than legal and administrative documents within the private archives. This is in contrast to the royal archives where such genres are quite scarce.

If the locations of the tablets are any indication, then, it seems that the transmission of scribal and religious lore—and a considerable amount of mundane recordkeeping—took place outside the palace. Thanks to the excavation of extensive residential areas in widely separated parts of the site, we possess a more balanced picture of scribal and administrative activity at Ugarit than we otherwise would, were tablet finds limited to the palace. Although it has not yet been attempted, a detailed prosopographical analysis of the economic and legal texts found in various private archives, comparing them with texts found in the palace, may someday provide important insights into the social and economic structure of Ugaritian society.

From the preceding brief description of the site and its texts, it should be clear that the evidence from Ras Shamra, both textual and archaeological, constitutes a promising source for the sociohistorical reconstruction of Late Bronze Age Ugarit, and, by extension, of the Late Bronze Age Levant as a whole, for which Ras Shamra provides by far the largest body of textual data that we currently possess. Over the years, however, scholarly work has focused on the philological study of the tablets and on the literary and religious aspects of the native myths and rituals, especially in relation to their parallels in the Hebrew Bible and the religion of ancient Israel. Less attention has been paid to the social and economic organization of the kingdom of Ugarit. A few researchers have, nonetheless, made important contributions in this area, and it is my purpose in this chapter and the next to discuss their conclusions, the evidence on which they base them, and the underlying models that inform their treatments of the material. I will then be in a position to offer an alternate approach, formed in response to previous scholarly work and in light of

5 See van Soldt 1991:226f. for a summary of the frequencies of texts by genre and archive. Of texts found and published to date, the palace held 76% of the economic documents, 96% of the international treaties, 95% of the international legal texts, 85% of the domestic legal texts, 44% of the international letters, and 39% of the remaining letters.
important data, both archaeological and textual, that in my opinion have been ignored or misinterpreted, or have only recently become available.

A new approach is both possible and desirable at this time for several reasons. In recent years, first of all, there have been significant advances in the philological understanding of the texts. In addition to ongoing work on the Ugaritic language and lexicon, detailed studies of the Akkadian texts have been produced by John Hhuehnergard (1987; 1989) and Wilfred van Soldt (1991). These studies include discussions of the syllabary and grammar of the Akkadian dialect used at Ugarit along with many new or corrected readings. Moreover, the current team of researchers at the site has now published a complete listing of all of the epigraphic finds from Ras Shamra with their findspots and attendant bibliography, as mentioned above (TEO 1 and 2, supplemented by Dietrich and Lorentz 1996). These new aids to the study of the texts are complemented by renewed and improved publication of the nontextual finds at the site. The earlier excavation reports, prepared under the direction of Claude Schaeffer, unfortunately are deficient in several respects. For the excavations before 1978 no complete inventory of pottery or other small finds has ever appeared. Many of the published plans are schematic and lack sufficient detail; indeed, for many buildings no large-scale plans are available at all. Since the late 1970s, however, archaeological fieldwork and publication at Ras Shamra have improved greatly and a new body of archaeological evidence has appeared. This new evidence is particularly important for the social history of Ugarit because the current team of excavators, under the direction of Marguerite Yon, has focused its attention on domestic architecture, both in previously excavated areas and in newly opened residential areas in the center of the city and in the southwest quadrant (for an overview see Yon 1997). These archaeologists have begun to produce impressively detailed studies of house plans and of building techniques that reveal much about the daily lives of ordinary inhabitants of the city (Callot 1983; 1994; Yon et al. 1987a; 1991).

Previous work on the society and economy of Ugarit has tended to neglect the archaeological data, however. This can be excused, perhaps, because of incomplete publication of the finds. But the time is ripe to integrate the textual and nontextual evidence from the site in a new sociohistorical synthesis. In attempting such a synthesis I do not claim to have produced a detailed treatment of all of the data. Rather, I have focused on what I believe to be the central issue for students of Ugaritan society and other societies like it, namely, the structure and functioning of the royal service system, in which land was granted or rations issued in return for the provision of goods and services. Not only is this the best-documented aspect of life in Late Bronze Age Ugarit, given the bias in our sources toward texts and material culture from the capital city and the palace; the royal service system was also the fundamental economic and political institution integrating the inhabitants and resources of the entire kingdom. Of course, if we knew more about the internal organization of the rural settlements of the kingdom, in which much of the populace resided, there would be no question of ignoring village life in a social history of Ugarit. But in the absence of relevant textual data, and with a lack of excavation in the countryside around Ras Shamra, we are left with the textual and archaeological evidence from the city of Ugarit itself, which will be discussed in this chapter and in chapters 11 and 13.

Much of the textual evidence is dealt with in this chapter and the next, in the course of my critique of existing treatments of Ugaritan society. The household was the basic socioeconomic unit in Ugarit; furthermore, in my opinion, the overall organization of the kingdom is best understood according to the patrimonial household model (PHM) described above in chapter 3. This model also characterizes a number of other Bronze Age Near Eastern polities, as I argue in chapter 12. The kingdom of Ugarit, like its neighbors, was essentially a hierarchy of households-nested-within-households, with the royal household at its apex. I will note the various indications of this unitary hierarchical structure in the legal and administrative documents.

Ideally, the validity of this model would also be demonstrated by a prosopographical analysis that might show whether the heads of large private households had dependents who were householders in their own right, and whose relationship to their masters was analogous to that of the leading householders to the king. Unfortunately, however, prosopographical analysis of the Ras Shamra texts is greatly complicated by the frequent lack of patronyms and the absence of dates, which makes it difficult to match references to the same person in view.

6 Prosopography has been used to good effect by van Soldt (1991) in dating the archives, but it has been largely neglected in socioeconomic studies of Ugarit, even though it is a technique by which the cryptic and formulaic economic and legal texts, which are so common at Ugarit, might be made to yield more information. Astour (1972) made a beginning in this area in his study of the "merchant class of Ugarit," but his conclusions have not been generally accepted.
of the many potential homonyms. Detailed prosopography must therefore be reserved for a future study, although I will discuss the problems and possibilities of this kind of study below in a brief appendix to chapter 11.

The famous mythological texts of Ugarit must be considered, and I shall do so below in chapter 14 under the heading "The House of Ilu." Of course, the Ugaritic myths do not provide direct evidence of mundane social relationships or of the functioning of the royal service system, but they do reflect the native understanding of fundamental principles of social order, insofar as the plots and characters in these myths were meaningful to those who composed and recounted them. From the point of view of the hermeneutical method that I have adopted in this book, mythological symbolizations of the world provide important evidence of the basic structuring principles of society. I will argue that the Ugaritic myths reveal a patrimonial rather than a bureaucratic conception of political authority. Such texts are open to many interpretations, of course, and they cannot serve as the main source of evidence in support of a model of ancient society; but the patrimonial reading that I have adopted is not simply speculative because it is controlled by the evidence drawn from more mundane documents and from archaeological remains. Indeed, when read with the PHM in mind, the Ugaritic myths provide a striking picture of typical social relationships in a patrimonial society.

The remains of domestic architecture at Ras Shamra—long neglected even by archaeologists—also contain important clues concerning Ugaritian social organization. In chapter 13 I will interpret the archaeological data from Ugarit, making use of the Mediterranean ethnographic and historical analogies and architectural parallels discussed above in chapters 6 and 7, with the aim of relating house form to household structure. As has long been recognized, both the textual and artifactual evidence indicate that many of the king's service personnel, especially craftsmen and other professionals, lived in the city of Ugarit. Unfortunately, individual houses can be connected with persons named in the texts in only a few instances. Nonetheless, there are clear archaeological indications of what typical living arrangements were like for the people known to us from the economic texts, legal documents, and letters. We can approach the royal service system at Ugarit "from the ground up" by looking at the organization of living space and the disposition of shared facilities like wells, courtyards, tombs, and oil presses. From this information it is possible to draw conclusions about household size and structure that may be compared with ideas derived from the written documents. Indeed, the textual evidence is badly in need of reevaluation in light of the recent archaeological research into domestic architecture. Some scholars, for example, have argued that nuclear families were the norm among service personnel living in the city of Ugarit (e.g., Liverani 1979c:1320, 1344), which is in keeping with a widespread assumption about ancient urbanism still current in ancient Near Eastern studies (cf. Snell 1997:121). I believe that the archaeological evidence points in the opposite direction: the floor plans and dimensions of houses found at the site indicate that relatively large extended- or joint-family households were common. Moreover, what the domestic architecture reveals about the size and structure of the households of so-called palace dependents has important implications for our understanding of the royal service system as a whole.

As I have argued in Part One, any sociohistorical reconstruction involves the application of a conceptual model, whether explicit or implicit, to fragmentary evidence, and it is necessary to make one's model explicit and to defend its use in the case at hand. The exchange nearly two decades ago between Dominique Charpin and Norman Yoffee in the *Journal of the American Oriental Society* is informative on this score (Charpin 1980; Yoffee 1982). In my opinion, Charpin was right to decry premature or ill-founded attempts at sociohistorical synthesis; but I agree with Yoffee (1982:347) that "all interpretations of extinct societies are ultimately derived from analogies with extant or historical ones," a fact that necessitates, for the ancient Near East, the careful construction of heuristic models using comparative evidence. If one wishes to form a picture of a society, not just in its parts but as a whole (and how can one avoid it?), some sort of simplifying model or analogy will come to mind and historical understanding can only be helped by openly stating, testing, and refining

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7 This is not surprising since population estimates place up to a quarter of the inhabitants of the kingdom in the capital city (see, e.g., Liverani 1979c:1319f.). Archaeological evidence indicates that craftsmen of various kinds (metalworkers, most obviously) were concentrated in certain areas of the city, and, judging by the contents of their libraries, many cult personnel also lived in the city, especially in the neighborhood of the temples.

8 The term "extended family" has been defined in several ways (see Laslett 1972:28–32). In the present work I prefer the term "joint family," which refers to the members of a patrilocl household consisting of a conjugal couple and their unmarried children, together with their married sons and their wives and children (see chapters 6.3 and 7.1 above).
it. I do not think that Charpin has denied this, and his specific criticisms of Yoffee’s model and Yoffee’s use of the textual evidence concerning late Old Babylonian society are probably well founded (see the discussion of Old Babylonian society in chapter 12.4). But it is to Yoffee’s credit that he attempted a coherent overall description of the society he was studying, providing a framework into which data culled from disparate sources could be fitted and given meaning.

Indeed, a guiding principle of the present work is that ancient terminology pertaining to social roles and economic phenomena cannot be adequately understood on the basis of disconnected word studies without regard to the structuring principles of the society as a whole, but must be interpreted in light of an overall conceptual model of the ancient society—a model which itself has to be developed dialectically with reference to all available data on linguistic usage and etymology. In other words, our translations of ancient terms must not only be plausible philologically but they must make sense overall, in view of everything we know textually, archaeologically, and ecologically about the society in question and analogous societies elsewhere. In light of the importance of explicit model-building, I turn now to a critical examination of current scholarly models of ancient Near Eastern society and their application to the material from Ugarit. Later, in chapters 12–14, we can consider in more detail the archaeological and textual evidence that justifies a new approach to the society and economy of Ugarit.

3. The Feudal Model of Ugaritian Society

Scholars have adduced evidence from Ugarit in support of both the feudal model and the two-sector (or “Asiatic mode of production”) model. On the one hand, the land-grant documents appear to support a feudal interpretation. The king gives property on a hereditary basis to persons who owe him service in return—service of a type appropriate to their professions. On the other hand, ration lists and other records of disbursements from the palace, which mention many of the same persons found in the land-grant texts, appear to support the two-sector model, or at least the idea that there was a large group of royal dependents who constituted a single household headed by the king and directly supported by him. Furthermore, the texts seem to distinguish between these “men of the king” in the “palace sector” (Ug. bnš mlk, pronounced /bunuš malk/\, sg. /bunuš Malk/) and the inhabitants of rural agricultural villages in the “free sector.”

The evidence for and against these two models of Ugaritian society will be considered in some detail below, but it should be clear from the preceding discussion that the differences between the feudal model and the two-sector model revolve around the question of ownership. Who owned what, and what did ownership entail? How was “ownership of the means of production” related to royal service? In assessing various scholarly approaches to the society and economy of Ugarit and other ancient Near Eastern states, it is important to recognize that ownership can be a very complex phenomenon. In many societies there is a hierarchy of rights to any property—especially land— involving various degrees of control and use of the property by different persons. Robert Adams (1982:1) has criticized Diakonoff on this score, for his emphasis on “the concept of formal ownership of the means of production, and above all land, rather than the broad relationship of forces within the society at large.” Adams goes on to argue that:

The difficulty with the narrower concentration of interest is that the idea of property, no less in land than in anything else, is itself only to be understood as a historically conditioned social construct. Access to and control over property has always been conditioned by successive accretions of rights and obligations that may survive or be altered only slowly according to the at least partly autonomous dynamics of all conceptual systems. [ibid.]

Despite his lengthy discussion of the concept of property in the ancient Near East, Diakonoff fails to allow for a hierarchy of overlapping rights, arguing instead that a ruler, were he the “supreme proprietor” of land in his kingdom, could not buy, sell, or transfer real estate without losing all his rights to the land (see Diakonoff 1982:8–23). For Diakonoff, apparently, ownership of land in the ancient Near East was “all-or-nothing,” and it conveyed exclusive rights of use and disposition—a curiously modern conception.

The ersatz-feudal notion that the king “owned” all of the land also invites qualification, however, for clearly in this model the recipients of land grants, whether they were high officials or peasant villagers, acquired certain hereditary rights to property that, in practice at least, were not easily revoked. On the other hand, the rather rigid schematic distinction in the two-sector model between free and nonfree persons, distinguished by their ownership or lack of ownership of land, fails to allow for evidence that the king enjoyed certain rights over all property and personnel throughout his domain, including the “free” sector; and it ignores evidence that royal dependents had hereditary rights to property and a measure of freedom in its use and disposition—rights that, in
practice, make the traditional Marxist criterion of the "ownership of the means of production" rather artificial and beside the point. We will revisit this issue in the course of the following examination of existing interpretations of the society and economy of Ugarit, beginning here with the feudal model.

Albrecht Alt’s “Men Without Names”

One of the first to comment on the social structure of Ugarit was Albrecht Alt, the German historian and biblical scholar whose influence is still felt in research on the history of ancient Syria and Palestine. Alt did not produce a detailed study of Ugarit; indeed, most of the relevant texts had not yet been published at the time he was writing. But in 1950 he published an interesting article entitled “Menschen ohne Namen” (Alt 1959b) in which he tried to explain the frequent use of the construction *bn PN* (“son of PN”) in administrative lists from Ugarit. Alt also published a study of the role of the high official known as the *skn* (pronounced /sákinu/) at Ugarit (Alt 1959a), and a series of brief articles containing observations on other aspects of the legal and administrative texts from Ugarit (and from Alalah), focusing especially on the terminology used to describe various occupational groups (Alt 1954–66). But his overall approach to the society of Ugarit is best indicated by the article on “Menschen ohne Namen.” Here he argued that the designation “son of PN,” when it stood by itself without the person’s own name, referred to the current holder of a hereditary office originally granted to PN; in other words, it referred to an heir of PN, however distant, who was known to the palace administration only as a descendant of the original recipient of the office. Those who were designated in the same lists by their own names, and not as the descendants of someone else, were (Alt suggested) *hominen novi*—recent recipients of a new hereditary office who had not yet died and passed it on to an heir. Alt maintained that the use of the “son of PN” designation, which is found not just in Ugarit but also in the Amarna texts and in the Hebrew Bible, was evidence of what he called “die alten kanaanäischen Institutionen des erblichen Königsdienstes bestimmter Geschlechter in besonderen Berufen” (Alt 1959b: 212).

Subsequent publication of additional administrative lists has prompted revision of Alt’s thesis (see Heltzer 1982:12). It is clear now that an officeholder could be designated variously by his own name alone, by his own name in conjunction with a patronym, or by a patronym alone. The latter two possibilities, in particular, are demonstrated by comparing four texts—one syllabic and three alphabetic—in which the same persons (in this case, priests) are listed:

Here the alphabetic texts use the “son of PN” construction only, but in the syllabic text, for some reason, the format is “PN son of PN2.”

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<tr>
<td><strong>SANGA</strong></td>
<td><strong>kknm</strong></td>
<td><strong>bn.amdn</strong></td>
<td>**bn am</td>
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<tr>
<td><strong>IR-du DUMU am-ma-da-na</strong></td>
<td><strong>bn.amdn</strong></td>
<td>**bn.am</td>
<td>nyn**</td>
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<tr>
<td><strong>IR-DINGIR-ti DUMU ka-bi-iz-zi</strong></td>
<td><strong>bn.dtn</strong></td>
<td>**bn.pzn</td>
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<td><strong>ia-an-ha-ama-mu DUMU pi-zu-ni</strong></td>
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<td><strong>bn quant</strong></td>
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<tr>
<td><strong>ku-an-um-mu DUMU ni-ga-la-a</strong></td>
<td><strong>bn.amdn</strong></td>
<td><strong>bn.amdn</strong></td>
<td>**bn am</td>
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<tr>
<td><strong>a-gap-SÉS DUMU ku-ni-yi</strong></td>
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<td><strong>bn.mglb</strong></td>
<td>**bn mgl</td>
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<tr>
<td><strong>ia-bur₂-ša-nu DUMU »NA» ma-ag-li-bi</strong></td>
<td>**bn.sn</td>
<td>n**</td>
<td>**bn sn</td>
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<tr>
<td><strong>SUM- U DUMU si-na-ra-na</strong></td>
<td><strong>bn.tgd</strong></td>
<td><strong>bn.tgd</strong></td>
<td><strong>bn tgd</strong></td>
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<tr>
<td><strong>ia-an-ha-ama-mu DUMU ši-gu-di</strong></td>
<td>**bn.sn</td>
<td>n**</td>
<td>**bn sn</td>
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Here the alphabetic texts use the “son of PN” construction only, but in the syllabic text, for some reason, the format is “PN son of PN₂.” There is no sys-

9 Comparisons between these four texts have been made by Heltzer (1982:13ff.), as well as by van Soldt (1991:34). The arrangement I have adopted follows that of van Soldt, except that I have included only part of the complete list of priests. The “RS” Ras Shamra excavation numbers are those of *TEO* 1, as will be the case in all other text citations in the present work. For alphabetic texts, the lineation is that of *KTU.*
the actual father of the person named and not to a more remote ancestor, as Alt had suggested. This is confirmed by comparisons between patronyms found in the cryptic alphabetic lists and the full names of the same persons in syllabic legal texts in which the filiations are clear. Van Soldt (1991:40ff.) has been able to reconstruct more than a dozen three-generation genealogical sequences using the filiations recorded in both syllabic and alphabetic texts.

Obviously, the formula “son of PN” did not have the significance Alt gave it, because it does not refer to a distant ancestor who founded a hereditary line of royal servicemen but simply identifies a man by his patronym. Nonetheless, Alt was correct to emphasize the hereditary nature of specialized occupations at Ugarit. For example, in an account of silver amounts (KTU 4.69 vi:23ff., one of the texts cited above; no. 1 below) there are three successive entries, the first for a man called bn ḫy, who heads the list of priests, the second for his heir or heirs (w.nḥlh), and the third for their heir or heirs (w.nḥlh lm). Note that the construction bn PN w.nḥlh, “son of PN and his heir(s),” with the singular pronominal suffix -ḥ, indicates that bn is singular. In other words, a typical list entry refers to the “son of PN,” not the “sons of PN.” On the other hand, w.nḥlh, “and his heir(s),” can be plural, as might be the case in the entries in which w.nḥlh is followed by a reference to the third generation, w.nḥlh lm, “and their heir(s),” using the plural pronominal suffix -hm (unless “their heir” simply refers to the father and grandfather).

In any case, it is clear that members of two and sometimes three generations within the same patrilineal family were serving simultaneously. Indeed, in a different section of the same account text four generations are listed in one instance (bn.kzn w.nḥlh w.nḥlh lm w.nḥlh in KTU 4.69 ii:20ff.). Nor was hereditary office confined to the priesthood. Heirs are mentioned, here and there, after the names of men practicing several different occupations, as the following examples demonstrate (compare also texts that mention the bn mṛyhn, “sons of the charioteers,” along with the senior mṛyhn and various other professions; e.g., KTU 4.137:4; 4.163:8; 4.173:5):

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<td>1. kḥmn (“priests”)</td>
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<td>RS 11.715+ vi:22ff. (KTU 4.69 vi)</td>
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<td>kḥmn</td>
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<td>bn ḫy</td>
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<td>w.nḥlh</td>
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<td>w.nḥlh lm</td>
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<td>bn.ḥly</td>
<td>4</td>
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<td>bn.ṣmrn</td>
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<td>2. qdšm (“temple/cult personnel”)</td>
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<td>RS 18.251 ii:8ff. (KTU 4.412 ii)</td>
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<td>qdšm</td>
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<td>bn[ ] ṣḥ[l]</td>
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<td>bn[ ] ṣ[l]</td>
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<td>3. ḥṛš bhtmn (“house-builders”)</td>
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<td>RS 8.183+8.201 i:16ff. (KTU 4.35 i)</td>
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<td>ḥṛš bhtmn</td>
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<td>20</td>
<td>ṣḥyn nḥlh</td>
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<td>nḥmn bn ḫln</td>
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<td>4. ḥršm (“craftsmen”)</td>
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<td>RS 15.51 (KTU 4.155)</td>
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<td>ṣpr ḥršm</td>
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<td>ṣḥy bn ṣqqln</td>
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<td>06 U.NIGIN2 KUBABBAR 86</td>
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</table>
5. hrš qšt (“bow-makers”)

**RS 16.130 (KTU 4.215)**

1. spr.hrš
2. qšt iql
3. bn.anny
4. 1šdq
5. ypltn.bn iln
6. špsm.nḥḥ
7. lptg

6. pslm (“sculptors”)

**RS 18.251 iii:9ff. (KTU 4.412 iii)**

7. pslm
8. bn.annnd
9. bn.šk’d
10. w.nḥḥ
11. bn.mkky
12. [b]n.bn[m]lt

7. yšhm (“?“)

**RS 20.145 (obverse) (KTU 4.692)**

1. abdy.yšhm
2. šd.bn.krmt.l.atrn
3. šd.bn.tan.l.ilyyn
4. šd.bn.krmt.l.bn.anlb
5. šd.bn.mdd.l.mznt
6. w.sh.nḥḥ.l.gmrd
7. šd.bn.kwn.l.(gm)[r]ld

8. cdbm (“servants”)

**RS 8.183+8.201 i:2ff. (KTU 4.35 ii)**

2. [cdbm
3. [b]n.mryn
4. arsvn
5. cb’dl
6. w.nḥḥ
7. atn.bn.ap[s?]n

9. hršm (“plowmen”)

**RS 11.602 (KTU 4.65)**

1. hḥrtn[

2. bn.tmq[
3. bn.ntp.[
4. bn.lbnm.[
5. ady.b[
6. [x]bsn.lm[l[
7. bn.atn.b.m[r]l[
8. bn.shr.mr[
9. bn.idrm.ṣ[x]
10. bn.bly.mr[
11. w.nḥḥ.mr[
12. 1šps.[
13. ilhny.[
14. bn.[

10. mrmm (“commanders?”)

**RS 11.715+ iii:11ff. (KTU 4.69 iii)**

1. mrmm
2. bn.ṭhr1
3. bn.adty
4. bn.krwn
5. bn.ngšk
6. bn.qnd
7. bn.pity
8. w.nḥḥ
9. bn.rt
10. bn.ḥr1
11. bn.ṣk[ ]

. . .

šu.nigin, kū.babbar[ ] 1 me-at 30+[x]

11. tnm (“archers?”)

**RS 8.183+8.201 ii:11ff. (KTU 4.35 ii)**

1. tnm[
2. bn.qqln
3. w.nḥḥ
4. w.nḥḥm
5. bn.shl
6. bn.brzn
7. bn.ḥdlr[
8. bn.yd[
9. bn.ṣ[w]
10. w.nḥḥm
11. w.nḥḥlm
12. bn.k[
13. bn.y[
14. fnm.l[i]

. . .
12. *mryn* (“charioteers”)

<table>
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<tr>
<th>RS 11.715+ i:1ff.</th>
<th>RS 11.858:7–19</th>
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<tbody>
<tr>
<td>(KTU 4.69 i)</td>
<td>(KTU 4.103)</td>
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<tr>
<td>1 <em>mryn</em> m₁</td>
<td>[ub] l<em>tl</em></td>
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<td>2. bn bly</td>
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<td>3. nnn</td>
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<td>4. w.nhlh</td>
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<td>5. bn rmyy</td>
<td>2</td>
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<td>6. bn tlmyn</td>
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<td>7. w.nhlh</td>
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<td>8. w.nhlhm</td>
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<td>10. bn adln</td>
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<td>11. bn sbl</td>
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<td>12. bn hnr</td>
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<td>13. bn arwt</td>
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<td>15. bn pghn</td>
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<td>19. bn prsn</td>
<td>10</td>
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<td>20. bn rhpj</td>
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<td>21. [š]l*bhr pgb</td>
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<td>22. [š]l*by</td>
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<td>23. [š]l*ly</td>
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<td>24. [š]l*pshyn</td>
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<td>25. [š]l*bdlmlk</td>
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<td>26. [š]l*ybn</td>
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<tr>
<td>27. [š]l<em>bn l</em>ndlh</td>
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... oter 4 me-at 87 š*a š*IŠU.NIGIN, KUBBAB neš | sa ša [š]U š*IŠU |mar-ia-ne

Notice that the senior man’s descendants usually remain unnamed, being described simply as *nlh*, “his heir(s),” or, in the third generation, *nlhm*, “their heir(s).” But there are also entries of the form “PN *nlh*” (“PN his heir”)—see no. 3, KTU 4.35 i:20, 22; no. 4, KTU 4.155:12; no. 6, KTU 4.215:6; and no. 11, KTU 4.35 ii:20, 21. In these cases, of course, no patronym is given for the heir since his father is named in the preceding line. ¹⁰ In either form, the references to heirs of occupational specialists prove not only the hereditary character of these professions, but also the fact that a man and his son(s), and even his grandson(s), could practice the same profession at the same time, and thus received separate (but contiguous) entries in lists of personnel and in royal accounts of receipts or disbursements.¹¹ This fits nicely with the archaeological evidence I will present in chapter 13, which indicates that, in a typical household at Ugarit, a man and his adult sons lived and worked together, with their wives and children, as a coresident joint family. What happened after the *paterfamilias* died is revealed, perhaps, by no. 11a (KTU 4.35 ii:12–15), in which four *rn* (archers?), all sons of the same man, *qrln*, are named individually rather than being designated simply as “heirs” collectively and anonymously in a single entry.¹² Presumably, they became heads of their own households—a situation reflected in no. 11b (KTU 4.66:2–4), in which a man called *bn qrln*...
(perhaps one of the four brothers who appear in no. 11a) is listed with his unnamed heirs and their heirs.

Not every profession known at Ugarit is included in the texts cited above, but the wide variety of occupations for which heirs are attested, ranging from plowmen to charioteers, suggests that hereditary service was widespread if not universal. This is even more likely if we consider how numerous are the other administrative documents in which heirs are mentioned but in which the profession is not indicated or is broken off. Furthermore, the heritable nature of royal service is shown by the formula found in many of the Akkadian land-grant documents, discussed in more detail below, in which the king gives property, in return for service, “to PN and to his sons (and to their sons) forever.”

Much of this evidence was unavailable to Alt in 1950, but he emphasized correctly, nonetheless, the hereditary principle at work in the royal service system of Ugarit, and he drew interesting parallels to similar phenomena in Canaan and Israel. In keeping with the habits of historical scholarship at the time, he made use of feudal terminology to describe the hereditary service system, referring to the land granted to royal servants as Lehensgüter, “feudal property” (Alt 1959b:208). Despite his implicit acceptance of the feudal model, however, Alt did not press the analogy very far in his writings on Ugarit.

John Gray and the Knights of Canaan

The tendency toward the use of feudal terminology to describe Ugarit, already evident in the work of Alt, emerged in full force in 1952 in two brief articles by a British scholar, John Gray (1952a; 1952b). Echoing prevailing opinions, Gray associated the feudalsm of Late Bronze Age Ugarit with an influx of Indo-Aryan and Hurrian charioteers from the north and east that took place in the first part of the second millennium B.C. An earlier Semitic “tribal” system (he thought) was detectable in the old Ugaritic myths and “sagas,” especially the story of King Kirta (or Keret); but according to Gray, the administrative texts showed that by the fourteenth century, tribalism had been supplanted in favor of military feudalism centered on the maryannu—the class of “maryannu” chariot-warriors—and augmented by a “guild system” encompassing the other professions.

In subsequent work, Gray (1966; 1969) repeated these ideas and did not substantially alter his views. But his description of Ugaritian social structure borrows uncritically from medieval European feudalism. There is no evidence, first of all, that the maryannu-charioteers were the dominant class at Ugarit, akin to the equestrian nobility of feudal Europe. In administrative texts they appear simply as one occupational group among others. It is true, as Gray (1952b:51f.) observed, that the maryannūma appear first in lists of professions, and in one lengthy account tablet (KTU 4.69 i, ii, iii:1–5; no. 11a above) they are the most numerous group and collectively pay or receive the largest amount of silver. But even if the maryannūma were wealthier or more numerous than most other occupational groups and enjoyed higher social status, there is no evidence that they completely dominated Ugaritian society. If it is pressed too far, the feudal analogy gives a false impression of a society controlled by a military elite who owned most of the land (which was worked for them by peasant serfs) and who overshadowed lesser men among the cult personnel, merchants, and craftsmen. Yet proof is lacking for this kind of military feudalism in Ugarit. Indeed, it is clear that members of all professional groups, not just military specialists, received royal land grants in return for service. Moreover, it appears that the maryannūma, unlike feudal knights, did not necessarily own the horses and chariots they used but depended on the palace to supply them.14

Gray’s use of the term “guild” is also anachronistic and misleading, although he is certainly not alone in describing the occupational groups of Ugarit as “guilds” or (in French) “corporations” (see, e.g., Violleaud 1940 and Gordon 1965:262 and passim), a usage that can be accurate only in the loosest possible sense and thus begs qualification. Within the framework of Gray’s own feudal model, for instance, one wonders whether the maryannūma are to be viewed as a guild like the others or a military aristocracy. Medieval European guilds were quasi-independent self-governing bodies possessing certain rights and exemptions and dedicated to the mutual aid and protection of their members. It is true that in some account texts at Ugarit a single figure or grand total is entered for each occupational group, suggesting that certain payments or disbursements were made collectively, or at least were recorded that way (e.g., RS 11.716 = KTU 4.68, and RS 19.17 = KTU 4.610). But many other lists and accounts name the members of


14 This has been argued by Heltzer (1982:113f.) on the basis of texts describing horse- and chariot-equipment kept in the royal stores, e.g., KTU 4.169.
occupational groups individually, indicating that they usually contributed services and received rations on an individual basis. We know too little about the internal organization of the occupational groups of Ugarit (or the lack of it) to argue that each such group constituted a close-knit corps with its own ethos and formal corporate administration, which is what “guild” implies. Classification by occupational group might rather have been simply the administrative means by which the palace identified the specialized function that the members of each group could be expected to perform when called upon to contribute their services to the king.15

Georges Boyer’s Occupational Feudalism

A more refined application of the feudal model came from the pen of Georges Boyer in a long essay that accompanied the publication, in 1955, of the first sizable corpus of Akkadian legal texts from Ugarit (PRU 3). With the expanded documentation available to him, Boyer was able to discuss various social arrangements current in the kingdom during the Late Bronze Age and to compare them to similar practices elsewhere in the ancient Near East. He deals, for example, with interpersonal transactions having to do with marriage, adoption, inheritance, slavery, and debt. But a major part of his essay is devoted to what Boyer was able to discuss various social arrangements current in the kingdom during the Late Bronze Age and to compare them to similar practices elsewhere in the ancient Near East. He deals, for example, with interpersonal transactions having to do with marriage, adoption, inheritance, slavery, and debt. But a major part of his essay is devoted to what “the droit des fiefs,” and contains his interpretation of the dozens of legal texts found in the palace that record royal grants or transfers of land. 16

The following two texts may be taken as representative of these royal acts, which follow a standard pattern. These two examples, of course, do not reveal the many variations found in the land-grant texts with respect to the descriptions of the property transferred,


16 Such texts constitute about half of the 179 legal texts published in PRU 3. Huehnergard (1989:323ff.) gives the following counts of various types of syllabic legal texts found at Ras Shamra, totaling 228 texts: adoptions, 8; exchanges of property, 12; guarantees, 4; manumissions, 10; memoranda, 1; private gifts, 3; purchases of property, 16; ransoms, 1; repayments of debt, 1; royal appointments, 2; royal confirmations of ownership, 3; royal exemptions, 1; **royal grants, 95**; sales, 18; verdicts, 5; wills, 9; other or uncertain types, 37. Two alphabetic grant-documents are also known (KTU 3.2 and 3.5), out of a total of only 10 Ugaritic legal texts. The land grants will be discussed in more detail below in chapter 11.3, as part of my critique of the work of Clayton Libolt, who made a special study of the land-grant texts from Ugarit in his 1985 University of Michigan dissertation.

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1. **RS 16.262 (PRU 3, p. 67)**

1) iš-tu u-mi an-ni-im
2) niq-ma-IM DUMU a-mis-tam-ra
3) LUGAL u-ga-ri-it
4) it-ta-aš-ši È AŠša
5) qa-du gáb-bi mi-im-mi-šu
6) ša’ 1a-aš-li-ma-na DUMU ’U-sī
7) na-ia-li
8) ù it-ta-din-šu
9) a-na’ a-da-nu-un-mi
10) a’ a-da-nu-un-mu
11) pl-il-ka, ša È ú-bal
12) ur-ra še-ra-am
13) ma-am-ma-a-an iš-tu ŠU ’a-da-nu-un-mi
14) û ST₄₆ DUMU₃₄₅₆šu
15) ù-ul e-kim-šu

Translation:

As of today, ‘Niqmaddu son of ‘Ammitamru, ‘king of Ugarit, ‘has taken the house and fields— together with all that belongs to it—to his daughter son of Ba’alasi ‘the nayyālu, and has given it to Adanummu. 10 Adanummu ‘will bear the service (piša) of the house. 12–13 ‘No one shall ever take it from Adanummu or from his sons.

2. **RS 15.111 (KTU 3.2)**

(One of only two alphabetic land-grant texts.)

Obverse:

1) lym.hnd
2) cmgtmr
3) bn.nqmp-
4) mlk.ugrt
5) ytn.bt.amndr
6) bn[a] 1 glym.bnš
7) [ml]k.d.b riš
8) [w.y]rlbn
9) [l.ç]bdmlk
10) [bn.]a[hm]r[l]n

Reverse:

11) [w l]bhn.ç’d
12) [č]m.mnk
13) mnkm.l.yqb
14) bt.hnd.bd
15) bdmlk
Translation:

As of today, 3Ammittamru’s son of Niqmepa, 3king of Ugarit, has given the house of Anani-garr 3son of AGYTN, serviceman (bsš) of the king, which is in RIŠ—he has given it to 3Abdu-malki 3son of AMTRN 3and to his sons. 12–13forever. No one shall ever take this house from 3Abdu-malki 3son of AMTRN 3or from his sons. Furthermore, there is no service (unľ) whatsoever due on it. . . .

In discussing the royal land grants, Boyer (1955: 294) argued that a distinction should be made between property-based feudalism (“la féodalité foncière”) and personal or occupational feudalism (“la féodalité de fonction ou personnelle”). Here he anticipates aspects of the two-sector model proposed by Heltzer and Liverani, which I will discuss in some detail below. In property-based feudalism, the service due to the king is tied to possession of the land, regardless of who holds it; in occupational feudalism, on the other hand, individuals owe personal service of a type determined by their occupational specialty, and the use of the land is a reward for their service.

According to Boyer, in occupational feudalism the land is, in theory, neither inheritable nor alienable, lest someone come to hold it who cannot perform the specialized function for which the land was granted in the first place. In property-based feudalism, however, the landholder may send a substitute to perform his service (which consists only of unspecialized labor), or the service obligation might simply be converted into a kind of rental payment in goods or silver; furthermore, the land is both inheritable and alienable. Boyer asserted that occupational feudalism was the norm in Old Babylonian Mesopotamia under Hammurapi. In Late Bronze Age Ugarit, however, and in the Hittite empire in general, royal power was weaker, he thought, permitting a trend toward the “patrimonialization” of fiefs (i.e., making them hereditary property) that moved an original occupational feudal system in the direction of property-based feudalism. The same transformation had occurred, according to Boyer, in Babylonia itself under Hammurapi’s successors, and later also at Nuzi (where fictive adoption was used to get around restrictions on alienating land) and in Kassite Babylonia.

I will not comment here on the situation in neighboring countries (the comparative evidence from Mesopotamia and Anatolia will be discussed below in chapter 12), but I find very little evidence from Ugarit for Boyer’s reconstruction of two different feudal systems, or for his suggestion of a chronological development toward the patrimonialization of land-holding in the kingdom. Boyer appeals to texts in which service owed in return for the use of land is distinguished from the specialized service performed by members of various professional groups. He cites two documents in particular: RS 16.162:24–26 (PRU 3, p. 126) and RS 16.139:13–14 (PRU 3, p. 145f.). At the end of both texts is a clause specifying the special type of service owed by the recipient of a royal grant and exempting him from any other service with respect to the land. The first text reads:16 26pil-ku-ma ša ša re-ši 25ub-bal ša-na pil-ku 26i-ia-na i-na AŠA šti an-na-tir, “he will bear the service of the ša reši-men; no other service is due for these fields”; and the second text reads: 13 šu-wa-ti ia-nu AŠA šu-wa-ti ia-nu 26pil-ka-ma ša re-ši umur-iš 26MAŠKIM ub-bal, “no service is due for the aforesaid fields, for he will bear the service of the prefect’s commanders?” (this text is actually not a grant but a private sale of property ratified by the king).17

But the distinction here is simply between the different types of service activity a landholder might engage in; it is not indicative of two different service systems. In my opinion, a less complicated interpretation is preferable; that is, a single, simple system linked land tenure to royal service, regardless of the occupation of the landholder. Unless they received a royal exemption, all landholders, whether specialists or not, were obliged to perform some kind of service for the king as a condition of their tenure, but the type of activity involved in rendering that service depended on the skills and profession of the landholder. It is clear from the land-grant texts that occupational specialists, just like nonspecialists, were theoretically liable for the ordinary kinds of service (such as unskilled corvée labor, military service, or delivery of taxes in kind) incumbent on anyone who held land—why else do these documents make a point of exempting specialists from ordinary service, specifying instead that they were to perform the specialized service of their professions? (See, e.g., RS 16.162:24–26 [PRU 3, p. 126] and RS 19.98:21–24 [PRU 6.31].)

16 bn.amrũn
17 pil-ku (equivalent to normative Akk. ilka) will be discussed below, along with Ugaritic terms (ubdy, unľ) related to the royal service system. Note that ša reši does not mean “eunuch” at Ugarit—the person so designated in RS 16.162 has sons (line 16) and is clearly granted land on a hereditary basis (cf. also RS 16.238+254:17 = PRU 3, p. 107f.).
As for Boyer’s argument that, at least initially, specialists would not have received heritable land grants for fear that their sons could not perform the same service, it is sufficient to note that the specialized occupations themselves were hereditary, so there was no reason that the landholding could not also be hereditary. In this respect, occupational specialists were again no different from “unspecialized” farmers who passed their land and their skills from father to son and continued to give rent or labor service to the king from generation to generation. Indeed, the very fact that occupational specialists were remunerated with landholdings and did not subsist entirely on royal rations indicates a fundamental similarity between their economic and social position and that of nonspecialists. Furthermore, there is strong evidence (discussed below) that almost all land grants were hereditary during the entire period attested in our texts; thus the service system did not change from one type to another but was at all times property-based and patrimonial, to use Boyer’s terms. Contrary to Boyer’s reconstruction, then, it is possible to argue that all landholders in Ugarit participated in a single service system. All servicemen received land grants because they all lived primarily on the fruits of their agricultural labor (or that of their families and sharecropping clients) and paid rent to the king, their landlord, in the form of goods and services; some also engaged in specialized occupations and performed correspondingly specialized work for the king.

Anson Rainey’s Feudal Despotism

The last proponent of the feudal model to be considered here is Anson Rainey, whose 1962 doctoral dissertation was one of the first book-length treatments of Ugaritian society. Parts of it were published piecemeal in journal articles, and a Hebrew version appeared in 1967. Since then Rainey has written other articles dealing with various aspects of the texts from Ugarit (e.g., Rainey 1962a; 1962b; 1963; 1965a; 1965b; 1965c; 1966; 1967a; 1967b; 1970; 1971; 1973; 1975). Rainey’s assorted writings on Ugarit consist largely of definitions and discussions of many different Ugaritic and Akkadian terms. In his doctoral dissertation he brought together a large amount of information and organized it in a useful way under broad headings like “monarchical author-

18 It is true that some texts do not make this explicit, and Libolt (1985) therefore makes a case for separating heritable and nonheritable land grants; but this distinction cannot be sustained. The evidence for the near universality of heritable grants and Libolt’s counterarguments are discussed in detail in the next section.

For Rainey, as for Alt and Boyer, the “feudalism” of Ugarit essentially consisted of a land-tenure system characterized by hereditary rights of usufruct in return for service or payment. Unlike Gray, Rainey attributes no special political role to the maryannu—charioteers or any other military corps. On the contrary, he places heavy emphasis on royal authority, in a way that tends to contradict the usual picture of feudal government as a somewhat decentralized arrangement in which powerful nobles wield substantial influence. Rainey (1962a:26, 245; 1965b:113) asserts that the king of Ugarit was the “titular owner” of all the land and towns in his domain, a right that he enjoyed by virtue of his position as a vassal of the Hittite king. In practice, this meant that he could give property to, or take it from, any of his subjects, and could transfer a person from one professional group to another, as a number of texts attest. Rainey (1962a:245) goes so far as to call the king of Ugarit “a typical oriental despot,” a comparison sharply at odds with the normal use of the term “feudal.” But this comparison may be more apt than the feudal model itself. In my opinion, “Oriental despotism,” properly defined, is not a bad description of Ugarit. If we jettison the confusing and misleading feudal terminology that Rainey and others have employed, we can make room for more appropriate historical analogies without rejecting the basic picture that these scholars have sought to portray. The royal service system of Ugarit, as Rainey describes it, resembles not so much feudalism as a miniature version of Ottoman sultanism and other Middle Eastern political systems like it—examples of what Max Weber called patrimonial regimes (see chapter 3.1 above).

It is worth repeating here that a patrimonial ruler, according to Weber’s definition, organizes his domain as an extension of his own household. Government officials are his personal servants and members of his household. In theory, all property belongs to the ruler because the entire kingdom is viewed as a single “household” of which the king is master and owner. Patrimonialism is an ideal type, of course, referring to a set of typical shared motivations for social action and the linguistic symbols by which
they are mediated, and it can appear in many different historical forms. But it is my contention that this model fits the evidence from Ugarit quite well, and communicates more accurately than do anachronistic terms like “fief” and “vassal” what scholars have tried to express by adopting this feudal terminology. The household model is also more faithful to the evidence, in my opinion, than the chief competing approach to Ugaritian society, namely, the two-sector model, to which we shall turn next.
Chapter 11. The Two-Sector Model: Ugarit and the “Asiatic Mode of Production”

My treatment of the royal service system of Late Bronze Age Ugarit is presented below in the form of a critique of Igor Diakonoff’s “two-sector” (Asiatic mode of production) model, as this has been applied to Ugarit in great detail by Michael Heltzer and Mario Liverani. In pursuing the negative goal of rebutting the two-sector interpretation of Ugaritian society, I am simultaneously developing an alternative model in terms of the Weberian concept of patrimonialism, which I have described at length in the preceding chapters. But whichever model we accept, we must answer certain questions about the basic social and economic workings of the kingdom of Ugarit: Who was liable for royal service—was the service obligation based on occupational specialization or did it apply to every landholder in the kingdom? What was the relationship between land tenure and royal service? Were service obligations and related landholdings passed on by inheritance from father to son? Was royal service full-time for most servicemen, or only part-time? Where did royal servicemen live—in the capital city only, or throughout the kingdom? Did they subsist for the most part on royally distributed rations, or were they and their families self-sufficient to a large extent in food they produced for themselves? Did the service system change over time, and if so, in what way? Evidence is available to answer these questions; moreover, in my view, the answers we obtain clearly contradict the two-sector model and confirm the patrimonial household model.

1. The “King’s Men” and the Royal Service System

Michael Heltzer has been by far the most prolific writer on the society and economy of Ugarit. Since the late 1950s he has published many articles on the subject in Russian and other languages, along with three detailed monographs in English that appeared between 1976 and 1982.1 These monographs—entitled The Rural Community in Ancient Ugarit; Goods, Prices and the Organization of Trade in Ugarit; and The Internal Organization of the Kingdom of Ugarit—are useful compendia of the information that Heltzer has gleaned from the texts over the years. Unfortunately, they are marred by numerous errors, as several reviewers have noted (e.g., Astour 1980; Healey 1980; Loretz 1980; Pardee 1982; Rummel 1982; Stieglitz 1980; Vargyas 1980; 1986). Nevertheless, Heltzer presents the data in a detailed and systematic fashion and his work provides a good starting point for a discussion of Ugaritan society.

Servants of the King and Landholding Servicemen

It is immediately apparent that Heltzer’s work constitutes a direct application of Diakonoff’s two-sector model (i.e., the model of the “Asiatic mode of production” or AMP) to the material from Ugarit. He does not explicitly frame his analysis in those terms, but Heltzer was Diakonoff’s student and all of the elements of Diakonoff’s model are to be found in his writings, including a strict separation between the royal servicemen (the state or slave sector) and the rural communities of the kingdom (the free sector), and a vigorous repudiation of the notion that the king owned all of the land. As evidence of the bipartite structure of Ugaritan society, Heltzer (1969:35f.; 1976a:4ff.; 1982:1; 1988:9f.) appeals to a Hittite royal edict (written in Akkadian) that was found at Ras Shamra (RS 17.238 = PRU 4, p. 107f.), in which the Hittite ruler Ḫattušili III pledges to extradite fugitives from Ugarit who are found among his ṣapišu people (landless expatriates or outlaws).

The Hittite edict is applied specifically to any “servant of the king of Ugarit, or son of Ugarit, or servant of a servant of the king of Ugarit.” 2 Heltzer argues that this text distinguishes the “sons of Ugarit,” who are the free inhabitants of the villages, from the nonfree “servants of the king” (and their servants) in the palace sector. To my mind, however, the Hittite author of this text is not separating villagers from full-time royal servicemen but is simply taking pains to include all inhabitants of the kingdom, of high rank or low, whether they be royal officials (the “servants of the king” par excellence) or ordinary independent householders (“sons of Ugarit” in general), or—lowest of all—mere dependents of the royal staff. “Sons of Ugarit” is a general description of all...

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2 The relevant part of the text reads:  šum-ma IR LUGAL  “a-ga-ri-it  ú lu-ú DUMU “a-ga-ri-it  lu-ú IR LUGAL “a-ga-ri-it  ma-am-ma i-te-eb-bi-ma . . . Note that, strictly speaking, this text reveals the viewpoint in the Hittite capital Ḫattuša, where it was written, not in Ugarit, and for that reason alone it is dangerous to place great weight on it as a systematic description of Ugaritan social structure.
residents of the kingdom (or, at least, male heads of land-holding households), which is in keeping with Akkadian usage elsewhere. This is clear from lines 11ff. of the same edict, where the term DUMUmeš kurúga-ri-it alone is used generically to designate the potential fugitives, and from a number of other international documents found at Ugarit, e.g., RS 16.270:12 (PRU 4, pp. 134ff.); RS 17.146:14–15 (PRU 4, pp. 154ff.); and RS 18.115:24 (PRU 4, pp. 158ff.).

This point is demonstrated quite clearly also in RS 17.130 (PRU 4, pp. 103ff.), where we learn that the “sons of Ugarit” (mere villagers, according to Heltzer) are at odds with certain merchants of Ura, who are called “sons of Ura.” Note that powerful merchants are here referred to as “sons of” the city from which they come; thus presumably merchants from Ugarit traveling in other lands would likewise be known as “sons of Ugarit.” Yet Heltzer would be the first to argue that merchants as occupational specialists were not free villagers, according to his scheme, but were nonfree royal servants of the palace sector (see Heltzer 1978:123). It is clear from this text that a “servant of the king” (such as a merchant) was also a “son of Ugarit”; thus the former, more restrictive designation, as it is used in the Hittite edict, does not indicate a bipartite social structure, but simply served to single out the immediate members of the king’s household, including his highest ranking officials and full-time staff, emphasizing that they, too, were subject to extradition if they fled their master’s house.

A similar criticism of Heltzer’s interpretation of the Hittite edict has been made by Péter Vargyas (1988). For one thing, he rejects Heltzer’s close identification of the Akkadian phrase arad šarrī, “servant of the king,” in this text with the Ugaritic term bunušu malki, “man of the king,” which occurs in alphabetic administrative texts (written bnš mlk). Vargyas argues that despite their obvious semantic overlap, there are no pairs of syllabic and alphabetic texts in which the two expressions are used in a precisely synonymous fashion. For example, although Heltzer cites RS 15.111 (= KTU 3.2, translated above on p. 216f.), a Ugaritic royal grant in which a man whose house is given to another is called bunušu malki, Vargyas responds by arguing that analogous Akkadian land-grant texts from Ugarit do not normally use the phrase arad šarrī, specifying instead the person’s profession (if they describe him at all).

Now, Vargyas fails to discuss those few Akkadian texts that do record grants by the king to “PN his servant” (ḫa3R-šu),3 parallel to the use of the term bunušu malki in the Ugaritic land-grant text that Heltzer cites. Given the similarity between the two expressions, there is no reason to deny that some persons might be called both bunušu malki, “man of the king,” and arad šarrī, “servant of the king.” Yet Vargyas is correct to say that this sort of overlap does not prove that the term arad šarrī, as it is used in the Hittite edict, has exactly the same range of meaning as bunušu malki. His main point is that the ambiguous phrase arad šarrī, because it is counterposed in this text to the broad category mār Ugarit, “son of Ugarit,” must have a narrower meaning here than it might otherwise have. Vargyas therefore proposes, quite plausibly, that in the Hittite edict arad šarrī does not include all royal servicemen (bunušu malki) without regard to rank, but refers only to the highest-ranking members of the king’s household, while mār Ugarit takes in the rest of the “free” population of royal servicemen (i.e., anyone who is not a propertyless slave or dependent client), including both ordinary farmers and the lower-ranking members of various professional groups. The division in this text is not between royal servicemen and rural villagers, but between high rank and low, rich and poor.

Vargyas also makes the point that differences in rank and wealth cut across group membership, making it dangerous to assign places in the social order based simply on a person’s profession—or lack of one. He notes that the disparities in wealth within certain professional groups at Ugarit far outweighed the differences in social rank that were due to the hierarchical ordering of the professions themselves. In other words, the wealthiest members of lower-ranking professions were better off than the poorest members of high-ranking professions. The usual order in which professions are listed places the mrynm (charioteers) first, then the mrum (commanders?), šrm (?), gnmm (archers?), qdm (cult personnel), and mkrm (merchants), followed by various groups of craftsmen and lower-ranking military personnel.4 There is some variation in the order of professions: in one Akkadian text (RS 17.131 = PRU 6.93), for example, the merchants rank higher than they do in the Ugaritic lists. More important than the relative rank of professions, however, are the striking internal differences within each profession in terms of the quantity of goods paid and received by individual members of these groups (see RS 11.715+ = KTU 4.69 and RS 16.126B+ = PRU 3, pp. 199ff.). A person’s wealth was not entirely dependent on his profession; the amounts of oil and

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3 E.g., RS 16.189:8 (PRU 3, p. 91f.), 16.247:8 (PRU 3, p. 65), and 16.260:5 (PRU 3, p. 98f.).

4 See, e.g., RS 14.84 (KTU 4.126), 18.252 (KTU 4.416), 25.417 (KTU 4.745), and 17.131 (PRU 6.93).
silver associated with certain priests and merchants, for example, are much greater than those associated with the poorest charioteers, even though members of higher-ranking occupations tended to be wealthier on average.

Similar evidence is available that indicates differences in wealth among villagers who are not identified as belonging to any professional group. Vargyas (1988:121) simply assumes, quite reasonably, that economic stratification must have existed in the village population just as it did among professional groups; however, he overlooks the texts in which this is actually demonstrated (e.g., RS 10.52 = KTU 4.63 and RS 19.43 = PRU 6.104; possibly also RS 16.155 = PRU 3, p. 205, although no village is mentioned). It is true that accounts of taxes paid by villages usually provide only the total amount for each village, making it impossible in most cases to tell how much each resident contributed, but there are a few texts in which individual payments are recorded and wealth differentials can be observed; for example, RS 19.37 (KTU 4.616) appears to record quantities of livestock delivered by various residents of the village of tbq.

Given the overall thrust of his argument, it is odd that Vargyas (1988:122) still sees a basic distinction between villagers who “owned” land and professionals who merely received the use of so-called state lands in return for their services, because the points he raises call into question not only Heltzer’s interpretation of the Hittite edict but the applicability to Ugarit of the two-sector model as a whole. What now distinguishes the “palace sector” from the “village sector”? Occupational specialists and ordinary farmers were all “sons of Ugarit”; if there was a basic social division it was not between specialists and nonspecialists but between an elite of royal officials who were closest to the king and the rest of the populace. Indeed, occupational specialists and ordinary farmers alike were also royal servicemen or “men of the king” of one kind or another. Vargyas himself notes that the term bunuša malki, which Heltzer simply equates with arad šarrri and translates as “royal dependent” (that is, a member of the palace or slave sector as opposed to the “free” village sector), was not limited to specialists or full-time workers in the palace establishment but could also be applied to ordinary villagers.

The Ugaritic word bunuša is equated with Akkadian amilla, “(service)man,” in the polyglot vocabularies found at Ras Shamra (e.g., RS 20.149 = Ugar. 5.130 ii 5’, 8’, where a-mi-lu is glossed as bu-nu-šu).6 There is no doubt that its basic meaning is quite broad, and a number of texts use the term in this unqualified sense.6 The phrase bunuša malki, on the other hand, appears in only a few texts, most of which are ration accounts or personnel rosters (see Heltzer 1982:3–15 for a summary of these texts). Clearly, bunuša malki means something like “royal serviceman” or “man performing the king’s service.”7 But not all of those so designated were necessarily occupational specialists or full-time palace workers, and some appear to have been ordinary villagers. In RS 19.16 (KTU 4.609) the bunuša malki include watchmen of vineyards (ňgr krm, l. 12; cf. also ňgr mdr, “watchmen of the sown land,” in RS 15.22+ = KTU 4.141 iii:16) and plowmen (hrtn, l. 27). These agricultural “professions” do not appear in the lists naming, in order of rank, various specialized occupations; e.g., RS 14.84 (KTU 4.126); RS 18.252 (KTU 4.416); RS 25.417 (KTU 4.745); RS 17.131 (PRU 6.93). This suggests that they did not constitute professional groups like the others but were simply functions assigned to farmers performing their temporary labor service. If rb qri in KTU 4.141 iii:3, a text which lists bunuša malki by name and category, is translated “chief of a village” (i.e., a local village headman or elder), then there is direct evidence in this text that a villager could be called bunuša malki.

Furthermore, the administrative records of the palace more often simply use the abbreviation bunuš (pl. bunušuma) without qualification to describe men on royal service, many of whom obviously did not have a craft or a military profession but were simple herdsmen or agricultural workers. This is true, for example, of the bunušuma assigned to various royal estates (termed gt, probably pronounced /gittul/, who include herdsmen (r’ywn) and donkey-drivers (r’y hmrn; 6 See UT Glossary no. 486, p. 373f. The letter KTU 2.33:33–34 reads w.mlk.b’ry.bnš l bnn, which Heltzer translates “and the king, my Lord, made out of me . . . a man” (Heltzer 1982:3); i.e., the king elevated him to the social position of bnš. But “he ‘built’ me” would be written bnn not bnn, and most other scholars translate bnš bnn as “intermediary” (literally, “a man between”) and interpret the passage quite differently; cf. UT Glossary no. 462, p. 371 and TO 2, p. 339.
7 In RS 18.26:17 (KTU 4.339) the phrase bnšm dt l mlk is a more elaborate version of the term bnš mlk, according to many scholars, including Vargyas (1988:112f.), who takes it to refer to men “en service commandé du roi” (cf. Heltzer 1982:10 for a similar interpretation). The persons called bnšm dt l mlk are contrasted, however, with the bnšm dt l u’gr th mentioned earlier in the same text; thus mlk might be simply the name of a town (i.e., the town “Mulukki” known from syllabic texts), analogous to ugr, and not a reference to the king.)
see *KTU* 4.243:45 and 4.618:3). Another text, *KTU* 4.618, enumerates the teams of oxen (*šmd alpm*) used by the *buusušma* in their work. In one account text (*KTU* 4.125) skilled occupations are listed together with the (unskilled) *buusušma* of various grs. Now, because the terms *buusušma* and *buusuš maltki* are used in identical contexts, Heltzer (1982:12–14) quite naturally, and I think correctly, regards them as equivalent. But in his scheme this implies that the palace sector included unskilled laborers, in particular agricultural workers, who did not reside in the villages but were permanently assigned to various royal farms. This is possible, and both Heltzer (1982: 49–79; esp. pp. 63ff.) and, even more so, Mario Livervani (1979c:1341; 1989)—whose work will be discussed below—envision an extensive network of royal farms staffed by permanent dependent workers throughout the kingdom.

For example, there are texts in which metalworkers (RS 17.23 = *KTU* 4.261; cf. *KTU* 4.181; 4.310; 4.337:3), merchants (RS 18.78 = *KTU* 4.369),9 and charioteers (RS 12.34+12.43 = *PRU* 3, pp. 192ff.) are associated with various villages. In RS 20.09 (*KTU* 4.690) certain *mdm* (“friends” of the king?) are identified by the places where they live (i.e., with entries of the form “PN GN-”), as are the *marglam*-soldiers in the long roster RS 5.248 (*KTU* 4.33). In a legal text, RS 20.146:20 (Ug 5.83), a leatherworker (Akk. *aškāpu*) of the town of Riqdū serves as a witness. Members of another professional group, the *mḥšm*, are found in various villages (RS 13.20 = *KTU* 4.124; RS 18.10+18.56 = *KTU* 4.332:14–17), as are the *mr̪m* (“commanders?”) and *kbsm* (“fullers”).10 It seems likely that priests and cultic personnel were also distributed throughout the kingdom to serve local sanctuaries (see Heltzer 1976a:71–74 for a defense of this point). One text mentions the priest of Ba’lu in a certain town,

8 Heltzer (1982:64 n. 136) implies that he would like to see the *buusušma* of *KTU* 4.355 as residents of 42 different royal farms (grs), but he admits that the word *gr* does not occur in this text; moreover, the place names in this text are exactly those of the villages of the kingdom known from other sources. Note also RS 18.35 (*KTU* 4.347), which lists *rism dt šrb b bnšm*, “inhabitants of RIS who act as guarantors for (literally, ’entered [as security for]’) their servicemen.” There are four entries in this list, of the form PN w PN2 šrb b PN3; i.e., “PN and PN2 act as guarantors for PN3” (although the fourth entry names just one guarantor, omitting w PN2). Heltzer (1976a:28) acknowledges that this text refers to *buusušma* who are associated with a village—although he incorrectly translates *šrb b* as simply “enter with” rather than “act as guarantor,” interpreting the text to mean that the villagers had come to perform their obligatory corvée labor.

9 This text does not indicate that merchants (Ug. *mkrm*, probably vocalized *mAikiruma*) acted as royal tax-collectors in the villages, as Heltzer suggests (e.g., Heltzer 1978:135f.). It is much more likely that they were simply village residents who paid tribute (*argnmn*) in silver. Heltzer translates the heading *spr argnmn* in line 1 of this text as “list of tribute-collectors,” but *argnmn* probably means “tribute-payers” in light of the similar text RS 17.23 (*KTU* 4.261), where the heading reads *spr argnmn nskm,* “account of the tribute of the metalsmiths”—unless the metalsmiths living in various villages were also tax-collectors. Furthermore, the *argnmn* of RS 18.78 include not just merchants (*mkrm*) but also shepherds (*mqdam;* l. 8), whom Heltzer takes to be taxpayers, not tax-collectors.

10 RS 18.10+18.56 (*KTU* 4.332) mentions men of the town of *qr* who are identified by profession as *mra*, *kbs*, and *mḥš*. The Prevalence of Part-time Service

In my opinion, then, the term *buusuš maltki* (often abbreviated as *buusušma*) does indeed refer to “royal servicemen”; that is, persons who worked for the king. But their work was not necessarily full-time nor is it necessary to postulate two sectors in the population, one of full-time royal servicemen and another of villagers. This distinction breaks down not only because occupational specialists in the capital and non-specialists from the villages alike performed royal service and were called *buusuš maltki*, as I have argued, but also because members of various professional groups (Heltzer’s “royal dependents of the palace sector” *par excellence*) lived not just in the palace or in the city of Ugarit but were to be found in villages throughout the kingdom.

For example, there are texts in which metalworkers (RS 17.23 = *KTU* 4.261; cf. *KTU* 4.181; 4.310; 4.337:3), merchants (RS 18.78 = *KTU* 4.369),9 and charioteers (RS 12.34+12.43 = *PRU* 3, pp. 192ff.) are associated with various villages. In RS 20.09 (*KTU* 4.690) certain *mdm* (“friends” of the king?) are identified by the places where they live (i.e., with entries of the form “PN GN-”), as are the *marglam*-soldiers in the long roster RS 5.248 (*KTU* 4.33). In a legal text, RS 20.146:20 (Ug 5.83), a leatherworker (Akk. *aškāpu*) of the town of Riqdū serves as a witness. Members of another professional group, the *mḥšm*, are found in various villages (RS 13.20 = *KTU* 4.124; RS 18.10+18.56 = *KTU* 4.332:14–17), as are the *mr̪m* (“commanders?”) and *kbsm* (“fullers”).10 It seems likely that priests and cultic personnel were also distributed throughout the kingdom to serve local sanctuaries (see Heltzer 1976a:71–74 for a defense of this point). One text mentions the priest of Ba’lu in a certain town,

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although the place name is broken off. In general, it is quite probable that occupational specialists formed an integral part of every community. Indeed, many of those in the “village sector” whose names appear in records of tax payments or labor service may also have been listed among the professional groups of the “palace sector,” as is clearly the case in the census lists from Alalah level IV (discussed below in chapter 12.7), which record the names of personnel and specialists of all ranks who resided in various villages of the neighboring Late Bronze Age kingdom of Mukiš.

Conversely, it is by no means likely that all or even most of the thousands of inhabitants of the capital city of Ugarit were members of professional groups or dependent servants of the king. There is archaeological evidence from Ras Shamra (discussed below in chapter 13.3) that agriculture was an important activity for most households in the city, a situation typical of “urban” centers in the region, ancient and modern. No doubt many of these households were headed by occupational specialists who combined farming with professional work or had kinsmen and clients to help them cultivate their land, but their houses are indistinguishable from those of ordinary farmers. Indeed, it is unwise to assume that a strict urban-rural dichotomy, corresponding to a separation between occupational specialists and farmers, prevailed under preindustrial conditions anywhere in the Near East. The purpose of the pervasive land-grant system of Ugarit was surely to permit royal servicemen and their dependents—wherever they lived, whatever their specialty, or lack thereof—to subsist primarily on the agricultural labors of their own households, an arrangement more efficient and less costly from the palace’s point of view than the distribution of rations on a massive scale. The estates granted by the king, even to professionals, were not just small garden plots intended to supplement a family’s food supply; they were complete farms, and often included fields, vineyards, olive groves, livestock, and buildings. Most servicemen, professionals included, probably received royal rations only while they were fulfilling their service obligations to the king, although there may well have been a relatively small group of full-time royal servants or slaves resident in the palace who did not have households of their own and did not grow their own food, but were fed solely by the king.

Unfortunately, our sources do not indicate whether royal service and rations (for specialists, in particular) were year-round or of limited duration. But at the very least it seems unlikely that servicemen resident in distant villages (including charioteers, merchants, metalsmiths, etc.) would have been fed from the palace stores; nor is it likely that they and their families abstained from working their own hereditary landholdings and handed them over to tenants or to other palace employees to cultivate on their behalf—especially if royal service was generally a part-time

11 RS 11.856, published by Virolleaud in RA 38 (1941):4f. The town in question might, of course, be Ugarit itself, in which case this text is not evidence of a local priesthood.
12 Such a conclusion is implied by Heltzer and stated outright by Liverani (1979c:1318f.). Liverani estimates the number of specialists dependent on the palace at about 800, based on evidence for approximately 40 categories of professionals with an average of 20 members each. Each of these specialists, in his opinion, represented a household of 6–8 persons, for a total of 4,800–6,400. Liverani adds to this total a few hundred palace workers of low rank, representing households of perhaps 3 persons each, for an additional 500–1,000 persons. The population of the city of Ugarit (i.e., of the “palace sector”)—calculated on the basis of the texts—would then agree with the estimate of 6,000–8,000 that he makes on the basis of the area of the tell and the average size of the private houses; i.e., 1,000 houses with 6–8 inhabitants each. But Liverani’s calculations rest on a number of unproven assumptions concerning the proportion of the tell devoted to private houses (and thus the number of houses), the size of the average household, and the question of whether most of the inhabitants of the city were dependents of the palace—issues that will be discussed below in chapter 13. In an earlier article, Liverani (1975:148) mentions in passing that “free” peasants also lived in the capital, which in some respects was a village like the others. In his later writings, however, he seems to have abandoned this idea; cf. his calculations described above and the statement that: “Les maisons privées d’Ugarit appartiennent pour la plupart à des fonctionnaires palatins et autres spécialistes” (Liverani 1979c:1318).

13 Renger (1984:65) voices a similar opinion concerning Old Babylonian society: “Most city dwellers including large numbers of officials, craftsmen and even menial workers relied for their livelihood on agricultural holdings too.”
14 Mesopotamian sources indicate, however, that part-time service and rations were common in the third millennium, at least, and probably later as well (see Gelb 1979b:23f.). Using dated administrative texts, Maekawa (1976) and Steinkeller (1987b) have shown that the érin-workers of Ur III Lagaš and Umma worked for the crown (on both corvée projects and specialized tasks) and received rations only during the last four or five months of the year. They cultivated their own land allotments during the rest of the year. Steinkeller (1987b:97) observes that “the part-time character of forester’s duties . . . is a necessary corollary of their having been prebenders, who would have had to work their own land allotments during the other part of the year.” Note also Steinkeller’s conclusion that both land grants and occupations were hereditary, and that teams of workers were organized on family lines, each gang being headed by a father who was assisted by his sons. On this phenomenon see further chapter 12.2 below.
affair. No doubt it was easier to distribute rations to specialists who lived in the capital, but there is no evidence that this was their primary means of support, and the evidence of agriculture found in many of their houses suggests otherwise (see chapter 13.3). Even members of the king’s own family were apparently self-sufficient, in some cases, and did not depend entirely on royal rations; the land-grant texts reveal that Niqmaddu II’s brother, sister, and daughter all received hereditary landholdings of their own.

In general, we may question the view that the population of the kingdom of Ugarit was sharply divided between rural farmers who worked their own land and urban specialists who worked full-time for the king and received rations from the palace. Everyone farmed to some extent and both specialists and nonspecialists were to be found everywhere, although members of certain professional groups were not doubt concentrated in the capital city. This explains the indiscriminate mixing of persons identified by village and by profession in account texts such as RS 11.839 (PRU 3, p. 194f.) and RS 20.425 (Ug 5.99).

Landholders and Their Landless Clients

A further consideration is that bunusu does not mean “servant” or “slave,” which was written ābd (pronounced ā-abdu/) in Ugaritic; hence it is incorrect simply to identify Ugaritic bunusu malki with Akkadian arad šarrī, as Heltzer does, in the sense of a full-time dependent worker in the royal establishment. It is probable that bunusu refers to a man of some standing—a householder (cf. Akk. amitu, with which Ug. bunusu it is equated in the polyglot vocabularies). This is indicated by the use of a different term, bidalu (written bdl in alphabetic texts), as an equivalent for Akkadian muskēnu, which is often used in contrast to amitu to refer to a propertyless or subordinate person (e.g., in the laws of Hammurapi). The basic system operative in Ugarit and elsewhere was one in which a landholder (bunusu, amitu) was granted land by the king in return for regular service and/or an in-kind rental payment (commuted to a payment in silver, in some cases). A landless man (bidalu, muskēnu) would subsist as a menial slave or soldier, or attach himself to a landholder as a dependent client or sharecropper. He was not himself directly liable for royal service but might be called upon to perform such service on behalf of a landholding patron. This system, with its basic requirement of royal service for all landholders but with the possibility of sending a substitute, was a simple, effective, and flexible solution to the problem of organizing manpower on behalf of a central political authority in the context of a poorly monetized, hierarchically structured society in which human labor was the scarcest resource.

There has been some debate over the meaning of bidalu, however. In RS 16.126B+, an Akkadian text which is a list of rations (or payments) of olive oil given to (or delivered by) persons who are grouped by occupation, Akkadian tamkāru, “merchants” (written logographically ḫmēšDAM.GARmeš) appears to be glossed by the syllabically written Ugaritic word bi-da-lu-ma (ii 12). This has led scholars to translate bidalu as “merchant, trader” (cf. Arabic badala, “to exchange”; e.g., Rainey 1963; Gordon, UT Glossary no. 448; Astour 1972; Huehnergard 1987:112). But there is already a word for merchant in Ugaritic, namely, mkr (pronounced mākīru or makkāru), which is related to the well-known Semitic root m-k-r, “to trade, sell.” This is the expected Ugaritic equivalent of Akkadian tamkāru; moreover, the uses of bidalu in alphabetic texts argue against a meaning “merchant” and in favor of a meaning like “substitute” or “assistant.”

It is worth noting that Akkadian tamkāru itself can refer to an “agent” or “proxy” of any kind, not just a “purchasing agent” or merchant. Thus it is conceivable that bidalūma here is a direct gloss on tamkāru, without requiring that bidalūma refer to merchants (rather than simply “substitutes” or “assistants” in general) in other texts where this meaning seems unlikely. Still, in light of the overall structure of RS 16.126B+ (discussed below), it seems more probable that tamkāru : bidalūma does not equate tamkāru and

15 Liverani (1975:149) assumes that “palace dependents” did not personally work their landholdings. This may be true, if he means that their dependent relatives or household servants cultivated the land for them. But even if a specialist did not personally engage in agricultural labor, it is reasonable to suppose that work on the land was an integral part of his household’s activity and its chief means of support (see chapter 13.3), and that the head of the household supervised the work. This holds true even in cases where a landholder handed over some or all of his land to a tenant or sharecropper to work on his behalf, either because he had no sons or dependent relatives to help him, or because he and his family lived too far from the landholding to work it themselves. There is no reason, then, to separate occupational specialists from ordinary agriculture as sharply as Liverani appears to do.


17 The meaning of the terms awilum and muskēnum in Old Babylonian texts is discussed below in chapter 12.4.
This is even more likely if one appeals not to the poorly attested Semitic root *b-d-l (even if this be taken in the sense of “exchanging” or substituting one person for another), but rather to Hurrian *p/bid(d)-, “to help” (see Márquez Rowe 1998:372; Girbal 1992:164ff.). The Hurrian derivation of bidalu is supported by the Late Bronze Age census lists from Alalah (level IV), capital of the kingdom of Mukiš, in which Hurrianized vocabulary is abundant and the term biddallenni designates a type of person (see AT 189, where it is written bi-id-dal-le-ni in l. 14 and bi-dal-le-en-ni in l. 167; the same word is also written dub-ba-le-ne-na in AT 189:53, dub-ba-le-ni in AT 197:1, and di-bal-le-nu in AT 192:24; on this metathesis, see Dietrich and Loretz 1970:110ff., 117ff.; von Dassow 1997:123, 142, 150ff.). The biddallenni-personnel of the kingdom of Mukiš were clearly of subordinate social status. It appears that this term was a synonym for (or at the very least referred to a sub-group of) the social category known more generally in the Alalah IV census texts as haniaahhe (Dietrich and Loretz 1970:122; cf. von Dassow 1997:133ff.).

Those called haniaahhe were landless persons, judging by the use of the Akkadian term ekā, “destitute,” to refer to them (see the discussion in Dietrich and Loretz 1969a:91f.).

It is therefore likely that haniaahhe was the local term in Late Bronze Age Alalah for the type of person, sometimes also referred to at Alalah as biddallemni, who was known in second-millennium Mesopotamia as a muškēnu(m). Very often, presumably, this sort of person served a landholder as a dependent client or sharecropper and so could be called upon to fulfill his patron’s royal service obligation, which was normally predicated on the possession of a landholding. Indeed, the term muškēnu itself appears very rarely in the Alalah IV texts, and where it does it can be equated with haniaahhe (see von Dassow 1997:384ff.). The Alalah evidence will be discussed further in chapter 12.7 below, but already it should be clear that there are useful parallels, both in the social situation and in the use of Hurrian terminology, between Ugarit and the neighboring kingdom of Mukiš.

Apart from Ugaritic there is no clear evidence for a Semitic root b-d-l in the second millennium B.C. or later, until it appears in Arabic as badala, “to exchange.” On the other hand, the occurrence of the term ba-da-lum (probably pronounced /baddalum/) in third-millennium texts from Ebla may point to an archaic Semitic etymology. Note that Eblaite ba-da-lum, although it was initially translated as “merchant” (Fronzaroli 1982:106; 1984b:137), is now taken to mean “representative, vizier” by Alfonso Archi (1988a) on the basis of its occurrence in several texts in places where one would expect the logogram EN, “king” (e.g., . . . NĜ.BA EN ra-à-ak ki EN ir-i-turn ba-da-lum [ha-r]a-an ki ba-da-lum sa-nap-zu-güm ki ba-da-lum ur-sá-um ki “. . . gifts for the king of Ra’ak, the king of Iritum, the baddalum of Harran, the baddalum of Sanapzugum, the baddalum of Ur-saum”; note also the reference to EN ha-ra-an ki wa ba-da-lum-si, “the king of Harran and his baddalum”; see ibid., p. 2, for these texts; see also Petti nato 1998:3–6 for numerous other text references). Archi points out that these uses of the word show that the term “indicates a function that: (1) pertains to the organization of some city-states situated in a certain region, or is called [baddalum] there . . . ; (2) is given to only one person in each of those cities; (3) ranks lower than kingship, but the person holding the title acts in the king’s stead” (ibid.). If Eblaite baddalum

18 Márquez Rowe (p. 372) suggests that “the form in question may be analysed as a root *p/bid(d)- suffixed with the morpheme building up professional designations =l(i)= . . . This etymology could perhaps explain the unique, non-Ugaritic and Hurrian-like plural morpheme =n(a) in the [other] syllabic Ugaritic example [k=a=ba-la-ru-na] in a list of professions. RS 15.172:14 = PRU 3, p. 204ff.]”

19 As both Márquez Rowe and Dietrich and Loretz have noted, the possible equivalence between the biddallemni-personnel of Alalah and the bidaluma of Ugarit was proposed long ago by Ann Drafkorn Kilmer in her 1959 dissertation on Hurrians and Hurrian at Alalah (p. 234). In census list AT 192 biddallemni is the second category of personnel, parallel to the position of haniaahhe as the second category in census lists AT 190 and AT 195, where the term biddallemni is not used. On the other hand, both terms appear in AT 189, suggesting that biddallemni could be distinguished as a subcategory of haniaahhe.

20 Dietrich and Loretz (1970:118) suggest a derivation for biddallemni (which they render as pitallemni) from Hittite pita-, “duty, obligation,” with a Hurrian suffix, and so they translate it as “Lehensträger, Abgabepflichtiger.” Márquez Rowe (p. 372) believes that such a “hybrid Hittito-Hurrian etymology” is unlikely. Moreover, it is now recognized that pita- (or piṭṭa-) means “allotment” in Hittite, rather than “duty” (see CHD P, p. 262), in which case one could perhaps argue that biddallemni at Alalah and the cognate term bidalu at Ugarit refer to a dependent client who receives an allotment (i.e., a sharecropper), although this seems doubtful. If one is seeking a hybrid Hittito-Hurrian etymology, it might be better to cite Hittite pedi, “instead of, in place of (another person)” (CHD P, p. 342f.), which accords with the context-based interpretation of Ug. bidalu as “substitute” or “lieutenant.” But a purely Hurrian derivation from *bidd-, “to help,” still seems more probable. Either way my interpretation of these terms is not greatly affected.
is indeed related to Ug. bidalu, then we have here striking confirmation of a basic meaning of “deputy, assistant.” Moreover, a connection with Hurrian bid(d)-, “to help,” as opposed to a native Semitic etymology, is still possible, in view of the fact that baddalam does not seem to be the usual word for “vizier” at Ebla but is associated with cities to the north such as Harran, in the upper Euphrates region, where Hurrian political and linguistic influence was probably felt already in the third millennium B.C. (see Steinkeller 1998).

Whatever its etymology, this meaning for bidalu is indicated by its use in alphabetic texts from Ugarit. The names of four bdl mrynm (“assistants[?] of charioteers”) are given at the end of a list of marynnāma in RS 11.715+i 6ff. (KTU 4.69); and, in the same text, three bdl mārgīm are listed after the mārgīm (vi 17f). In RS 11.786 (KTU 4.85), a list of five qrtym from various villages is followed by five bdl qrtym. It seems unlikely that there were merchants attached to these disparate occupational groups or that merchants also had military professions.21 The bidalāma here are more likely to have been landless dependents of certain land-holding royal servicemen (bunuša malki) who were acting as substitutes for their masters.22 If this interpretation is correct, then the heading lū.mul.GAR in RS 16.126B+i 12 does not contain a repetitive gloss on the word for merchants but refers to propertyless subordinates or clients of merchants. In other words, the term bidalu is not equivalent to tamkāru but merely qualifies it, and the “gloss mark” (composed of two small angled wedges, indicated in transliteration by a colon) does not designate a semantic equivalence but is simply a foreign word indicator (see Huhnerberg 1987:206 on the frequent use of the so-called gloss mark in Ugarit Akkadian texts to mark the following word as non-Akkadian).

In light of the structure of the lists preserved for two other professions in the same text, we may assume (following Heltzer 1978:122 n.10) that a roster of full-fledged merchants—those who were property-holding and thus service-paying bunušašu, not mere bidalāma—originally preceded the list of tamkāru : bidalāma, although the expected heading (lū.mul.DAM. GAR without any qualifier) is lost in the broken edge of the tablet at the beginning of column ii. There are ten names preserved here before the following section that has the heading lū.mul.DAM, and these might well be the names of merchants. We can therefore agree with Heltzer (ibid.) that the word bidalu used in this heading is equivalent to Akk. mškēnū, which is the term used in similar headings in the same text, followed by the name of a profession (see also Albrecht Alt’s discussion in WO 2/4 [1957]:338–42). The headings mškēnūtu lū.mul.” and [mškēnū]tu “širūma follow lists of lū.mul.” and “širūma, respectively (see RS 16.126B+i 17 and iii 30).23 Note that in these two headings the second term is in the nominative case, in apposition to the first, just as it is in the heading tamkāru : bidalāma. This underscores the similarity between the use of the term bidalu and its Akkadian equivalent mškēnu in this text, although it is not clear why the scribe used tamkāru : bidalāma in ii 12 instead of restricting himself to Akkadian only (i.e., mškēnūtu tamkāru), as he did elsewhere in this text.24

This interpretation of bidalu is admittedly somewhat speculative, but no more so than the alternative; and other occurrences of the term in alphabetic texts support it. There were bidalāma identified with various places as well as with professions. RS 16.128:i 4, iii 1 (KTU 4.214) refers to bdl ar, i.e. “the bidalāma of the village ar.” RS 11.788 (KTU 4.86) also lists bidalāma of various villages. It is conceivable that these texts refer to merchants or procurement agents

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21 My colleague Dennis Pardee has suggested to me that the bidalāma were “procurement agents” or “commercial agents” acting on behalf of these occupational groups, and that the mākirišma were merchants of a different type (perhaps the official commercial agents of the king), which accounts for the existence of two different words for “merchant.” In my view the bidalāma were indeed agents acting on behalf of others, but there is no evidence that they were procurement agents other than the one occurrence of the collocation tamkāru : bidalāma in RS 16.126B+i 12, which I would explain by saying that the bidalāma here are agents of mākirišma just as they are agents of marynnāma and other professionals. Furthermore, mākirišma were resident in various towns of the kingdom (see RS 18.78 = KTU 4.369, discussed above), like metalworkers, charioteers, and other professionals, which suggests that they were not a special group of royal merchants.

22 In terms of its meaning, we can compare Ug. bidalu to Middle Assyrian pāhum, which Postgate (1982:305f.) argues was the technical term for a “substitute”; that is, a person employed (by means of a private transaction) to perform one’s ilku service.

23 On the unusual masculine plural ending -štu, see Huhnerberg 1989:146. The rare logogram un-tu (with a phonetic complement) may represent Akk. massartu (“garrison”); see Huhnerberg 1989:66.

24 One possibility is that the scribe mistakenly began the heading by writing tamkāru, duplicating the previous heading, instead of writing mškēnūtu tamkāru as he intended. Having already disrupted his standard format, he simply qualified tamkāru by adding the Ug. term bidalāma. In any case, the presence of a Ug. word here violates the pattern established in other headings in this text.
resident in various towns, but there is a list of bidal gt bn tbšn in RS 11.840 (KTU 4.96), and it is more difficult to explain the presence of merchants on a rural estate (gt). Another text has the heading spr bdlm, followed by a list of 13 names, but there is no other indication of the status or profession of those listed (RS 15.06 = KTU 4.134; cf. RS 16.355:42ff. = KTU 4.232).

In light of all the evidence, Rainey (1962a:136f.; 1963) seems to waver between two interpretations for bidalu (“substitute” and “merchant”), depending on the context. Michael Astour (1972), however, has argued that bidalu everywhere means “merchant”; thus he believes that merchants held military offices and merchants of various ranks were numerous and influential throughout the kingdom. (He also takes ḫaširāma to be the term for merchants who were organized in groups of ten, but this interpretation has generally been rejected.) The fact remains, however, that the only evidence for bidalu meaning “merchant” is the heading tamkārā : bidalāma in one Akkadian text, which can be explained differently, as we have seen—either by arguing that the two terms are equivalent but that tamkārā here means “proxies, agents” in general rather than “merchants, procurement agents,” or (more likely) by arguing that bidalāma is used in conjunction with an occupational name in the same fashion as muškēnūtu elsewhere in this text. Astour points to the names of known merchants that also occur in Ugaritic lists of bidalāma, but most of these names occur without patronyms or other identifying information and cannot be proved to refer to the same persons, as Heltzer (1978:122 n. 10) has noted.

If the Akkadian equivalents of Ugaritic bunušu and bidalu are amilu and muškēnu, respectively, it appears that men who served the king might be called bunuššuma or bunušuš malki in general, but strictly speaking the bunuššuma were landholders who were distinguished from their propertyless dependents or clients (bidalāma), who sometimes performed royal service as substitutes for their patrons. The bunušuš malki had hereditary occupations, if only that of “farmer,” and they had hereditary landholdings, as we have seen. Many of them probably possessed large households, complete with grown sons, servants, and other dependent relatives, to judge by the size and layout of the houses uncovered at Ras Shamra (see chapter 13.1). It is therefore reasonable to suppose that these men did not devote all of their energies to the king’s service, but that a great deal of private economic activity took place outside of the royal service system. In the first place, householders had to raise staple crops and livestock to feed themselves and their families. But there may also have been production for purposes of exchange, whether of specialized agricultural products like oil and wine, luxury items like purple dye and dyed and embroidered textiles, or the products of skilled craftsmen such as the metalsmiths, housebuilders, Leatherworkers, and engravers of whom our texts speak—not to mention the wide range of services provided by professional fullers, herders, merchants, priests, scribes, and so forth (for evidence of such private transactions in Old Babylonian Mesopotamia involving craftsmen and other specialists, see Renger 1984:86–89).

Unfortunately, direct evidence is lacking for matters that did not concern the palace, so the extent and vitality of the “private” economy are difficult to assess. Nevertheless, I think it is fair to presume that royal service was, by and large, a part-time affair, not just for ordinary farmers but also for occupational specialists. This is impossible to show conclusively, of course, because terms of service are not indicated in our texts and most of the documents are not dated. An account text like RS 20.425 (Ug 5.99) records disbursements of wine and oil made in a certain month to various professional groups and to the men of two different villages (a combination, by the way, that calls into question any strict separation between the service performed by villagers and that of occupational specialists). The period of service was obviously short term for the villagers, who at some point would have returned to their homes. Can the same be said for the professional groups listed in the same text? In the absence of a series of dated accounts we can only suggest that this was indeed the case.26

Heltzer advances other arguments in defense of the two-sector approach to the society and economy of Ugarit. For one thing, he finds a semantic difference in what others have taken to be synonymous terms describing the royal service obligation. The Akkadian texts from Ugarit usually employ the word pilku for such service instead of normative Akkadian ilku,

25 Among the men listed in this text are an Ashdodite (adddy), an Egyptian (msry), and a Canaanite (kr’)ny); but the presence of foreigners among the bidalāma need not be taken as evidence that they were merchants (cf. Rainey 1963:314), because aliens were equally likely to have been landless clients.

26 See Heltzer 1982:39–43 for a summary of the texts dealing with the provision of rations. The longest such text in Ugaritic is RS 19.16 (KTU 4.609), which is entitled spr hpr bnš nlk b yh ḫtbnm, “account of the rations of the king’s servicemen in the month of ḫtnm” (this text has now been reedited by Pardee, in press).
although *ilkū* is found in a few texts. In alphabetic texts, the equivalent term is *ilu* (pronounced *iluʃtu*), which also appears as *unuššū* in some syllabic texts in place of *pilkū* and is almost certainly a Hurrian loanword (cf. the personnel category *unuššahali* in Alalakh IV texts; see Dietrich and Loretz 1966:194–97). Heltzer (1982:16–19; 1984:169f.; 1988:10f.) argues that Akkadian *ilkū* and Ugaritic *iluʃtu* refer to the collective service obligations of the village sector, while Akkadian *pilkū* and another Ugaritic term, *ubdy* (of uncertain vocalization), describes the professional service of individual members of the palace sector. I will discuss these terms in more detail below, so it is sufficient to state here that there is no evidence in the texts for a difference in meaning among *pilkū*, *ilkū*, and *iluʃtu*. Indeed, it is only the a priori assumption of a distinction between a putative village sector and a palace sector that could give rise to such an interpretation. In my opinion, the evidence points to a single service system at Ugarit, and the term *pilkū* and its synonyms denoted the service obligation of all landholders, villagers and professionals alike.

Heltzer (1969:36) also points to the fact that in two texts—one dealing with contributions of tribute for the Hittite king and the other with the provision of bowmen—the amounts recorded collectively for the villages of the kingdom are followed by totals for various professional groups, which he takes as evidence of a distinction between village communities and royal servicemen; that is, between the village sector and the palace sector (RS 11.716 = *KTU* 4.68 and RS 19.17 = *KTU* 4.610; cf. RS 19.35A = *PRU* 6.131). But the professions listed in these texts form only a small subset of those known at Ugarit. There is no mention of plowmen, herders, sheep-shearers, watchmen, vinedressers, or any of the other agricultural professions performed by *bunuššū malki* in what Heltzer calls the palace sector. Absent, too, are most of the varieties of craftsmen known from other lists of professions. It seems, therefore, that only the higher-ranking professions paid taxes collectively. These included mostly military and cultic specialists along with merchants and metalsmiths. Such professions, like the villagers, may have had some degree of internal organization, or at least a mechanism for arranging a joint contribution (note that the provision of a single archer could apparently be shared among two or three different professions, as it was in some cases among two or three villages). But these higher-ranking professions by no means included all of those who were called *bunuššū malki*, many of whom must actually have been unskilled villagers, as we have seen. Indeed, some members of these professions themselves were village-dwellers. Perhaps it was only those who lived in the capital city whose contributions were recorded according to their profession and not their place of residence. In any case, it is important to note that these texts do not indicate a division between all villagers on the one hand and all servicemen on the other. Many professionals were villagers and all villagers were potentially royal servicemen; that is, *bunuššū malki*. The distinction in these texts is between a small set of high-ranking specialists and everyone else, farmer and professional alike.

Who “Owned” the Land?

At the most basic level, Heltzer’s adoption of the two-sector model leads him to reject the notion that the king of Ugarit was the ultimate owner of all of the land in his kingdom. Like Diakonoff, he cites royal purchases and sales of property as evidence that the king did not own all of the land (Heltzer 1969:44; 1976a:103; Diakonoff 1982:14–17). But the only “royal” purchases recorded in our texts are those of the queen-mother Tarriyelli, and there is every indication that she acted here in a private capacity, for the texts are phrased no differently from the case of any other private purchase.27 Indeed, there are other property transactions in which members of the royal family were treated like ordinary citizens. Niqmaddu II gave property to his brother Nuriy, his sister Dalaptu, and his daughter Apapa, who were all treated like any other grant recipients.28 In other words, in property matters the members of the king’s immediate family were treated legally in exactly the same fashion as other subjects and dependents of the king. But there is no indication that the king himself did not exercise ultimate proprietorship over all of the land in the kingdom. No king of Ugarit is known to have purchased land from one of his subjects; but even if there were evidence that the king paid to obtain the use of a certain plot of land, it need not imply that the crown had no prior rights over such property: the payment could be seen simply as compensation to the previous occupant for the loss of his use of a hereditary holding.

Furthermore, the so-called sales of land by the king appear in the legal texts not in the form of private sales but in the quite different form employed

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27 Purchases of property by the queen-mother Tarriyelli are recorded in RS 17.86+17.241 (*Ug* 5.159); 17.102 (*Ug* 5.160), and 17.325 (*Ug* 5.161).

for royal grants of property, with the added mention of an accompanying counter-gift or honorarium given by the recipient.29 The same is true of the one example of an exchange of property between the king and another person (RS 16.383 = PRU 3, p. 164). In many cases, in fact, it is explicitly stated that such land transfers were “gifts” given by the king even though a payment was made to him by the recipient; moreover, just as in other royal grants, the recipient’s ongoing service obligation or exemption from service is often specified, indicating that in theory at least the king remained the “landlord” of the property and was still owed “rent.” Even private sales and gifts of land between ordinary citizens were apparently subject to ratification by the king, who, at least in some cases, was said to “give” the property in question to the buyer or recipient.30 No amount of buying, selling, donating, inheriting, or exchanging of land nullified his ultimate rights, including the right to demand service from every landholder.

The main problem here is that Heltzer, like Diakonoff, fails to recognize that there is a hierarchy of rights to land in traditional agrarian societies. In the kingdom of Ugarit, for example, we may suppose that there were traditional restrictions on the king’s powers stemming from each family’s customary right to use and inherit its ancestral landholding. But this did not amount to an abstract legal right of exclusive private ownership on the part of the landholders—an alien thought in such societies. Furthermore, the fact that, in practice, the king would not usually violate traditional mores by evicting landholders and expropriating their land at will did not nullify the belief that he was the supreme landlord within his domain. Thus the distinction made by Diakonoff (and adopted by Heltzer) between the king’s rights as “sovereign” and his rights as “proprietor” is false and anachronistic. In a patrimonial state (which is how I would characterize Ugarit) the king is sovereign because he is the proprietor; the political unit and its territory are understood to be a single household of which the king is master and owner. This can be seen quite clearly in the case of the “merchants of Ura.” These foreigners were forbidden by the Hittite king to encroach on the “houses and lands of the king of Ugarit” (RS 17.130.33 = PRU 4, p. 104), even though it is quite clear that it was their acquisition of “private” estates by purchase or default that gave rise to the complaint against them.

But perhaps the term “owner” should be avoided here entirely, since it calls to mind the modern concept of private property, entailing exclusive rights to use and dispose of what is owned. The king’s “ownership” itself only consisted of a limited set of traditional hereditary rights and powers over land and persons within his kingdom, and so was analogous to the rights of a simple householder on his hereditary landholding. This is demonstrated most clearly by the fact that the Hittite overlord could (and did) take away property from a subordinate king who failed to serve him and give it to a neighboring king, in the same way that the king of Ugarit transferred property from one of his subjects to another. But even the Hittite king was merely a dependent of his gods, who were the ultimate “owners” of the land. Royal “ownership” must therefore be understood in a qualified sense. Nonetheless, the evidence is clear that, however one wishes to express it, the king of Ugarit possessed a permanent set of rights to all of the land in his kingdom. There is no need to posit a distinction between a “royal landfund” from which grants were made, on the one hand, and the “private property” of the village sector on the other. At most there was a distinction between land that the king kept for his personal use to farm for himself and land throughout his kingdom that was granted, in perpetuity, for use by subhouseholds of farmers and occupational specialists alike.

2. Royal Estates and the Crisis of the Villages

Mario Liverani’s approach to the society and economy of Ugarit is similar to that of Heltzer (see Liverani 1975; 1979a; 1979b; 1979c; 1982; 1983a; 1987; 1989). He, too, adopts Diakonoff’s two-sector model, and he agrees at many points with Heltzer’s interpretations of the texts. But Liverani has tried to make our picture of Ugaritarian society more concrete with the aid of detailed calculations and estimates based on various clues in the texts, especially in the areas of demography and economy, in the hope that vague impressions can be replaced to some extent by precise figures. Thus he discusses family size and structure in the city and the countryside; he estimates the total population of the kingdom and the proportion living in the capital as opposed to the villages; and he studies the organization and the scale of royal production and consumption. Moreover, Liverani takes


30 See RS 15.119 (PRU 3, pp. 86ff.), 15.139 (PRU 3, p. 166f.), and 16.249 (PRU 3, pp. 96f.).
the two-sector approach further than Heltzer does by adding to Heltzer’s rather static version of the model a diachronic element that accounts for the eventual collapse of the kingdom of Ugarit at the end of the Late Bronze Age.

Royal Farms and Their Workforce
Liverani has written extensively on the political and social history of Ugarit and of Bronze Age Syria-Palestine in general, but the characteristics of his approach are particularly evident in an article he wrote on the “royal farms” of Ugarit, in which he studies a few administrative texts in some detail (Liverani 1989 [original Italian = 1979a]; see also Liverani 1982). These texts reveal the existence of a number of estates scattered throughout the kingdom of Ugarit, most of which had names of the form gt GN or gt PN (i.e., the gittu-estate associated with a certain village or person). Not all of the places so named were necessarily royal establishments, in the sense of being directly administered by the palace, but many of them appear to have been managed by the palace for its own benefit because they are mentioned in palace administrative texts recording personnel, rations, tools, and livestock at various gts. The Ugaritic word gt is perhaps related to Hebrew gat, “oil- or wine-press;” but gt in Ugaritic texts seems to refer not to a press alone but to an entire estate. Furthermore, its equivalent in Akkadian texts is the word dimtu (or danna), written logographically 6AN.ZA.GAR, which denotes a “tower” (e.g., a watchtower of a field or garden) or “a fortified area in the countryside”; that is, a fortified farmstead.

The broader meaning of the word gt at Ugarit might be explained by the fact that a press was often a central feature of a rural agricultural establishment and so came to be used metonymically for the whole estate, whereas in Akkadian usage the term dimtu, “tower,” served the same purpose. This difference is understandable because presses were presumably less common in Mesopotamia, where much less olive oil and wine was produced than in the Levant. It appears, however, that the meaning of gt was not so far extended that the term could be applied to an entirely nonhorticultural establishment. Liverani (1989:146) argues that br k[ ] in RS 17.106 (KTU 4.269) was a royal estate devoted to raising sheep, which therefore had no press and so was simply called a bt, not a gt. Other royal farms listed along with gts but not called gts themselves are identified by the villages near which they were found, or by otherwise unknown place names without a gt-prefix. On the other hand, it is clear that estates with presses (i.e., gts) could also produce grain in addition to oil and wine, as Liverani’s analysis shows.

Danièle Michaux-Colombot (1997) has recently defended a different interpretation of the term gt, suggesting that the meaning “wine-press” is a secondary development in Biblical Hebrew. She notes that in lexical lists at Ebla the word gi-tum is equated with the logogram DU₈ (which is the equivalent of Akk. tillu, “hill”) and gi-tum is equated with DU₈.KI. From this she concludes that “l’idée de base est donc un habitat bien délimité, sur une hauteur et autonome” (p. 585). This meaning corresponds well with the Akkadian equivalent of Ugaritic gt in texts from Ras Shamra, namely, dimtu. Less convincing is Michaux-Colombot’s argument that gt cannot refer to a “royal farm” but always refers to “une propriété privée afranchie, ‘seigneurie’ ou ‘fief,’ plutôt qu’un domaine royal” (ibid.). Whatever its etymology, it appears that in some cases, at least, the term gt refers to royal farms that were supplied and supervised directly by the palace, and were not “fiefs” given to someone else.

33 Huehnergard (1987:128) argues that 6AN.ZA.GAR represents masc. dunnu (or perhaps even masc. Ugaritic mdll, “number,” instead of an Akkadian term) rather than fem. dimtu because it is written dunnu in RS 15.85:15 (PRU 3, p. 52f.) and never appears syllabically as dimtu in Ugarit Akkadian texts; furthermore, it takes a masculine adjective and a masculine pronominal reference.
34 See CAD D, pp. 144ff. The dimtu is mentioned in grant-texts from Ugarit as a particular building that is a component of the estate being granted, which often also includes a house, garden, vineyard, and olive grove—see, e.g., RS 15.140 (PRU 3, p. 135f.), 15.155 (PRU 3, p. 118), and 16.138 (PRU 3, pp. 143ff.). In a rare alphabetically written royal grant (RS 16.382 = KTU 3.5:7), one of the components of the transferred estate (along with a garden and a vineyard) is gih, “its gt,” which is either a press or perhaps, if gt is used here mechanically to translate dimtu, a watchtower. The equivalence between dimtu and gt in the broader sense of “farm” or “rural estate” is demonstrated in RS 20.12 (Ug 5.96), which records the number of “servants” (šīḫu practitioner) at various farms; i.e., 6AN.ZA.GAR šī-kalqa-ni-ma (ll. 0, 17) = gt skmm (KTU 4.213:3; 4.243:7); 6AN.ZA.GAR ta-ga-bi-ra (l. 4, 22) = gt tgbry (KTU 4.271:7, 9; 4.296:13); and 6AN.ZA.GAR ma-ba-ri (ll. 6, 19) = gt mēbr (KTU 4.243:12). See also Heltzer 1979a, where gt and dimtu are compared to later Greek ψηφός, “tower, farm building.” There is also an interesting Middle Eastern parallel to the gildimtu during medieval Islamic times, namely, the isolated, fortified farmsteads or agricultural compounds called in Arabic qasār. sg. qasr- (Conrad 1981).
### Table 12. Grain Production of Royal Farms in Ugarit

<table>
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<tr>
<th>RS 19.97</th>
<th>tgmra kl</th>
<th>hpr c'ltbm</th>
<th>drc</th>
<th>a drc l alpm</th>
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<tbody>
<tr>
<td>Name of farm</td>
<td>Total grain produced</td>
<td>Servants’ rations</td>
<td>Seed-grain</td>
<td>Fodder for oxen</td>
</tr>
<tr>
<td>---</td>
<td>---</td>
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<td>---</td>
<td>---</td>
</tr>
<tr>
<td>gt bir</td>
<td>1,000</td>
<td>120</td>
<td>200</td>
<td>80</td>
</tr>
<tr>
<td>gt b'ln</td>
<td>360</td>
<td>66</td>
<td>70</td>
<td>40</td>
</tr>
<tr>
<td>gt hldy</td>
<td>310</td>
<td>72</td>
<td>100</td>
<td>40</td>
</tr>
<tr>
<td>gt gil</td>
<td>320</td>
<td>72</td>
<td>80</td>
<td>40</td>
</tr>
<tr>
<td></td>
<td>800 + [x]</td>
<td>100 + [x]</td>
<td>200</td>
<td>[x]</td>
</tr>
<tr>
<td></td>
<td>300 [+ x?]</td>
<td>80 + [x]</td>
<td>80</td>
<td>40</td>
</tr>
<tr>
<td></td>
<td>[x] + 30?</td>
<td>40 + [x]</td>
<td>[x]</td>
<td>20</td>
</tr>
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</table>

<table>
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<th>hpr bns</th>
<th>drc</th>
<th>a drc</th>
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<td>Servicemen’s rations</td>
<td>Seed-grain</td>
<td>Fodder</td>
</tr>
<tr>
<td>---</td>
<td>---</td>
<td>---</td>
<td>---</td>
</tr>
<tr>
<td></td>
<td>[x] + 12 for hrâ c'rq</td>
<td>100 + [x]</td>
<td>70</td>
</tr>
<tr>
<td></td>
<td>120 + 12 for rpâ</td>
<td>[x]</td>
<td>100</td>
</tr>
<tr>
<td></td>
<td>100 + [x]</td>
<td>70</td>
<td>40</td>
</tr>
<tr>
<td>gt skmnn</td>
<td>100 + [40?] + 2</td>
<td>160</td>
<td>100</td>
</tr>
<tr>
<td>tbh</td>
<td>24 for yhâr</td>
<td>40</td>
<td>10</td>
</tr>
<tr>
<td>gt n'br</td>
<td>120</td>
<td>140</td>
<td>80</td>
</tr>
<tr>
<td>gt gil</td>
<td>180</td>
<td>120</td>
<td>90</td>
</tr>
<tr>
<td>gt alhâb</td>
<td>60</td>
<td>60</td>
<td>50</td>
</tr>
<tr>
<td>gt knpy</td>
<td>74</td>
<td>100</td>
<td>60</td>
</tr>
<tr>
<td>gt trmn</td>
<td>36</td>
<td>40</td>
<td>20</td>
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<tr>
<td>gt hdt</td>
<td>72</td>
<td>40</td>
<td>30</td>
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<tr>
<td>nhl</td>
<td>120</td>
<td>120</td>
<td>70</td>
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<tr>
<td>y'ny</td>
<td>36</td>
<td>40</td>
<td>20</td>
</tr>
<tr>
<td>c'nmky</td>
<td>12</td>
<td>20</td>
<td>[x]</td>
</tr>
</tbody>
</table>

**Sources:** Liverani 1989:130; CAT/KTU 4.636, 4.243.

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* The meaning of the term *drt* is uncertain, but that *drt* was used to feed oxen is indicated in RS 19.97 by the phrase *drt l alpm*, and Liverani deduces that the same use was intended in RS 16.395. It seems likely that *drt* is grain, although the word *drt* by itself does not necessarily refer specifically to animal fodder. It is clearly a kind of food and not, as Gordon suggested (*UT* Glossary, no. 710, p. 387), a unit of linear measure (“a span”; cf. Heb. *zeret*). Perhaps *drt* means “millet,” a translation accepted by Aistleitner (*WUS*, no. 796, p. 83; cf. Arabic *durra*, “millet; maize”). Currently, there is no archaeobotanical evidence for the use of millet in the Levant before 700 B.C., although it may have been cultivated earlier (Zohary and Hopf 1993:78–83). There is another more plausible interpretation of the term, however: *drt* may be derived from Ug. *dry*, “to winnow” (cf. Heb. *zârâ*; Akk. *zarû*; Arabic *durâ*). In that case, *drt* would be a feminine noun derived from the G passive participle (or passive verbal adjective) of the verb *dry*, and so should be vocalized *darîtu < *darîy-t* (cf. Heb. *bâsîr*, “vintage,” from *bâsar*, “to cut off [grape clusters]”). The verb *dry* occurs twice in the Ba’lu myth (*KTU* 1.6 ii 32 and v 13), in which *Anatu* splits Môtu with a sword, winnows him with a sieve (*bhtr tdrryn*), burns him (burns off the chaff?), grinds him, and sows him in the field. The entire sequence of activities involved in processing grain (including threshing, grinding some for meal, and sowing seed-grain) is in view here. In *KTU* 1.6 v 13, Môtu complains of having been winnowed with a sword (*dry* *bhrb*), but in light of the parallel passage in col. ii, it appears that there is haplography here, as Ginsberg suggested (see *ANET*, p. 141); thus in col. v also (as reconstructed), Môtu is winnowed with a sieve and split with a sword. Thus *drt* may simply refer to winnowed grain in general (i.e., grain that has been processed and is ready for use or storage), without indicating either the type of grain (e.g., wheat versus barley, which is usually specified in other administrative texts) or the use to which it was to be put. In RS 16.395 and RS 19.97, in particular, there was a need for a word for processed grain in general, in contrast to the standard term *drc*, which is here specifically winnowed grain set aside for sowing (cf. Dietrich and Loret 1991, where there is a detailed discussion of Ug. *drt* and *dry*).
Liverani focuses on RS 16.395 (KTU 4.243) and RS 19.97 (KTU 4.636), the two longest and best-preserved alphabetic administrative texts containing accounts pertaining to various royal farms. In RS 16.395 most of the 14 entries in lines 1–29 (the rest of this fifty-line text is too damaged to permit analysis) are of the form b gt GN X dr S w Y ddr w Z dd hpr bnšm, which can be translated as: “In gt GN: X (measures) are seed-grain and Y (measures) are drr and Z dd-measures are the servicemen’s rations.”35 In RS 19.97, each of the 7 entries is of the form tgmr akl b gtGN N X hpr ʾbdm Y dr S Z. drr t alpm (although the first entry, l. 4, leaves out the phrase l alpm after drr). These entries can be translated as: “Total food in gt GN is N (measures). X (measures) are the servants’ rations; Y (measures) are seed-grain; Z (measures) are drr for the oxen.”36 Liverani notes the interesting parallelism between these two texts, both of which deal with harvested grain of which portions were set aside as rations for the workers, as animal fodder, and as seed for the next season’s planting. The amounts recorded for each farm for these different uses are shown in table 12 below.

Liverani extracts information from RS 19.97 and RS 16.395 in a creative and illuminating fashion, but his interpretation is not immune from criticism. As he says, these texts were probably annual summaries by the royal administration listing the total amount of grain produced at various farms (in RS 19.97) and the amounts retained to feed the men and animals and to supply seed for the next season (in RS 16.395 and RS 19.97). By comparing the total grain production (tgmr akl) in RS 19.97 with the amount set aside for seed, Liverani calculates that the yield ranged from three- to fivefold.37 This yield is low but not unreasonable for dry-farming in this region using simple tools, especially if we consider that enough extra grain had to be set aside for seed to make up for what would have been spoiled in storage by the time planting season arrived.38 Liverani also observes that the total quantity of rations for the workers on the farms is divisible by 12 in most cases. From this he concludes that the annual ration for one worker was 12 dd (1 dd per month),39 which enables him to calculate the size of the workforce at each farm. The resulting figures (ranging from 1 to 16 workers) are reasonable in light of the actual numbers of workers at various gts recorded in another text (RS 19.45 = KTU 4.618). But even if each worker received 1 dd of grain for a month of service, we need not assume that the same workers remained on the farm throughout the year, in contrast to Liverani’s assumption of a full-time enslaved workforce.

35 The term kbd is used with numbers in both RS 16.395 and RS 19.97 with the meaning “plus” (Tropper 1997: 661f.; e.g., in RS 16.395:21, in the entry for gt Ṿrnm, the phrase Ṿlm dd b kbd means literally “30 dd 6 in addition,” i.e., “36.”

36 Following Liverani, I have read dr instead of drr for the first of what the original editor took to be two occurrences of drr in each entry of RS 19.97 (i.e., Virolleaud has “Y drr Z drr l alpm” at the end of each entry; see PRU 5 no. 13, pp. 23–25). The dr reading was earlier proposed by Rainey (1971:158), as Liverani notes. The final letter of the word in question is preserved on the tablet in only 3 of the 7 entries (ll. 4, 13, 17). Virolleaud’s copy shows the letter in each case to be t, but t and r are sometimes difficult to distinguish (see Gordon, UT 4.13 and 4.17, p. 22, and examples there). Furthermore, the combination of dr, dr, and hpr in RS 16.395 argues strongly for a similar combination in RS 19.97, as Rainey and Liverani point out, while a double occurrence of drr would be awkward. The matter can now be considered settled because RS 19.97 has since been collated by Dennis Pardee (pers. comm.), who has confirmed the sequence drr . . . drr.
Liverani also calculates the number of teams of oxen employed at each farm. The quantities of grain given to oxen are divisible by 10, not 12, and Liverani concludes that 10 dd was the annual ration for a pair of oxen. This means that an ox-team received less grain than a man, which seems odd; but Liverani (1989:133f.; 158 n. 22) argues that grain rations in these amounts would only have been a supplement to the oxen’s diet, because they would have subsisted by grazing most of the year and would have been fed grain only during the plowing season. I would add that the oxen themselves were not necessarily resident on the farm year-round, but were in all likelihood brought by corvée workers performing their temporary service at plowing time. This would have spared the palace the expense of maintaining livestock which were needed on the royal farms for only a short time each year. In any case, the figures for oxen obtained on the assumption of a 10-dd ration (ranging from 1 to 10 teams per farm) are in line with the number of workers calculated for each farm (nearly one team per worker, on average). And support for Liverani’s estimate is found, once again, in RS 19.45, where numbers of similar magnitude are recorded for teams of oxen—albeit at different gts from those listed in RS 16.395 and RS 19.97.

Another text (RS 19.52 = KTU 4.625) lists the complement of metal tools at various farms, and Liverani points out that each farm possessed only one or two of most types of tools (i.e., specialized tools like axes, hammers, and mattocks that were shared by the workers), but that the number of sickles (hrmtt) varies, perhaps in proportion to the number of men working at each farm. This is an interesting observation, and the numbers of sickles (from 3 to 16) at various farms fall more or less in the same range as the numbers of workers and ox-teams that he calculates based on the ration amounts in RS 16.395 and RS 19.97. And it is striking, as he says, that the three farms mentioned in both RS 19.52 (which lists tools) and RS 19.45 (which lists numbers of oxen and men) have an equal number of sickles and ox-teams, suggesting that there was an equal number of reapers at harvest-time as there were plowmen during the planting season.

By comparing his calculated number of workers with the total amount produced, Liverani also tries to determine the productivity of the workforce of the royal farms. He computes the average amount of seed sown by each worker and the yield per worker, concluding that each man produced enough food to support himself and 2.5 other persons—who, in the absence of families on the royal farms, would (in his view) have been other members of the palace sector in the city of Ugarit (Liverani 1989:143). By comparing the total amount of grain produced on each farm with the amounts deducted for rations, seed, fodder, and transportation costs, Liverani also estimates the size of the surplus available to the king from his own farms. This amounted to roughly 50% of the harvest, in contrast to the 10% that was taken as the king’s tithe from private farms in the village sector. In addition to cereal farming, the royal farms also engaged in horticulture and sheeprearing, and Liverani argues plausibly (although the figures he uses are necessarily speculative) that grain cultivation subsidized the production of specialized goods like wine, wool, and dairy products that were used and distributed within the royal establishment or traded abroad.

Another far-reaching conclusion that Liverani draws from his calculations is that the royal farms were staffed exclusively by adult male workers. If his reasoning is correct, not enough rations were provided to support wives and children. Each worker received 1 dd per month, which was only enough to feed himself. Without families the workforce of the royal farms did not reproduce itself; instead, Liverani argues, the royal administration preferred to keep the disbursement of rations to a minimum and to rely on regular recruitment of adult workers from the villages in order to staff the royal farms. According to Liverani, such recruitment took the form of induction into permanent servitude, which depended on the precarious economic condition of the peasantry, many of whom, having lost their land through debt or other misfortune, were forced to become royal slaves.

Liverani’s work on the royal farms is an important contribution to the study of Ugaritan society and economy. The detailed calculations he undertakes (although at many points; but these calculations serve, by their very specificity, to provoke thought about the scale of activity on the royal farms and the effect that these farms had on the rest of the economy. One of Liverani’s main objectives, in fact, is to show how the system of production that was centered on the royal farms entailed the exploitation of the “free” peasantry. He argues that the constant transfer of men and animals from private farms into what were, for the king, the more profitable farms of the palace sector resulted in a gradual weakening of the village sector and the alienation and flight of the peasantry. Indeed, there was a vicious cycle operating here, because the impoverishment of the peasantry was necessary to ensure a steady supply of dependent labor for the royal farms. The king had no incentive to liberate debtors or redistribute land because it was
much more profitable for him to draw peasants into servitude to himself than to foster private farming, from which the yield to him was much less. In the end, however, the alienation of the peasantry, combined with the increasing independence of high-ranking royal dependents, was a major factor (according to Liverani) in the collapse of the whole palace-based system—a collapse triggered by the invasion of the Sea Peoples early in the twelfth century.40

Liverani’s approach is particularly attractive because he begins with a detailed examination of some long-neglected texts, shows their significance for understanding the economy of Ugarit, and then relates his conclusions to a thought-provoking explanation of a major cultural change, an explanation that stresses internal social dynamics rather than external events. But, in the end, his elaborate reconstruction breaks down at a key point having to do with his insistence on the two-sector model and its strict separation between palace dependents and the “free” peasantry. Quite simply, there is no evidence that most of the workers on the royal farms were slaves who lived there year-round and had no families. On the contrary, it is more likely that most of them were villagers whose own land was close to the royal farm, or who left their homes only temporarily to perform their labor service. Liverani himself acknowledges that a certain amount of work on the royal farms was done by corvée workers, especially at the busiest times of year; for example, the shearsers (gżezm) mentioned in RS 17.106 (KTU 4.269) were obviously employed only for a short period.41 As he points out, in this text these temporary workers “are not known by name by the palace which merely allocates an amount of rations for the whole group” (p. 148; see also Liverani 1982:256). But the same could be said for the būnasūma and ḡabādīma (“servicemen” and “servants”) in RS 16.395 and RS 19.97, whom Liverani takes to be permanent residents of the royal farms.42

Indeed, comparative evidence suggests that corvée workers were used as plowmen during the planting season and as reapers at harvest-time, during the two periods in the agricultural year when a large number of extra workers was needed. In an Amarna letter (EA 365), Biridiya of Megiddo complained to the pharaoh that, although he had brought corvée workers to plow at Šunama, other ḡazănus (indigenous rulers governing towns on behalf of the pharaoh) were not doing the same (see Moran 1992:363). Presumably, the rulers of the Canaanite towns in the Jezreel valley were required to provide laborers to cultivate Egyptian royal farms. This practice continued into the Iron Age, for in a famous biblical passage (1 Samuel 8:12, 16) the prophet Samuel warns the elders of Israel that the king they wished to set over themselves would conscript workers to plow his land and reap his harvest, taking their servants and animals to do his work (for discussions of corvée labor in Israel and neighboring areas, see Mendelssohn 1956; 1962; Rainey 1970). Whatever the date of composition of the Samuel text, the practice of taking a tithe of the produce and requiring labor service is frequently attested in the Bible and elsewhere and clearly reflects a longstanding practice of the Bronze and Iron Ages.43

In texts from Ugarit itself there is evidence that villagers came with their oxen and donkeys to perform their labor service for the king. These texts indicate that villagers were required to provide oxen and donkeys as well as workers for royal service.44 RS 17.386 (KTU 4.308) lists the number of oxen contributed by various villages, while RS 18.99 (KTU 4.380) lists the number of donkeys and men (usually households (būnasūma) and their dependent clients (bidālua), whom they might send as deputies to perform their required service. Thus the fact that royal farmworkers could be called “servants” does not mean that they were full-time dependent “slaves” of the king, as Liverani contends (e.g., Liverani 1989:154 n. 4; 168 n. 75). On this point, Heltzer (1982:64f. and n. 139) notes that persons called ḡabādīma had families—one man in a list of ḡabādīma has an heir (nhl in KTU 4.35 12–7)—and men identified simply as unskilled ḡabādīma appear in lists with other professional categories of būnasū malki (KTU 4.35; 4.71 iii 10ff.; 4.126:13; possibly also KTU 4.183: 19f.).45

As Mendelssohn points out, 1 Samuel 8:15 refers to taxes in kind, while vv. 12 and 16 deal with the labor tax; that is, the provision of workers and animals for temporary corvée labor, not permanent servitude.

41 In an earlier article, Liverani (1975:150) himself argued that the palace kept only the minimum number of permanent farmworkers, relying on corvée labor at harvest- and shearing-time, just as the permanent staff of the army was bolstered by conscripts when necessary.
42 The parallelism between these texts indicates that the terms būnasūma and ḡabādīma were used interchangeably to refer to royal farmworkers. It is likely that the extremely common noun ḡadūtu is the more general term, denoting any person under another person’s authority. With respect to royal service, it could refer to any of the king’s subjects, of high rank or low, including both land-holding heads of households (būnasūma) and their dependent clients (bidālua), whom they might send as deputies to perform their required service.
43 As Heltzer 1976a:24–30 for a summary of the evidence pertaining to the labor obligations of the villages, but it should be noted that one text he cites (RS 11.830 = PRU 3, p. 190) may refer to numbers of workers (ēnūn) rather than days of labor (udm); cf. Huehnergard 1989:391 n.69.
the same number of each, ranging from 1 to 10) contributed by 34 different villages. RS 18.293 (KTU 4.422) is a long list of bnsm dt ît alpm ḫmn, “servicemen who have oxen.” Most revealing, however, are the exemptions from service in certain land-grant texts, which state that the recipient is free of the work of the palace (e.g., RS 16.269:14f. = PRU 3, p. 68f.), or, more specifically, that he is no longer subject to the overseer of the fields (RS 15.137:15f. = PRU 3, p. 134f.), or “he will not enter the field of the king” (e.g., RS 16.348:8f. = PRU 3, p. 162f.; here the exempted person was a murûʿu ʿibirānu, one of “Ibirānu’s commanders,” which shows that even high-ranking persons theoretically had to work in the king’s fields). RS 16.188:2’ ff. (PRU 3, p. 150) is even more explicit: “neither his oxen nor his donkeys [nor anything else?] [shall go to] work for the palace”; in other words, supplying animals as well as men was part of a landholder’s pilku service. Note also RS 15.114:12–17 (PRU 3, p. 112f.) in which an entire village is exempted from pilku service, meaning that “neither their oxen nor their donkeys nor [their] men shall go to work for the king.” Presumably the oxen were used to plow the king’s land at the time of planting (in cases where the palace did not itself supply the animals), while donkeys were needed to transport the harvest to the granaries of the capital (see Liverani 1989:141f.).

It is possible—indeed, likely—that it was just such corvéeworkers and their oxen, and not royal slaves, whose rations are recorded in the texts studied by Liverani. No doubt there were some full-time personnel resident at the royal farms; for example, the three men who are identified individually in RS 16.395 (KTU 4.243): in one case by occupation (ḥrš ṣrq, l. 2) and elsewhere by name (rpš, l. 4; yḥšr, l. 11). These men received 12 ḏd of cereal rations each, except for yḥšr, who (with his son?) was the sole worker at tbq and received 24 ḏd. As Liverani points out, 12 ḏd was probably the annual ration for an adult male, and these men may have lived on the royal farms year-round. Perhaps they served as overseers of the corvéeworkers (cf. the “overseer of the fields” in RS 15.137:15 = PRU 3, p. 134f.). But there is no proof of full-time service for the workers who are not identified individually in RS 16.395 and RS 19.97 but are called simply bunuṣṭima or ʿabadima. Liverani argues that because the amounts allotted are usually divisible by 12, rations were given to a constant number of full-time workers who each received 12 ḏd per year. But this conclusion is entirely hypothetical, and it contradicts his own statement that the workforce of the royal farms was augmented by corvéeworkers (who also received rations) during the busiest periods of the agricultural year. With a fluctuating number of workers, a total of 120 ḏd of rations for the year at a particular farm, for example, may well imply 120 man-months or 10 man-years of labor (assuming, with Liverani, a standard monthly ration of 1 ḏd); but this labor could well have been performed not by 10 year-round workers but by larger numbers working for short periods, especially during the seasons of planting and harvesting. In that case, there must have been some other reason or administrative practice which resulted in total ration amounts that were divisible by 12.

Even if we were to insist that 120 ḏd of rations, for example, means that 10 men were employed on the farm continuously for 12 months—implying that it was necessary, for some reason, for the number of workers to remain constant throughout the agricultural year—different groups of 10 men could have rotated in shifts, even though the rations disbursed by the farm stayed the same from month to month (i.e., 10 ḏd per month). If the numbers in RS 11.836+842 (KTU 4.95) are typical, 1 (lunar) month plus 5 days (i.e., 33 days) was the normal term of service for corvéeworkers in the kingdom of Ugarit; that is, slightly more than a solar month.46 Thus a full-time staff of 10 was equivalent to a dozen ten-man corvéegangs serving successively throughout the year, and the amount of rations in itself does not tell us which arrangement was in effect. As for the oxen, Liverani explains the relatively small amount of fodder that was allotted to them (based on his assumption of 10 ḏd per year for each ox team) by suggesting that they

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46 This was the term of service for 5 of the 8 villages listed in this text; for the other 3 villages the terms were 2 months, 1 month, and 15 days. Apparently, all of the workers from a given village served for the same length of time. In general, terms of service and numbers of workers are recorded by village, not individually, suggesting that villagers came together in groups for the workers who perform their service. Note that a standard term of 33 days allows 1 or 2 days at the beginning and at the end for travel to and from the place of work, leaving a month of ca. 30 days for actual work; thus 12 corvéé teams, working in shifts, could cover a full year. This calculation is somewhat speculative, of course, because we do not know whether travel time was included in the corvéé term—and no travel would be entailed if a royal farm was close to a village from which the workers were drawn. In any case, a man probably received 1 ḏd, the standard monthly ration, for a ca. 30-day period of work.
received grain only during the plowing season; but he
assumes, nonetheless, that the oxen remained on the
royal farms throughout the year and survived in the
off-season by grazing (p. 158 n. 22). Yet, as I have
noted above, there was no reason for the palace to
own and manage large numbers of oxen when it
could more cheaply and efficiently conscript them
from local farmers when it needed them.47 This was
even more the case with human laborers, who could
not subsist by grazing but had to be fed year-round.
Liverani suggests that horticulture occupied the
workers at times when cereal cultivation required
little work (p. 144). But the labor requirement for
horticulture fluctuates widely, too. Large numbers of
workers were needed for short periods to harvest the
grapes and olives and to man the presses; but these
activities could also be done more efficiently with
corvée workers than with a large permanent staff.
Horticulture and grain-growing together would not
occupy, throughout the year, a workforce adequate
for the busiest seasons.48

Furthermore, there is other evidence that the ad-
ministration of royal labor—and the payment of
“taxes” in general—was kept as simple as possible
and involved a minimum of direct supervision by the
palace. The hazanu of each village was responsible
for ensuring the delivery of goods and services to the
king and, in most cases, only the total amounts for
each village were recorded by the palace.49 In some
circumstances, it is true, the palace did keep track of
individual workers. There are a few texts in which
men assigned to various gts are listed by name (RS
15.189 = KTU 4.200; RS 17.326 = KTU 4.297; RS
Liverani (1989:135; 159 n. 26) argues that these were
destitute peasants who had become permanent royal
dependents, but nothing in these documents supports
this interpretation. He is right to say that these were
villagers: the men are identified by their villages in
one text (RS 17.326). But they could easily have been
corvée workers rather than slaves. Another text cited
by Liverani lists men who were called msry d’r rb b
unt (RS 18.118 = KTU 3.7). These men also came
from various villages, but it is clear that they were
not impoverished peasants entering royal service as
agricultural slaves. The term msry (literally, “Egypt-
ian”) seems to denote a kind of soldier and d’r rb b
unt probably means “who entered for service”; that
is, to perform the service of their profession.50 More-
over, no gr is mentioned in this text. Three other texts
that Liverani adduces as evidence of royal recruit-
ment of peasants into permanent servitude are equally

47 See, e.g., RS 15.114:12–17 (PRU 3, p. 112f.), cited above. Another text, RS 17.31 (KTU 4.296) shows that
oxen did not remain permanently on the same farm but
were assigned to various places as needed: lines 8–10 speak of ‘it alp spr dt ahld hrt, “three lean(?!) oxen that its(?)
plowmen took,” one from each of three gts.

48 Even the large estates of the Roman period, which were
manned by slaves, required additional, hired laborers dur-
ing the busiest times of the year (K. D. White 1965:104).
For his part, K. D. White (1970:370ff.) takes this as evi-
dence that the slaves were fully employed during the rest of
the year, challenging the usual view that Roman slave agri-
culture was inefficient because a permanent, tied labor
force tends to be chronically underemployed in a Medi-
terranean agricultural setting, where grain cultivation and
horticulture together do not occupy the whole year (see the
data cited in K. D. White 1965). But White offers no con-
vincing evidence for his view; moreover, the seasonal re-
quirement for extra workers need not imply that the perma-
nent workforce was fully employed—it simply shows that
Roman estate owners balked at the extreme wastefulness of
maintaining a permanent workforce large enough to ac-
complish all of the work demanded during the busiest peri-
ods. White suggests that careful management and multiple
crops kept the slaves fully employed, refusing to admit that
the amount of available work fluctuated widely over the
course of the year, and speculating instead that the slaves
were usefully engaged in repairs and handicrafts during the
winter and during occasional spells of bad weather. In re-
spose to the argument that Roman slave agriculture must have
been efficient, or it would not have been practiced, it is
sufficient to note that free, hired laborers and nonslave
dependent laborers were both scarce in this period, and
that, in any case, relative efficiency was not a considera-
tion, as long as some sort of profit was made (see Finley
1980:90ff.).
 incapable of proving this conclusion. Two of them list men of various villages with their wives and children, but there is no indication that they were destitute or that they were giving themselves into slavery (RS 18.26 = KTU 4.339; RS 18.258 = KTU 4.417). Liverani argues that the first text, in particular, provides evidence of broken families, some of whose members had fled or been banished, because half of the men in the list have no wives and children are rare; but we do not know the purpose of this text, so his interpretation is entirely speculative. The other text (RS 19.96 = KTU 4.635) simply lists a few dozen men from various places, many of them Ashdodites (referred to as “PN adddy,” who are thus not impoverished Ugaritian peasants, at any rate); but there is no way of knowing the purpose of this list or the status of the persons listed.

Finally, there is evidence that some of the men who worked on the royal farms did have families, contrary to Liverani’s interpretation. In RS 19.52:19–21 (KTU 4.625), which is the text listing tools supplied to various grs, the final entry gives the names of a man, his wife, and his son—perhaps a family living full-time on a royal farm as resident managers. Another text, RS 13.12 (KTU 4.122), is almost certainly a list of plowmen at a certain farm—the first line reads [hr]3m b gt irb3—and the first man mentioned had an heir (he is listed as “PN Iw1 nhlh”), who was serving on the farm with him. A man and his heir are also listed together in a similar tablet (RS 11.602:10f. = KTU 4.65) naming plowmen (unfortunately it is broken off on the right side), whose heading begins with lhrjm followed by a lacuna where the name of the gt would have been. What is especially interesting about this text is that the name of a village apparently follows the name of each man, although only the first two letters (at most) are preserved. In 5 of the 7 entries where the name of the village is at all legible, it begins with mr... (or, in one case, m...), suggesting that many of the plowmen on this farm came from the same place—perhaps from mrl, a well-known village in the kingdom of Ugarit.

There is reason to dispute, therefore, Liverani’s central contention that workers on the royal farms were in permanent servitude to the palace and had no families. This is not to deny the fact that some of his conclusions about the royal farms are correct. For example, Liverani’s comparison between total grain production and the amounts set aside for seed and rations leads to plausible estimates of a three- to five-fold crop yield and a surplus for the palace approaching 50% of the harvest. It is also possible, as he suggests, that part of the grain produced on the royal farms was used to feed workers who were devoted to the production of wine, oil, and wool on other royal establishments, as well as to feed military personnel and workers engaged on projects for the king in the capital city or elsewhere, although there is no unambiguous evidence of this. Moreover, Liverani is probably right to conclude that the royal farms were staffed almost exclusively by adult male workers and did not directly support a resident population of dependents, provided that we allow for the likelihood that many of these workers had homes in nearby villages and were rotated in temporary shifts. He may even be correct in attributing the eventual collapse of the kingdom to the chronic and excessive exploitation of the peasantry, who were forced into debt or flight (for which, to be sure, there is some evidence) because of their harsh economic circumstances. But there is no evidence for a large-scale enslavement of the rural population for work on the royal farms, as opposed to more traditional types of exploitation in the form of onerous taxes and conscription for service—royal exactions that may have reduced many to poverty but did not make them royal slaves. And even if Liverani were correct in his view of the workforce of the royal farms, the small number of such farms (a few dozen, among ca. 150 villages), and the small number of workers assigned to each farm (10–15 at the most, by his own calculation), indicate that the royal farms constituted only a small fraction of overall agricultural activity in the kingdom. In general, one can question Liverani’s sharp distinction between a large palace sector consisting of numerous royal dependents without means of production and a “free” village sector of independent farmers. Certain details of his reconstruction may be retained, however, without accepting the two-sector model or his elaboration of it.

The Breakdown of Village Life and the Emergence of the “Slave Mode of Production”

Aside from his work on the royal farms, Liverani has outlined the sweeping demographic and social changes that he thinks took place in Ugarit, and elsewhere in the Levant, over the course of the second millennium B.C. (see especially Liverani 1975:156–64; 1979c:1342–48; 1984). Like Diakonoff, Liverani assumes that the “urban revolution” of the third millennium had little immediate effect on the villages, which remained relatively autonomous and retained their traditional kin-based structure, continuing to be solely concerned with unspecialized production and reproduction (in other words, Bronze Age Syria-Palestine was characterized by the Asiatic mode of production; see chapter 9.2). In contrast, the specialized palace sector in the new cities, which was
rafted on top of the villages and extracted their surplus to support itself, was characterized from the beginning by nuclear families and an individualistic ethos. Unlike villagers, palace dependents did not own the means of production and so could not support large households; but neither did they need extensive kinship ties because the royal household provided all of the protection and support that they required. Liverani argues, however, that over time there was a mutual influence between the palace and the village sectors. Under the influence of the individualistic land-grant system of the palace sector, the age-old system of communal hereditary property in the villages gradually gave way to individual hereditary property—property that was both alienable and divisible—with the concomitant loss of the traditional extended-family structure of village society. At the same time, what were originally only lifetime landholdings of palace dependents became hereditary property under the influence of the village sector’s hereditary principle. Nuclear families and hereditary but alienable land therefore came to predominate in both sectors.

Liverani offers no detailed defense of this idea. He does not attempt to show from the land-grant texts, for example, a diachronic trend in the palace sector toward hereditary holdings and exemption from service (as Clayton Libolt does—see the next section). In his view, our Late Bronze Age sources reflect only the later stages of the process of mutual influence between the two sectors. They come from a time when village land was bought and sold with increasing frequency and royal land grants were given to favored dependents not just for life but on a hereditary basis, and were increasingly free of service requirements. But the overall process, as Liverani reconstructs it, is central to his theory because it explains the diachronic development in Ugarit from a viable economic system in the Middle Bronze Age to one near collapse by the early twelfth century. On the one hand, the demise of the extended-family structure of the villages deprived peasants of the traditional support of their kin in bad times, leading to an increase in debt-slavery and landlessness and eventually to the alienation of the peasantry and frequent flight into banditry. On the other hand, the tendency for high-ranking palace dependents to acquire extensive hereditary estates and to substitute monetary payment for personal service led to decentralization within the palace sector and a loss of control by the king; indeed, whole villages were taken over by the king’s men. On both counts the traditional structure of village society was disrupted and there was a general breakdown in village life, a breakdown that the king had no incentive to ameliorate through amnesties and land redistribution, as in Old Babylonian Mesopotamia, for example, because he and his high officials profited (in the short term, at least) from the increasing availability of destitute slaves and abandoned property. The great reckoning came, however, when neither the peasants nor the king’s nominal palace dependents would provide military service when the need arose. Indeed, Liverani argues that in earlier times the invading Sea Peoples would have been easily repelled or assimilated, but by 1180 B.C. the king of Ugarit, like other rulers in the region, had lost his hold on both the villagers and the military elite.

Liverani goes on to discuss developments in the Iron Age, noting that after the invasion by the Sea Peoples the palace sector was weakened for a time throughout Syria and Palestine—the dynasty and city of Ugarit, for example, disappeared forever—but that the villages of the Levant quickly reconstituted themselves on the traditional kin-based pattern and became the locus of an ideological reaction to the Late Bronze Age system. This reaction is evident in Israelite social legislation, which calls for the abolition of debt slavery, the protection of fugitives, and the periodic liberation of slaves and redistribution of land—measures reminiscent of Middle Bronze Age royal practices that had been forgotten during the Late Bronze Age. But the utopian and archaizing impulses that flourished during the partial hiatus in urban life at the beginning of the Iron Age could not arrest the sweeping socioeconomic changes that were underway. Liverani (1975:163f.) argues that the Late Bronze Age crisis of the villages actually marked the transition from one “mode of production” to another—from the two-sector situation characteristic of the entire Bronze Age since the beginning of urbanism in the third millennium B.C., in which the villages remained relatively autonomous and internally undifferentiated, to the system of “generalized slavery” in the Neo-Assyrian period and thereafter when the entire population was incorporated into the palace sector.

In terms of the traditional Marxist sequence of socioeconomic formations, Liverani is explaining here how the two-sector mode of production of the Bronze Age (i.e., the Asiatic mode of production, discussed above) constitutes an intermediate stage in the progression from preurban kin-based communialism to the full-blown slave societies of classical antiquity. He does not view the AMP as a stagnant dead-end, as some Marxists have done. Instead, he attempts to establish its Marxist credentials by explicating the dynamics within it (its “internal contradic-
tions”) that resulted in its breakdown and transformation into the slave mode of production. Liverani thereby solves the problem that had plagued proponents of the AMP—that of identifying its built-in mechanism of change—by positing a mutual interaction between its two constituent sectors, whose independent functioning had earlier been highlighted by Diakonoff, even though Diakonoff himself has refused to admit that (in terms of Marxist theory) the two-sector mode constitutes a separate, nonfeudal, nonslaveholding mode of production that others have called the AMP.

A theory as broad and internally consistent as Liverani’s version of the two-sector model is bound to be appealing, but the evidence fails to support it at certain key points. There is no indication in the texts from Ugarit that land grants to “palace dependents” were originally not heritable, so there is no reason to distinguish villagers from members of the palace sector on that score (see the next section on Libolt’s arguments to the contrary). Hereditary professions and hereditary landholdings appear to have been the norm at all times for farmers and specialists alike. Similarly, there is no evidence for a dramatic change in family structure in the Levant over the course of the second millennium B.C. The archaeological evidence discussed below in chapter 13 suggests, rather, that “traditional” extended or joint families existed among residents of the capital city (specialists or not) until the last days of Ugarit; and it is reasonable to suppose that the situation was the same in the villages. Without a fundamental distinction between two sectors, permitting one to argue that interaction between the sectors led to major social and economic changes, Liverani’s explanation of the collapse of Ugarit cannot be sustained.

Rejection of the two-sector model also calls into question Liverani’s view of the nature of the transition between the Bronze Age and the Iron Age. It is possible that the Levant experienced an increasing “bureaucratization” of royal power with the arrival of Neo-Assyrian hegemony and the emergence of a new kind of “world empire” in the first part of the first millennium B.C. But we need not accept the dubious notion, encapsulated in the identification of an Asiatic mode of production in the Bronze Age Levant, that before the first millennium the effects of urbanization were radically different, and cities were simply grafted onto an unchanging agrarian base in such a way that state-formation and urbanization had little social or economic effect on village life. Moreover, we need not accept the idea that kinship disappeared as an effective force in the villages (or, for that matter, in the cities) after the mid-second millennium B.C. There is no evidence of this sort of urban-rural dichotomy or of a qualitatively distinct sector of autarkic “free” villagers during the periods of urban civilization in the ancient Near East.

Indeed, the major methodological problem in Liverani’s application of the two-sector/AMP model to the Levant is that data which fail to fit the model do not count as evidence against it but are treated as proof of the breakdown of an earlier two-sector mode of production and of the structural transformation that eventually ushered in the slave mode of production. This procedure is followed not only in Liverani’s analysis of Late Bronze Ugarit but also in his treatment of Alalah (Liverani 1975), and in Carlo Zacagnini’s (1979b; 1984a) studies of the society and economy of Nuzi and the kingdom of Arrapḫa (see chapter 12.7 below). For example, the evidence for transfers and inheritance of land by individual persons (rather than kin-groups) in the village sector is explained as the result of the “influence” of the individualistic ethos of the palace sector in a period after the floruit of the two-sector mode of production in its purest form during the Early and Middle Bronze Ages, for which, conveniently, few texts are available. Similarly, hereditary landholdings in the palace sector are viewed as a late phenomenon resulting from the “influence” of the hereditary principle of the village sector. Thus we see the two-sector mode of production in the Late Bronze Age only at the point of breakdown and we must, in effect, infer the characteristics of the pristine and stable two-sector situation of earlier times (in the third and early second millennia B.C.) from the theoretical predictions of the AMP. In the next chapter I will offer in more detail my own view of urban-rural relations in Bronze Age Syria and Palestine, but it has been necessary to outline here my criticisms of Liverani’s approach in order to highlight the extent to which his reconstruction is based on questionable assumptions about social evolution in the ancient Near East.

3. Land Grants and the Question of Heritability

Clayton Libolt is the last proponent of the two-sector model whose work will be discussed here. In a 1985 University of Michigan doctoral dissertation Libolt undertook a detailed examination of the land-grant texts from Ugarit. He tried to apply the two-sector model thoroughly and consistently to the data in these texts—something that Heltzer and Liverani have not attempted. Libolt transliterates and translates 82 legal texts (using Nougayrol’s handcopies) and adds notes and commentary (note that a few of these texts are simply royal exemptions from service or grants of revenue and not land grants as such). He
also discusses the special terms and formulae employed for land grants as well as the structure of the texts as a whole; and he arranges the texts chronologically according to the kings during whose reigns they were written.

One of Libolt’s main concerns is to determine whether there were changes in the wording of the texts and in the nature of the grants during the 130-year period covered by these documents (from the mid-fourteenth to the late thirteenth century B.C.). He concludes that there was such a change, arguing that land grants to palace dependents became increasingly heritable and that monetary payment or outright exemption from any obligation tended to be substituted for personal service, while at the same time land grants or transfers to members of the free village sector ceased. These findings support the earlier statements of Boyer and Liverani, who had argued for just such a development. To make his case, Libolt classifies the land-grant texts from each king’s reign as either heritable or nonheritable and, within these categories, he separates grants entailing obligations on the part of the recipient from those without obligations. Finally, he divides the grants entailing obligations into those requiring personal service and those requiring monetary payment. The changing frequencies of texts in each subcategory, together with slight changes in the verbal formulae used in these texts, constitute Libolt’s principal evidence for a fundamental transformation in Ugaritian society, a transformation that he suggests was stimulated by Ugarit’s inclusion in the Hittite empire in ca. 1340 B.C.

Until the mid-fourteenth century B.C. Ugarit was, according to Libolt, a traditional agrarian kingdom; a century later it was a mercantile city-state in which the king’s patriarchal concern for the free village communities had given way to the exploitation of the land and people of the villages by an “urban elite” of wealthy merchants and other royal favorites belonging to the burgeoning palace sector. On the whole, Libolt accepts Liverani’s view of the social and economic changes that took place in the Bronze Age Levant and he agrees with Liverani’s assessment of the causes and consequences of the crisis of the villages. But Libolt differs from Liverani in arguing that traditional village kinship structures were still more or less intact in Ugarit during the reign of Niqmaddu II in the mid-fourteenth century (the date of the earliest surviving land-grant texts), and he suggests that the dissolution of the two-sector mode of production began somewhat later than Liverani supposed and was linked to the advent of Hittite rule.

Upon closer examination, however, Libolt’s interpretation of the land-grant texts cannot be sustained. His analysis of the terminology and verbal formulae used in these texts is overrefined, leading him to attach undue significance to slight scribal variations; moreover, his fundamental distinction between heritable and nonheritable grants is simply not supported by the evidence. Almost all land grants were heritable during the entire period documented by our sources. Of the 82 legal texts that Libolt studies, 55 indisputably involve heritable grants because they contain either or both of two standard duration clauses, which state that the grant is made “to PN and to his sons (forever)” (types A1 and A2 in table 13 below) and that “in the future, no one shall take it [i.e., the granted property] from PN or from his sons (forever)” (types B1 and B2). In nine more texts the sons of the grant recipient are not mentioned, but an abbreviated form of one or both of the standard duration clauses is used to indicate that the property is granted in perpetuity; that is, the grant is made “to PN forever” (type A3) and “no one shall take it from PN forever” (type B3). In 15 other texts the hereditary nature of the grant must be inferred from the overall similarity between these texts and more fully written exemplars, either because the duration clauses are abbreviated to such an extent that the heritability of the grant is not explicit—the grant is made simply “to PN” (type A4) and “no one shall take it from PN” (type B4)—or because a break in the tablet destroys all or part of the duration clauses (types A5, B5, A6, and B6). Some of these 15 grants may have been restricted to the lifetime of the recipient; for example, RS 16.260 (cf. RS 16.189) where the grant is to “PN his (i.e., the king’s) servant,” no patronym or sons are mentioned, and a one-time honorarium is paid to the king. In this case, the grant recipient may well be a royal dependent or household servant without hereditary property of his own. But because most of the other land grants for which we have documentation were clearly heritable, it is likely that a similar proportion of the ambiguous texts also record heritable grants.

51 In Akkadian texts the latter clause is usually written ur-ram šešram mamman ša šu qatu PN u šu qatē mārīšu (ana dāri dāri). In Ugaritic it is written š̄ ur-š̄ šahb bnsm I qənPN w š̄ bnh š̄ulm (RS 16.382 = KTU 3.5). In Akkadian texts, “forever” is written variously ana (or adî) dāri dāri, ana (or adî) dārīti, and ana (or adî) dārīš (see Libolt 1985:103); “forever” is left out in some cases.
Table 13. Types of Duration Clauses in Land-Grant Texts from Ugarit

<table>
<thead>
<tr>
<th>Nigmaddu II (17 texts)</th>
<th>ŠArhalba(A) and Nqmespa(N) (20 texts)</th>
<th>Ḫammattamura II (41 texts)</th>
</tr>
</thead>
<tbody>
<tr>
<td>RS 15.85 A4 B2 (r, f)</td>
<td>RS 15.88 A1 B2</td>
<td>RS 15.111 A1 B1 (e)</td>
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<tr>
<td>RS 15.89 A4 B3 (f)</td>
<td>RS 15.91 A4 B4</td>
<td>RS 15.114 A4 B6 s</td>
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<tr>
<td>RS 15.247 A3 B3 a</td>
<td>RS 15.160 A4 B4</td>
<td></td>
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<tr>
<td>RS 16.264 A1 B2 (r, N)</td>
<td>RS 16.189 A4 C (c) a</td>
<td></td>
</tr>
<tr>
<td>RS 16.263 A4 B2 (r, N)</td>
<td>RS 16.239 A1 B4 (p, A) m</td>
<td></td>
</tr>
<tr>
<td>RS 16.275 A1 B1 (r, N)</td>
<td>RS 16.264 A4 B4 C (v) s</td>
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<tr>
<td>RS 16.283 A1 B2 (c)</td>
<td>RS 16.250 A4 (p, A) d</td>
<td></td>
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<tr>
<td>RS 16.385 A5 B2/B4?</td>
<td>RS 16.251 A4 B4 (c)</td>
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<td></td>
<td>RS 16.254 A2 B2 (p, A) m</td>
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<tr>
<td></td>
<td>RS 16.260 A4 B4 (c) a</td>
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<td>RS 16.268 A6 B4</td>
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<td>RS 16.182 A1 B1</td>
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<td>RS 16.197 A1 B2</td>
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<td>RS 16.202 A1 B2 (v)</td>
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<td>RS 16.204 A1 B1</td>
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<tr>
<td></td>
<td>RS 16.184 A6 B2</td>
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<td>RS 16.178 A3 B3 s</td>
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<td>RS 16.182 A1 B1</td>
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<td>RS 16.197 A1 B2</td>
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<td>RS 16.202 A1 B2 (v)</td>
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<td>RS 16.204 A1 B1</td>
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<td></td>
<td>RS 16.238 A1 (e, S) t</td>
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<td>RS 16.242 A1 B1</td>
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<td>RS 16.234 A1 B1</td>
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<td>RS 16.236 A1 B2</td>
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<td>RS 16.282 A4 B1</td>
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<td></td>
<td>RS 16.286 A5 B1/B2 (c)</td>
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<td>RS 16.348 A2 (e)</td>
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<td></td>
<td>RS 16.382 A1 B1 (e)</td>
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<td></td>
<td>RS 16.145 A3 B4</td>
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<td></td>
<td>(grant by King “Yaqaru”?)</td>
<td>RS 16.383 A5 B3</td>
</tr>
<tr>
<td>RS 17.61 A4 B2 (c) (grant by the sākīnu of Riqdu)</td>
<td>RS 16.386 A1 (e)</td>
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<tr>
<td></td>
<td>RS 16.386 A1</td>
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<tr>
<td></td>
<td>RS 17.01 A3 B4 s</td>
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<td></td>
<td>RS 17.39 A1 B2 (e)</td>
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<td></td>
<td>RS 17.147 A1 B5</td>
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<td></td>
<td>RS 18.285 A1 B1</td>
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<tr>
<td></td>
<td>RS 19.98 A1 B2 m</td>
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</tbody>
</table>

For dates of reigns see table 11 above. RS numbers in boldface are Libolt’s nonheritable grants. RS numbers in italics are exemptions or appointments, not land grants. Note that RS 15.111 and RS 16.382 are written in Ugaritic, not Akkadian.

**Types of duration clauses:**
- A1 “to PN and to his sons forever”
- A2 “to PN and to his sons”
- A3 “to PN forever”
- A4 “to PN”
- A5 “to PN . . . [broken]”
- A6 [completely broken where this clause is expected] (A grant to Abdu son of Abdu-Rašši, a charioter and “friend” of the king)
- B1 “no one shall take it from PN or from his sons forever”
- B2 “no one shall take it from PN or from his sons”
- B3 “no one shall take it from PN forever”
- B4 “no one shall take it from PN”
- B5 “no one shall take it from PN . . . [broken]”
- B6 [completely broken where this clause is expected] (C “to PN for all the days of his life”)
- B7 (possibly Nigmaddu II)

**Special circumstances:**
- (c) grant with countergift to king
- (e) total exemption from service
- (f) grant made to a female
- (v) grant of a village
- (r) grant made to a member of the royal family
- (p) exemption from service with annual payment of silver or gold
- (A) grant to Abdu son of Abdu-Rašši, a charioter and “friend” of the king
- (N) grant to Nuriyāma brother of Nigmaddu II
- (S) grant to Sinarānu son of Siginu, a merchant
- (T) grant made to Tuppiyāna the leatherworker
- (P) grant to Pahi the Egyptian

**Noteworthy grant recipients:**
- a = arād šarrī, d = mātiš šarrī (or šarrū), i = ikkaru, k = aškāpu, m = māryānuu, q = “son of the queen,”
- r = sākīnu, s = sākīnu (i.e., "MARIM), š = šattānuu, t = tamišaru, ụ = tupašaru

Furthermore, since heritability was the norm, a nonheritable grant would probably have been identified explicitly as such. But only 3 texts specify that the grant was limited to the lifetime of the recipient (type C), and one of these texts (RS 16.244 = PRU 3, p. 93) does not record a land grant at all but rather the allotment of a village’s revenue to a local official. Thus nonheritable grants are indicated in only 2 of the 66 land-grant texts in which heritability is explicitly stated (i.e., texts with clauses of types A1–A3, B1–B3, or C).

Table 13 above shows the distribution by reign of various forms of the duration clauses among the 82 texts Libolt studied. The texts labeled in bold type are those Libolt considers to be nonheritable. Texts labeled in italics do not record land grants but rather royal exemptions from service or royal appointments. Libolt finds 22 nonheritable grants among the 82 texts, but his procedure is faulty. If sons are not mentioned he assumes that the grant is nonheritable, even if the text states that the property is given “forever” or that no one shall take it from the recipient “forever” (clause types A3 and B3).52 In most cases, however, the failure to mention sons is merely a stylistic variation resulting from the abbreviation of the standard duration clauses (types A1 and B1), just as the phrase “forever” is occasionally omitted in cases where sons are mentioned (types A2 and B2) without negating the heritability of the grant. Often, it is true, references to “sons” and “forever” are both omitted, resulting in the highly abbreviated clause types A4 and B4. Texts with these clause types are ambiguous, but there is no warrant for Libolt’s assumption that all of them record nonheritable grants: once again, the standard duration clauses indicating heritability might simply have been abbreviated. This is clearly what has happened to these clauses in the 16 texts that contain one highly abbreviated clause of type A4 or B4 along with a more fully written clause that indicates a heritable grant.53 Libolt also classifies as nonheritable all texts in which sons are not men-

52 It is true that one of the lifetime grants (RS 15.122:22f. = PRU 3, p. 131f.) is made “to PN forever (ad la dāriti), for all the days of his life,” suggesting that “forever” in grant texts where sons are not mentioned (i.e., clause types A3 and B3) might refer only to the lifetime of the recipient, even though this is not stated explicitly. But “forever” in combination with “all the days of his life” is anomalous. It occurs in only one text and might well be a scribal error; i.e., the scribe wrote “forever” from force of habit (because most grants actually were made in perpetuity) before correcting himself by adding the lifetime clause.


54 In RS 15.Y (PRU 3, p. 78) Tuppiyānu is transferred from the leatherworkers to the imittu-spearmen(?). This text is explicit that Tuppiyānu is placed “among the spearmen(?).”
But there are too many other blatant exceptions to justify retaining the two-sector separation between royal dependents who possess nonheritable land and free villagers who possess heritable land. For example, three different texts from the reigns of Niqmepa\textsuperscript{a} and ʿAmmitamtru II record heritable grants made to the wealthy merchant Sinar\textsuperscript{a} son of Siginu, who also received exemption from royal service for himself and his descendants (see RS 15.109\textsuperscript{+}, RS 15.138\textsuperscript{+}, and RS 16.238\textsuperscript{+}). Libolt (pp. 241ff.) is forced to argue that Sinar\textsuperscript{a} was not a royal official but rather a member of an independent “commercial class” that was separate from both the palace and the villages. Thus he is obliged to posit a third sector in order to save the two-sector model. But his only evidence for the existence of a separate mercantile sector is the fact that Sinar\textsuperscript{a}, although he was a merchant who would normally be considered a royal dependent from the point of view of the two-sector model, received heritable grants! Furthermore, Libolt’s third sector has to accommodate other putative members of the palace sector who were not merchants. For example, Niqmaddu II gave heritable land grants to his brother (5 texts), sister, and daughter (see table 13). It could be argued that members of the royal family constitute a special case, but there is other evidence to challenge Libolt’s thesis. ṢAbdu son of ṢAbdu-Rašī, who was not a merchant, received several heritable grants (see RS 16.143, RS 16.157, RS 16.239, RS 16.250, RS 16.254D). ṢAbdu was a maryannu-chariooteer and a müdā ("courtier?") of the king, and thus a palace dependent par excellence according to the two-sector model. Libolt (p. 222) suggests that ṢAbdu was not an ordinary chariooteer but part of an “independent nobility,” and he speculates that the heritable grants made to ṢAbdu indicate the beginnings of “feudalization” in Ugarit during the brief and troubled reign of ṢArhalba, who needed the support of such men for his fight against the Hittites.\textsuperscript{55} But the hereditary landholdings and exemptions of ṢAbdu and his sons were confirmed by ʿArhalba’s brother and successor, Niqmepa\textsuperscript{c}, who seems to have been a loyal Hittite vassal and presumably did not need to accede to untraditional demands for land and independence on the part of his military staff. Furthermore, the grant texts that name ṢAbdu as recipient follow the same pattern as other grant texts from both earlier and later periods and give no indication of reflecting an abnormal situation.

It is unclear whether Libolt’s independent third estate, in which he places both merchants and chariooteers (and presumably also the king’s brother), is a “commercial class” or a quasi-feudal nobility. In reality, of course, he includes in it only those members of the palace sector who inconveniently possessed hereditary land in violation of the two-sector model. Such men, representing various professions, are also known from several texts written in the reign of ʿAmmitamtru II.\textsuperscript{56} Again Libolt (p. 436f.) assigns these men to a “special feudalized class” drawn from the “urbanized elite”, that is, to a third sector of society independent of the palace and the villages. He argues that this third sector emerged after the reign of Niqmaddu II (i.e., after the Hittite conquest) and became especially prominent under ʿAmmitamtru II in the mid-thirteenth century. In other words, Libolt, like Liverani, finds a social and economic transformation in Ugarit leading to increasing wealth and independence among nominal palace dependents, a concomitant breakdown in village life, and, finally, the collapse of the kingdom. Unlike Libolt, however, Liverani does not try to trace this development within the texts, arguing that the transformation of the older two-sector arrangement was already underway by the time of Niqmaddu II (see, e.g., Liverani 1975:157f.). But this leaves Liverani with an argument from silence which Libolt tries to eliminate. On the basis of the two-sector model, Liverani simply assumes that grants to palace dependents must have been nonheritable in earlier times and that this restriction had largely disappeared by the Late Bronze Age, which explains the heritability of most grants in the texts we possess. Libolt, on the other hand, tries to find a substantial number of nonheritable grants and argues that the two-sector distinction between heritable and nonheritable grants remained in effect, for the most part, until the reign of ʿAmmitamtru II. There is no evidence, however, that heritable grants to members of the palace sector increased in frequency over time. On the contrary, heritable grants were the norm throughout the period attested by our texts, with very

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\textsuperscript{55} There are hints in the texts that ṢArhalba, who succeeded his father Niqmaddu II, rebelled against Hittite rule. After Muršili II reestablished Hittite control in Syria, he made a treaty with Niqmepa\textsuperscript{c}, ṢArhalba’s brother, which suggests that ṢArhalba had died or was deposed after reigning only a few years.

\textsuperscript{56} RS 15.137 (ʿašīru, müdā šarr), 16.132 (maryannu), 16.138 ("sons of the queen"), 16.162 (ša rēšī), 16.204 ("sons of the queen"), 16.242 (ʿašīru?), 18.285 (tamkāru), and 19.98 (maryannu).
few exceptions. Moreover, the assumption that non-heritable grants were more common in earlier times is questionable.

Exemptions from service or the substitution of monetary payment for service do appear to have increased over time (see table 13), although the sample of legal texts on which this is based is too small to permit us to assert this conclusion with much confidence. If this finding is valid, however, it may well indicate the increasing independence of grant recipients and a looming fiscal and manpower crisis for the king, arising from the fact that he had granted too many exemptions.\textsuperscript{57} But there is no need to maintain that a change occurred in the heritability of land grants or that “palace dependents” were treated any differently than “free villagers” in this regard. In other words, there is no need to posit two sectors in Ugartic society, let alone three. The assertion made by proponents of the two-sector model that royal grants to palace dependents were originally non-heritable is entirely a deduction from the model; it is not supported by the evidence. Despite Libolt’s best efforts, the two-sector model cannot be made to fit the data of the land-grant documents. I have discussed his arguments in some detail, however, not only because of the importance of the land-grant texts for understanding the royal service system of Ugarit, but also because Libolt’s work demonstrates the problems inherent in ideas briefly sketched by Heltzer and Liverani but not subjected by them to a rigorous test.

4. Two Sectors or One? The Terminology of Royal Service in Ugarit

At this point it will be useful to take a closer look at the special terms used in texts from Ugarit to describe the royal service obligation and those who performed it. The Ugartic term \textit{bunušu} (\textit{maliki}), “man (of the king),” was discussed in some detail above and need not be dealt with again here. It is sufficient to recall that this term does not refer exclusively to members of the so-called palace sector but includes any landholder who performed service (often part-time service) for the king, whether he was a member of a professional group or an ordinary farmer, and whether he lived in the capital city or in a rural village. We must examine, however, the Akkadian words \textit{ilkû} and \textit{pilku}, which refer to the service performed by landholders, and the related Ugartic terms \textit{ubdy} (vocalization uncertain; probably \textit{ Ubadi}, if it is derived from Luwian and Hittite \textit{ubdi}, as seems likely) and \textit{unt} (pronounced \textit{unutû}; written syllabically \textit{u}–\textit{nUt-ša} [accusative] in Akkadian texts—see Huehnergard 1987:108). Under the influence of the two-sector model, Heltzer (1982:23–37) distinguishes the pair \textit{ilkû} and \textit{ubdy}, on the one hand, from \textit{ilkû} and \textit{unt}, on the other, arguing that Akk. \textit{ilkû} and Ug. \textit{ubdy} pertain to the professional service performed by members of the palace sector, while Akk. \textit{ilkû} and Ug. \textit{unt} refer to the corvée obligations of the unskilled village sector (in which royal servicemen were occasionally involved as well). Originally, according to Heltzer (p. 35), \textit{pilku} denoted a “delimited land-plot” (cf. Akk. \textit{palâku}, “to delimit, divide”) and so is semantically equivalent to \textit{ubdy}, which refers to the land granted to royal servicemen. The Akk. term \textit{ilkû} in texts from Ugarit, on the other hand, is equivalent to \textit{unt}, in Heltzer’s opinion, because both are etymologically associated with verbs meaning “to go.”\textsuperscript{58}

The evidence does not support a distinction in meaning between these sets of terms, however. Whatever their etymology, the terms \textit{pilku}, \textit{ilkû}, and \textit{unt} are used in identical contexts in legal texts from Ugarit. They are synonyms referring to the service owed by landholders to their overlord without regard to the status or profession of the landholder. The normative Akkadian term \textit{ilkû} has, for some reason, been replaced by \textit{pilku} in most cases in the legal texts from Ugarit, but the few occurrences of \textit{ilkû} that do exist show these words to be synonyms.\textsuperscript{59} There is no

\textsuperscript{57} It is also possible, however, that the rate of exemption from service did not increase, but that there was a change in legal practice during the thirteenth century, as Márquez Rowe (1999) has suggested. The change in the frequency of the clause stating that no \textit{ilkû} service was to be performed might be the result of a legal reform during the reign of \textit{Ammitamru II} whereby the requirement to perform \textit{ilkû} service was implied and was automatically in force unless an exemption was explicitly stated, whereas previously, during the reign of Niqmaddu II and his immediate successors, exemption from \textit{ilkû} service was implied and it was the requirement to perform \textit{ilkû} that had to be explicitly stated.

\textsuperscript{58} This is well established for Akk. \textit{ilku} (cf. \textit{alâku}). Like many other scholars, Heltzer associates \textit{unutû} with the Hurrian verbal base \textit{un-} (“to come”), an etymology originally suggested by Speiser (1955:162); see Huehnergard 1987:108 and the references there.

\textsuperscript{59} The term \textit{pilku} occurs in about 40 texts (royal land grants, exemptions, and appointments), while \textit{ilkû} occurs in 4 texts written at Ugarit: RS 15.143–164:10 (\textit{PRU} 3, p. 117), 15.155:26 (\textit{PRU} 3, p. 118), 15.140:22 (\textit{PRU} 3, p. 135f.), and 19.32:4, 9 (\textit{PRU} 6.77). Note also that the regular service owed by the king of Ugarit to his Hittite overlord (from which he was exempted) was called \textit{ilkû} by the Hittite scribe in RS 20.212:5 (\textit{Ug} 5.33). For the equation \textit{ilkû} = \textit{pilku}, see Kienast 1980:53 and Huehnergard 1987:168.
indication in texts from Ugarit that pilku ever meant “a delimited land-plot”; in all cases it refers to the regular service obligation of the landholder (like ilku), not to the landholding itself. The equivalent Ugaritic term is ʿunṭ, which appears as ʿu-nu-ušša in three Akkadian texts in place of expected (p)ilku, and is also found in Ugaritic legal texts as the equivalent of Akkadian (p)ilku.⁶⁰

Furthermore, the Ugaritic term ʿubdy cannot be equated with pilku and placed opposite ʿunṭ to denote the service of the palace sector as opposed to the service of the village sector, as Heltzer would have it. The word ʿubdy appears only in alphabetic administrative texts and does not refer to the service obligation at all but rather denotes the landholding (or perhaps the landholder himself, if the -y suffix be interpreted as a gentilic). There is no convincing Semitic etymology for ʿubdy. Gordon (UT Glossary no. 17) cites Arabic ʿabadyun, “perpetual” (so also Aistleitner in WUS no. 15); hence, “perpetual land grant.” But it is more likely derived from Hittite upati, which itself is a loanword from Luwian ubadi < *ubadid-i (or *ubadid-i), meaning “landed property” (see Starke 1990:195-98; 1993:21).⁶¹ In Hittite texts (_corners-pa-ba-(a-)i) seems to describe landholdings; it is translated “feudal estate” by Güterbock (1956:135; 1957:360; cf. Friedrich 1952:235), who also connects this term to Ugaritic ʿubdy.

The same term appears in Old Assyrian texts from Kültepe (ancient Kaneš) as ubadinnum (see AHw p. 1423 s.v. “upatinnum”). On the basis of the Luwian-Hittite derivation of the term, Starke (1993:21) revises Lubor Matouš’s (1979:38) definition of ubadinnum as “eine Menschengruppe einheimischer Personen, die auf dem Gemeingut arbeiteten und lebten,” which Matouš had argued on the basis of a Kültepe sale contract (I 568).⁶² This contract lists the names of several persons (in addition to the buyer and the seller) followed by the phrase ki-ma ū-ba-di-nim, “representing the ubadinnum.” For Starke, in contrast to Matouš, this does not mean that these named persons were the legal representatives of a larger co-resident group known as an ubadinnum (collective singular)—a group which, according to Matouš, jointly owned the property being sold, which was itself referred to simply as a betûm, “house.” For Starke this phrase means, rather, that the property being sold was an ubadinnum and the persons named in the contract as acting on behalf of the ubadinnum had some sort of stake in this property (very likely as kinsmen of the seller, as Matouš suggests) and so were involved in effecting the sale. Starke therefore translates kima ubadinnim in this text as “für die/in Vertretung der Domäne,” arguing that the suffix -im(m) does not create a noun of agency or an adjective denoting possession or group membership, but merely indicates a non-Semitic loanword (Starke 1993: cf. Bilgic 1951:17 n.123); thus Old Assyrian ubadinnum, like Luwian-Hittite ubadi, means “landholding,” not “landholder(s).”

The Ugaritic form ʿubdy, however, might reflect the addition of a gentilic -y suffix to the non-Semitic base ubadi- in order to describe a landholder; that is, one who holds ubadi-land (cf. Hittite upatinyaš, cited by Starke 1990:198). If not, then we must posit a consonantal glide between ubadi- and the Ugaritic case vowel (i.e., ʿubadi-y-û/i/a), and translate ʿubdy as “landholding” (Ignacio Márquez, pers. comm.). Yet landholders appear to be intended in RS 17.389 (KTU 4.309), where the heading [s]pr ubdym b uškn, “[list] of ubadi-men(?) in the village of uškn,” is followed by 30 personal names. More ambiguous is the reference in RS 16.396:9 (KTU 4.244) to ūt krm ubdym, which means either “3 ubadi-vineyards” or “3 vineyards of ubadi-men.” The use of the bound form krm in the plural (as opposed to the unbound plural krm, attested in line 25 of the same text) shows that ubdym is not employed here adjectivally to modify krm; it is either an attribute of krm or indicates simple possession (cf. also zt ubdym in RS 15.75:3 = KTU 4.164, “ubadi-olives/olive-trees” or “olives/olive-trees of ubadi-men”). This has been pointed out by Gordon (UT Glossary no. 17; see also WUS no. 15 for the meaning “landholder” as opposed to “landholding”). The word ʿubdy also occurs several times in the headings šd ʿubdy GN or (šd) ʿubdy ON (= occupation

⁶⁰ For syllabic ʿunṭšu see RS 15.89:20 (PRU 3, p. 53), 16.156:15 (PRU 3, p. 69f.), and 16.167:17 (PRU 3, p. 62f.). For ʿunṭ used in alphabetic texts to translate (p)ilku, see RS 15.125:2; 5 (KTU 2.19), 15.111:18 (KTU 3.2), 16.191+16, 19 (KTU 3.4), and 16.382:20 (KTU 3.5); these texts are discussed by Kienast 1979 (esp. pp. 443ff.).

⁶¹ I owe this reference to Ignacio Márquez. Note also the Ug. variant updti with p instead of b in RS 17.52:1 (KTU 4.264), where the heading spr updt d bd mlkyn, “list of updti-personnel who are in the charge of Mlkyn,” is followed by 8 personal names; cf. also the heading labdut in RS 1.10:14 (KTU 4.12), followed by 2 names.

⁶² It is worth noting that Matouš is here attempting to apply Diakonoff’s two-sector model of Bronze Age Near Eastern society, arguing that this contract deals with the sale of rural “communal” land that is owned collectively by a group of kinsmen, who are designated by the Anatolian term ubadinnum. From the perspective of this model, such
which is called profession, taking "17.389 (cited above), ubdy ubdy might instead refer to "the fields of the ubadi-men" of a certain place or profession, as this phrase is usually rendered, taking ubdy to be a type of property; but if ubdym denotes landholders in RS 17.389 (cited above), šd ubdy is followed by entries of that form. Thus šd ubdy in these texts could mean “the fields of the ubadi-fields” of a certain place or profession, as this phrase is usually rendered, taking ubdy to be a type of property; but if ubdym denotes landholders in RS 17.389 (cited above), šd ubdy might instead refer to “the fields of the ubadi-men” of a certain place or profession, taking ubdy to be a type of person. In any case, ubdy does not refer to the service obligation, which is called umr in alphabetic texts.

Advocates of the two-sector model associate ubdy-land exclusively with members of the palace sector, who were supposedly compensated for their professional services with temporary land allotments taken from a “royal landfund,” as Heltzer puts it. Privately owned hereditary land in the village sector was thus entirely separate from ubdy-land according to this view. But as we have seen, ubdym are listed by village as well as by profession. To explain this Heltzer (1976a:69) suggests that the local village administration “managed the fields” of the royal landfund on behalf of the king and his servicemen, and perhaps was responsible for harvesting the crops. A less awkward interpretation of the textual evidence, however, does away with the two-sector distinction between “private” village lands and a “royal landfund” from which plots were distributed to royal servicemen. Thus ubdy land was held by men of all kinds, including both occupational specialists and ordinary villagers. Moreover, ubdy-land was clearly hereditary. RS 19.72 (KTU 4.631), which has the heading spr ubdy art, “list of the ubdy-men/holdings(? of the (village) art,” records landholding arrangements involving various persons. Of the 20 entries in this list, 10 are of the form šd PN bdll PN2 nhīh, “the field(s) of PN are in the hands of transferred to PN2 his heir.”

63 For villages: see RS 12.06 (KTU 4.110), where the heading šd ubdy ššn-Š dt bd skn is followed by entries of the form šd PN bd Š PN2; also RS 19.72 (KTU 4.631), where the heading spr ubdy art is followed by entries of the form šd PN bdll PN2 (cf. the badly damaged text RS 18.296 = KTU 4.424 with the heading spr [.šd rišym], “list of the fields of the [men of] riš”). For occupations: see RS 11.858 (KTU 4.103), where the headings ubdy mdm, ubdy mrim, ubdy ṭgm, ubdy Šrm, ubdy nqdm, and so on, are each followed by entries of the form šd PN bd PN2; also RS 20.145 (KTU 4.692), where the heading ubdy yšlm is followed by 6 entries of the form šd PN l PN2; and KTU 4.7, where the heading ubdy trmr is followed by 19 entries of that form.

64 In a similar vein, Postgate (1971:508) would like to erase the distinction made for Middle Assyria by Garelli (1967:6–14) between “private property” and royal land granted to private persons.

and 8 are of the form šd PN bdll qrt, “the field(s) of PN are in the hands of transferred to the village.”

Apparently the royal administration recorded the names of villagers who were currently in possession of hereditary landholdings. Often enough a man’s son or other heir succeeded him as landholder, but in certain cases—presumably when no heir was available or willing to take the land—it reverted, temporarily at least, to the village as a whole. Perhaps the king was free to transfer such land to another individual landholder at a later date, but in the meantime (if my interpretation is correct) the inhabitants of the village were collectively responsible for cultivating the unassigned land and giving a share of the crop to the king. Indeed, if the Akkadian term nayyālu refers to a landholder without heirs and not a "defaulter," as it is usually rendered, then a number of the land-grant texts from Ugarit reflect just this situation; that is, they officially record the transfer of the former holdings of men without heirs to other landholders.

It is worth digressing here for a moment to consider the meaning of the word nayyālu (ḥna-ia-lu), which is used in 14 land-grant texts from Ugarit to describe men whose land was transferred to someone else. Following Nougayrol (PRU 3, p. 29; cf. Heltzer 1976a:52–57), the original editor of these texts, most scholars translate nayyālu as “defaulter”; that is, someone who failed to fulfill his obligations and so forfeited his land. This is a plausible interpretation, but in the course of a discussion of Middle Assyrian land tenure, Nicholas Postgate (1971:509) suggested instead that nayyālu, in the Middle Assyrian kingdom and in Ugarit, referred to a landholder who "had no...

65 The preposition bd indicates oversight or management and the preposition l indicates a transfer of some kind. There are 2 other entries in this text (in ll. 13 and 19) that are probably to be read šd PN bn ann .dtd, “the field(s) of PN son of ann—dtb.” In line 19 Gordon (UT 2029) reads šd kmn[,]bd ann[,]bd and he translates ʿdb as “agent” (UT Glossary no. 1818) on the basis of the Ug. verb ʿdb, “to make, prepare, set”; i.e., “the field of kmn is in the care of ann, an agent(?)”. Line 13 is broken and Gordon reads šd kmn[,]bd ann[,]bd in l. 19; and in l. 13 the correct reading is šd kmn[,]bd bn ann[,]bd (based on a collation by Dennis Pardee, pers. comm.). Thus it appears that two sons of the same man have fields that are called ʿdb (or the man himself is ʿdb), although the precise meaning of this term is unclear.

heir to inherit the concession on his eventual death.” He retracted this interpretation in a later article, with respect to Assyria (Postgate 1982:311), but there is still much to recommend it with respect to the use of the term at Ugarit. Nowhere in the land-grant texts from Ugarit is there any hint that a nayyālu was at fault or had lost his land under unfavorable circumstances. In RS 15.89 the grant recipient is the niece of the former landholder (i.e., his brother’s daughter), who was a nayyālu. It is odd that a defaulter’s land should have been given to a close relative (and a woman at that), especially since the writer of the document assumes that she will give it away or sell it and not work it herself (see lines 10–16). Yet if the nayyālu was not a defaulter but simply a man without male heirs, it makes sense that the land should go to a female relative. Similarly, in RS 17.61 the sākīnu (MAŠKIM) of the town of Riqdu transfers the landholdings of two women—the daughters of two nayyālus—to another man in return for 300 shekels of silver. It is not clear whether the women or the sākīnu received this payment, but if the women did receive it, then it appears that they were selling property inherited from their fathers, perhaps because they could not work it themselves. Moreover, the fact that they were in possession of the property in the first place suggests that their fathers had no male heirs. In another text, RS 16.174, it seems that the land of a nayyālu was first purchased by the new landholder and only then was the land transfer ratified as a “grant” from the king—again, an unusual procedure if the nayyālu was a defaulter who had forfeited his land. Furthermore, RS 15.145 states that there was no service obligation on land transferred from a nayyālu to another landholder. Unless this was a new exemption for the new landholder, failure to perform royal service could not then have caused the loss of this nayyālu’s land. Finally, it is worth noting that in RS 19.32 the term nayyālu is not used to describe men who refused to perform their ilku service, as might be expected if that was its meaning. Instead, the scribe employs the circumlocution sābū la alikā (written la-li-ku) ša ilki (ll. 8–9), “group who did not perform ilku.”

To sum up: in no nayyālu text from Ugarit is the translation “defaulters” required; moreover, if the term nayyālu instead denotes a landholder without male heirs, the king’s role in these and other land transfers becomes clearer. As the “father” of his kingdom and the ultimate landlord, the king intervened in cases where normal hereditary transmission of landholdings was interrupted or where local customs otherwise proved inadequate. Judging by the types of legal texts found in the palace of Ugarit, royal intervention was necessary not just in the absence of male heirs but also to approve unusual inheritance arrangements, adoptions (with subsequent inheritance of the landholding by the adopted person), and the sale or other form of alienation of family property. This is not to say that royal expropriations of land from defaulters or miscreants did not occur, but many of the “land grants” recorded in our texts were probably not initiated by the king at all but were simply royal ratifications of unusual transactions (i.e., not involving normal hereditary succession) that arose at the local level. Most of the legal acts of the king, especially those involving the sale or inheritance of property, exemptions from service, and changes in professional status, were probably executed in response to odd situations of various kinds for which local customs were inadequate. The vast majority of ordinary hereditary transfers of landholdings, occupations, and service obligations no doubt remained unrecorded.

Returning now to the alphabetic text RS 19.72 (KTU 4.631) that lists the ubadi-men (or ubadiholdings) of art, we must compare it to several other texts that also possess entries of the form šd PN bdll PN2, “the field(s) of PN are in the hands of PN2,” although none of the entries in these other texts indicates that PN2 is the heir of PN or that the village (qrt) has taken charge of PN’s land.68 There are two possible interpretations of such texts: either they record permanent transfers of land from the former landholders to their heirs or to other unrelated men, or they register some kind of tenancy or stewardship arrangement in which PN has not died or otherwise lost his landholding but has handed it over to PN2 to care for it temporarily while he engages in other activities (e.g., performing his royal service).69 The first

67 Nougayrol (Ug 5, p. 13, n. 1) argues that the women were called nayyālus, not their fathers. The text is ambiguous, but the determinative ši suggests that their fathers were intended.

68 The prepositions bd and l are apparently used interchangeably in these texts. See KTU 4.7, RS 11.858 (KTU 4.103), 12.06 (KTU 4.110), 16.193:18–21 (KTU 4.222), 18.46 (KTU 4.356), 18.295 (KTU 4.423), 18.297 (KTU 4.425), and 20.145 (KTU 4.692); cf. RS 17.246 (KTU 4.282) and 18.47 (KTU 4.357), which have entries of the form: (X) šd(m) dbd PN.

69 Landsberger (1955:126), in his classic study of the archive of Ubarrum, draws attention to the arrangement made between Ubarrum and his brother whereby one cultivated the other’s field while his brother was away performing ilku-service. Apparently no formal contract was drawn up for this, but the reluctance of Ubarrum’s brother to fulfill his part of the bargain led Ubarrum to initiate judicial proceedings in order to guarantee his brother’s compliance.
interpretation seems more probable for RS 19.72 (KTU 4.631), with its frequent references to heirs; but even here it is possible to understand the heirs (or, in their absence, the village as a whole) as temporary caretakers and not permanent owners of the land. In any case, the second interpretation better suits other texts of this type in which it is recorded that the fields of different landholders (sometimes of differing professions) were placed in the care of the same person. For example, PN₂ is gmrd in 10 entries in 3 texts (KTU 4.7:10, 11, 15; KTU 4.103:14, 23, 24, 49, 50; and KTU 4.692:2, 7), where PN is variously a member of one of the following professions: trrm, mdm, mrun, or yšm. Several other names also appear two or three times as PN₂ in different entries in these lists. It is possible that these were caretakers appointed by the palace to manage the fields of certain high-ranking specialists. As a rule, however, it seems likely—given the complexities of personnel management that would have resulted if royal supervision were extended to every landholding in the kingdom—that such arrangements were made locally and were merely recorded after the fact by the royal administration, which needed to know who was looking after the land and who was therefore responsible for deliveries to the palace. Heirs are clearly not involved here, and it is doubtful that these texts recorded permanent land transfers. It is important to remember that these were ephemeral administrative lists, not legal documents; it is therefore more likely that they registered the temporary assignment of responsibilities rather than permanent transfers of land, which we know to have been recorded for posterity in formal legal texts written in Akkadian and sealed by the king.

However we interpret the formula šd PN bd PN₂, it is clear that the ubdy-holdings mentioned in Ugaritic administrative texts were the same as the landholdings granted by the king that are recorded in Akkadian legal texts—as even Heltzer admits is the case, at least for land grants to members of the “palace sector.” In other words, the simplest and most consistent interpretation of the evidence found in both the Ugaritic and the Akkadian texts is that there was one land-grant system in operation in Ugarit, not two, and there was no legal distinction between land held by “free villagers” and land held by “palace dependents.” All landholders owed the king service, which was called (p)ilku in Akkadian and ṣanūtu in Ugaritic. This service obligation involved the provision of both goods and services, as the specific exemptions in certain land-grant texts make clear. Goods delivered to the palace included oil, wine, grain, and livestock; and the required services included unskilled corvée labor, military duty, or specialized work in various agricultural, craft-related, and professional tasks. It might be argued—based on the etymology of ilku, which is related to the verb alāku, “to go”—that the term (p)ilku included only military or labor service, and did not include payments in kind. But the verb used with pilku (and ṣanūšu) is (wa)babu, “to bring, carry” (see Huenenggard 1989:178f.), suggesting that the regular delivery of goods was also involved. The same verb is used in relation to annual silver payments made in lieu of other service (e.g., RS 16.157:21 = PRU 3, p. 83f.); i.e., monetary payment could be substituted for pilku-service. And note that, in Mesopotamia, ilku referred not just to “services performed for a higher authority in return for land held” but also to “delivery of part of the yield of land held from a higher authority, also payment in money or manufactured objects in lieu of produce” (CAD, s.v. “ilku A”).

In this regard, it is worth emphasizing the occurrence of the phrase b’l šd, “field-owner(s),” in two alphabetic texts: RS 15.116:1 (KTU 4.183) and RS 19.16:53 (KTU 4.609). These texts have recently been reedited and their structure discussed in considerable detail by Dennis Pardee (in press), in light of a new text, discovered in 1994, which refers to n’rm b’l šdm (RS 94. 2439). The men who are called owners of fields were themselves royal servicemen (bunušu malki), but far from receiving rations, they were required, in return for their possession of landholdings, to contribute grain to be used as rations by other royal servicemen of various professions, who were perhaps full-time palace personnel without landholdings of their own. Here we

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70 This is implied by his equation of ubdy with pilku (see Heltzer 1982:36). Libolt (1985:24), however, argues that the ubdy-system was completely separate from the landgrant system; but this view depends on his rather implausible argument that hereditary land grants and pilku-service did not apply to palace dependents (i.e., occupational specialists) at all—see my critique of his views above. This view also depends on the dubious notion, inspired by the two-sector model and shared by both Heltzer and Libolt, that ubdy-land was not hereditary and was not held by ordinary villagers as well as professionals.


72 In this context, the n’rm (“servants?”) who are “owners of fields” are a category of royal personnel.

73 This is suggested by the predominance of relatively low-ranking military, cultic, and administrative personnel among the ration recipients listed in RS 19.16, who received rations supplied by other royal servicemen. These recipients include: four military “chiefs of tent” (rb 5r) and
have a glimpse of how the palace redistribution system was integrated with the land-tenure system, functioning in a simple and quite efficient manner by means of obligatory transfers directly from one group of servicemen to another. Pardee concludes that:

Ces deux textes [RS 15.116 and RS 19.16] fournissent les principales données actuellement disponibles sur le phénomène de la possession de champs par des membres du personnel royal; les données des nouveaux textes s’alignent à côté de celles-ci sans grand complément d’informations. RS 15.116 illustre le fait qu’une quarantaine des membres d’au moins treize catégories du personnel, vraisemblablement des fonctionnaires royaux en service permanent au palais, possédaient un terrain en propriété privée. . . . En revanche, si notre interprétation structurelle de ce texte [RS 19.16] est correcte, ces propriétaires, au nombre de onze, étaient responsables des rations en céréale(s) d’une partie du personnel, en service au palais royal pendant le mois de TTTBNM d’une année non identifiée. . . . Selon notre interprétation de RS 19.016, il est vraisemblable que les autres listes de propriétaires de champs recensées ici étaient dressées selon la même perspective, pour permettre à l’administration royale d’exploiter efficacement la source de revenus qu’êtaient ces propriétaires fonciers. [“Conclusions générales” in Pardee, in press]

As I argued above in my discussion of Georges Boyer’s work, the only difference between occupational specialists and non-specialists, as far as the royal service system was concerned, was that specialists usually contributed labor service of a kind appropriate to their skills and were therefore frequently exempted from ordinary kinds of unskilled service. The extant land-grant texts mention ten different kinds of specialized pilku-service:

1. pilku ša aškāpi, “service of the leatherworker” (RS 16.142:8f.)
2. pilku inimitūti(?), “service of the spearmen(?)” (RS 15.Y:16; written lā:me/ZA.G.IU-III)
4. pilku marī tibīrāna, “service of the commanders(?) of ‘Ibirānu” (RS 16.348:5)
5. pilku ša māri šarratī, “service of the ‘sons’ of the queen” (RS 16.138:35; 16.204:10f.)
6. pilku ša lá:meša rēšī, “service of the ša rēšī-men” (RS 16.162:23; note that ša rēšī does not mean “eunuch” at Ugarit)
7. pilku ša tamkāruttī, “service of the merchants” (RS 18.285:9f.)
8. pilku ša maryannītu, “service of the charioteers” (RS 19.98:22f.)
9. pilku ša šatammī, “service of the clerks” (RS 15.122:30; 17.01:30)

Presumably, every specialized (nonfarming) occupation had its own form of service. In addition to occupationally based service, there was ordinary un-specialized pilku-service, which was called pilku ša bitti, “service of the house,” or pilku ša eqlī, “service of the field.”

Specialists and non-specialists alike received royal rations during the time they were engaged on royal service, as we have seen, but most landholders, whatever their occupation, probably served only part-time or sent assistants on their behalf because they had households of their own to manage. Contrary to the two-sector model, the king was the ultimate “landlord” and every landholder was in some way part of the same royal service system and paid “rent” to the palace. And the king of Ugarit in turn was merely a “tenant” of his own overlord, the Hittite king, to whom he owed goods and services that were likewise implied ildo (see RS 20.212:5’ = U 5.33). As an alternative to the two-sector model, then, I favor a picture of Ugaritan society that bears some resemblance to the unitary, hierarchical feudal model, but dispenses with certain characteristics, peculiar to medieval Europe, that are implied by feudal terminology. Such a picture is provided by the patrimonial household model, which was first proposed by Max Weber as a suitable description of numerous premodern societies, including those of the ancient Near East.

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4. Libolt (1985:118–124) attempts to distinguish these as separate assessments on the “household” and on the “land,” but these phrases are almost certainly synonymous. As elsewhere, Libolt’s analysis of the terminology of the landgrant texts is overrefined.
5. *Patrimonial Administration in the Kingdom of Ugarit*

The edict of the Hittite ruler Hattušili III (RS 17.238 = PRU 4, p. 1071f.), which is repeatedly cited by Heltzer as proof of the two-sector model of Ugaritan society, actually provides evidence of the hierarchy of households envisioned in the patrimonial household model.° This text refers to “any servant of a servant of the king of Ugarit,” revealing a social hierarchy consisting of the king, his immediate servants, and their servants. The king of Ugarit himself was likewise the servant of his Hittite master; thus the patrimonial hierarchy of households extended as far as the imperial court. The social and political organization of vassal kingdom and empire alike was structured on the simplest possible basis as a self-replicating household of households, in which the social actors’ understanding of household-based relationships of authority and obedience remained the same at every level.

If Ugarit was indeed a patrimonial regime, the existence of official titles and delimited duties should not be taken as evidence of the operation of an impersonal bureaucracy, a point that I will make in relation to other Bronze Age Near Eastern polities below in chapter 12. Although we call certain men “officials,” we should not imagine that their “offices” could be easily separated from their personal status and the constellation of social relationships in which they were embedded. For example, the *ḥazannu* (an Akkadian term almost certainly to be equated with Ugaritic *rabbu qariti*, “chief of the settlement,” sometimes abbreviated as *rabbu*) governed a subsidiary town or village in the kingdom of Ugarit. In my view, it is unlikely that this official was a non-indigenous royal appointee, as Liverani (1975:153ff.) asserts.°° Although there is no direct textual proof one way or the other, comparative ethnographic evidence suggests that such an official was normally a local headman or leading householder whose role as liaison with the palace was simply ratified by the king, who relied on the official’s prestige and influence among his neighbors to ensure that royal commands were obeyed and service obligations were fulfilled. The king might depose a *ḥazannu* who was not doing his duty, but he would not normally appoint an outsider to this office.

Liverani disputes this, arguing that the *ḥazannu* belonged to the palace sector and so was a focus of discontent and hatred among rural inhabitants—“un corps étranger, comme un ‘terminal’ du mécanisme de perception des impôts du palais” (ibid., p. 154). In defense of this view he cites a Ugaritic text (RS 15.22+ = KTU 4.141 iii:3) in which a *rabbu qariti* is listed among the “men of the king” (*bunūša malkī*), and he notes that in the Alalāḫ IV texts we hear of *ḥazannus* who are also *maryanne*-charioteers. But unless we adopt the two-sector model a priori, with its basic dichotomy between royal servicemen and unspecialized rural villagers, we should not be surprised to discover that native administrators with deep roots in the local community were regarded as “men of the king” and practiced prestigious specialties like that of charioteer.

Liverani also cites the attacks on *ḥazannus* recounted in Amarna letters sent to Egypt by Rib-Hadda of Byblos (EA 73, 74, 89). The circumstances of these letters were unusual because rebellion was being fomented in the area by ʿAbdi-Ašīra of Amurru; but even in the Byblos letters the term *ḥazannu* is consistently applied to indigenous vassal rulers like Rib-Hadda himself, whether they are Rib-Hadda’s “friends” or “associates” (EA 113, 126), or rebellious vassals who threatened Rib-Hadda and, according to him, jeopardized Egypt’s control of Canaan itself (EA 129, 130). In general, it is clear in the Amarna letters that the *ḥazannu* of a city almost always came from a leading family of local origin, and was viewed as a “king” (*ṣarru*) from the native perspective (see Moran 1992:xxvii). In EA 365, to cite another example, Biridiya of Megiddo complains to the pharaoh that, although he had brought corvée workers to plow at Šunama, other *ḥazannus* (i.e., the kings of neighboring cities) had not done the same. Canaanite kings like Biridiya and Rib-Hadda who ruled their native cities on behalf of the pharaoh played exactly the same role as the humble village *ḥazannu* in the kingdom of Ugarit and elsewhere.

To be sure, the *ḥazannu*’s duty to collect taxes and to conscript corvée workers probably made him unpopular at times; but the obvious fact that the fiscal system of the kingdom of Ugarit functioned effectively for a long period without the need (as far as we know) for constant military intervention suggests that the *ḥazannu*’s actions as village leader were generally regarded as legitimate. There are indications that the secondary towns and villages of Ugarit were internally self-governing by means of a council of elders that was presumably headed by the *ḥazannu*, although there is no direct textual evidence for this (see Heltzer 1976a:75–79). In any case, the administration of the villages seems to have involved a minimum of direct supervision by the palace.

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°° On the texts from Ugarit mentioning the *ḥazannu* = Ug. *rb* (qrt), see Heltzer 1976a:80ff.

The other administrative title that occurs frequently in texts from Ugarit is sākinu (skn in alphabetic texts, written ṣā-ki-in-ni or ṣā-ki-ni in several syllabic texts; see Huehnergard 1987:157). In all likelihood, this Northwest Semitic term was semantically equivalent to the logogram MAKŠIM often used in Akkadian texts from Ras Shamra to denote a certain kind of royal official. The principal sākinu of Ugarit (the royal “vizier,” for whom, specifically, the Akkadian term šakin mātī was used) resided in the capital city and appears prominently in the diplomatic correspondence. But there were also lesser officials known as sākinu qarriti (skn qrt; also skn GN), “prefect of the town,” who were resident in certain major settlements outside the capital. This type of official is called the MAKŠIM (rābiṣu) of such-and-such a town in Akkadian texts (e.g., the MAKŠIM of Riqdu in RS 17.61; see Heltzer 1976:82f.; 1982: 150f.).

In contrast to the rabbu qarriti (Akk. ḫażanunu) or “chief of the settlement,” the sākinu seems to have been a higher-ranking royal representative, presumably appointed by the king’s immediate circle. He may have served on occasion as an inspector or commissioner dispatched from the capital city or from his regional headquarters (in the case of a sākinu qarriti) to oversee the king’s possessions and report back to him. This is indicated by the equivalence with the logogram MAKŠIM, which in the Amarna letters of the same period, in the context of Egyptian imperial administration in Canaan, denotes a roving inspector or messenger who was a native Egyptian (read as Akk. rābiṣu or Canaanite sākinu; see Redford 1992:198–203 and the discussion of New Kingdom imperial administration in chapter 12.9 below). Oppenheim (1968) pointed out that Akkadian rābiṣu denotes both a demon and a type of official, and he noted that the term refers to “a subaltern official acting mainly as the representative of authority” (ibid., p. 178). More specifically, he argued that the rābiṣu must have had (at least originally) “the hated function of spying on people in order to report on them” (ibid., p. 179), because of the term’s association with demons and its derivation as an active participle of the verb rabāṣu, which denotes the lying down or crouching of animals, including snakes (cf. Heb. rōḇēq in Genesis 4:7).

Regardless of the mobility or lack thereof of the sākinus of Ugarit, the point to note here is that the administrative responsibilities of this official, like those of the local town or village chief, were broad and ill defined. The authority they exercised stemmed from their role as representatives of their master, the king, rather than from their tenure in bureaucratic offices with clearly defined rights and duties. Furthermore, if my interpretation is correct, the contrast between the local magnate or chief (rabbu) and the royal commissioner (sākinu) is not a matter of their different administrative functions, but of their different proveniences. The textual evidence suggests that both were involved in economic, judicial, and police affairs; in other words, they did whatever had to be done to ensure that taxes were paid, troops were supplied, and the peace was kept. They differed, however, in their origins: the rabbu qarriti (Akk. ḫażanunu) was normally a local headman whose native authority was simply recognized by the king, whereas the sākinu (Akk. šakin mātī at the highest level, otherwise MAKŠIM/rābiṣu) was a trusted agent or inspector periodically sent from the palace or a secondary administrative center to act as the eyes and ears of his lord.

Moreover, these constituted the two main roles in what was a relatively simple administrative system. The other “administrative titles” used in texts from Ugarit either refer to occupational specialties (e.g., Ug. spr, Akk. ṣupšarru, “scribe”) that were necessary to the royal administration but did not constitute bureaucratic “offices,” or they denote the high status or personal royal favor enjoyed by the men who bore them (e.g., mūdā, a “courtier” or “familier” of the king or queen; see Heltzer 1982:141–67; Huehnergard 1987: 144f.). There is no evidence of a complex system of ranked bureaucratic offices. Even the military organization seems to have been relatively simple: we hear of “commanders(?)” (mru) and “chiefs of ten” (rb ʾšrt), and a few kinds of military specialty (e.g.,

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77 MAKŠIM is usually read as Akk. rābiṣu but perhaps should be read as NWS sākinu in the Ras Shamra texts (Buccellati 1963; cf. Rainey 1966). In any case, the semantic equivalence between sākinu and MAKŠIM/rābiṣu is clear. See CAD R, s.v. rābiṣu, which defines this term as “an official representative of and commissioned by a higher authority, attorney.”

78 The statement in CAD S, p. 76 that sākinu was written ugar-kin Kur-ti (or Kur GN) at Ugarit, in addition to the syllabic writing usā-ki-it-in-ni, is incorrect. The term ugar-kin Kur-ti should be read as Akk. šakin mātī, “governor of the land”; cf. ša-ki〈-in〉 Kur ṣū-루-ri-ti in the Akkadian letter from Ugarit found at Tel Aphek (Owen 1981). In RS 15.182:6 and 10 the terms ugar-kin Kur-ti and usā-ki-in-ni refer to the same person, but this can be explained by the semantic equivalence of Akk. šakin mātī and syllabically written Ug. sākinu when referring to the highest-ranking sākinu of the kingdom of Ugarit (cf. Heltzer 1982:141–49). A lower-ranking sākinu (skn qrt or skn GN) could not be called šakin mātī, and so was referred to as the MAKŠIM (rābiṣu) of the town/GN in Akkadian texts from Ugarit (the Akk. term sākinu is not used in this context).

79 See KTU 4.160:6, 9; 4.288:2–5; 4.609:10, 11. Note esp. KTU 4.288, in which a list of five unnamed officials consists of four “skn GN” entries followed by one entry of the form “rb GN.” Here is a clear indication of the distinction between the administrative titles sākinu and rabbu.
is true, for example, of the four lists containing names of patrons and the absence of dates, which makes it difficult to match references to the same person. A solution to this problem may lie in the fact that series of the same names are sometimes repeated in more than one text. This increases the probability of valid matches among the persons named in the same list. If the probability be deemed high enough, ancillary information about a person that is available in one text (e.g., his patronym, town, or profession) could then be applied to a person with the same name in another text with a similar structure.

Aside from problems with homonyms, matches among texts are hindered by the need to equate variant orthographic representations of the same name written in diverse alphabetic and syllabic (or logographic) forms. Correct equations between variant forms of the same name would require attention to the representation of Ugaritic consonants in syllabic texts, along with a consideration of the probable NWS and Hurrian (as opposed to Akkadian) values of certain logograms, especially those representing divine names. Uncertain textual readings also cause problems, especially because many of the Ugaritic administrative texts are so far published only in transliteration; but it will often be possible to guess the scribe’s intention and to assign a rough probability to the reading chosen. Uncertain readings can be used, therefore, provided that some estimate of the probability of a match between names is made explicitly, and the degree of uncertainty is taken into account in interpreting the results. On the positive side, the Ugaritic administrative texts have the advantage of being limited to the last few decades in the life of the city, which increases the probability of valid matches among the personal names found in them.
Chapter 12. Patrimonial Society in the Bronze Age Near East

MAX WEBER’S patrimonial household model (PHM) deserves greater attention in ancient Near Eastern studies than it has hitherto received. This is because it agrees with the very durable native terminology used for all manner of political and social relationships throughout the Near East in the pre-Hellenistic period. Household language—the use of terms such as “house,” “father,” “son,” “brother,” “master,” and “servant” in an extended political sense—carries more significance than is usually thought, for it reveals the self-understanding of the social order that was at work in these societies. These terms were used metaphorically, to be sure, but this does not mean that they were merely casual figures of speech or euphemisms for “real” economic and political relationships. They were widely used because alternative conceptions of social hierarchy were not readily available. In the absence of the rather abstract idea that an impersonal political constitution or universal egalitarian social contract might underpin the social order, personal relationships patterned on the household model served to integrate society and to legitimate the exercise of power.

As I have argued at length in Part One of this book, the native understanding of society is of prime importance in sociohistorical reconstruction because the symbolically mediated interpretations attached to social relationships by those who take part in them invariably affect social behavior, with the result that such behavior cannot be adequately explained without taking these interpretations into account. Indeed, such interpretations are an integral part of the social reality being studied, as hermeneutically oriented theorists have long argued. This is not to deny the influence of economic or material factors that may be hidden from social actors, but I agree with those who argue that sound methodology requires equal if not greater attention to linguistically mediated symbols and their dialectical interaction with the factual conditions of social life. Furthermore, the PHM, because it conforms to the native symbolization of society in Ugarit and elsewhere in the ancient Near East, sheds light not just on political attitudes and behavior but also on the economic and religious aspects of social life. In other words, familiar household relationships provided the pattern not only for governmental authority and obedience but also for the organization of production and consumption and for the integration of the gods with human society. Although our sources are inadequate to show in detail how the household concept expressed itself in each of these areas, in what follows I will sketch the ways in which I think that it conditioned political, economic, and religious behavior in various Bronze Age patrimonial polities that were close to Ugarit in time and space.

This is not to deny the existence of regional and temporal variations in social organization in the ancient Near East, which served to distinguish Egypt, for example, from the Asiatic Near East, and perhaps served to distinguish southern Mesopotamia from its northern and western neighbors within the Semitic-speaking area (see Steinkeller 1993 and 1999 for a defense of the hypothesis that a long-lasting regional difference existed between a “southern” or Sumerian and a “northern” or Semitic socioeconomic system in Syro-Mesopotamia). But, as I have suggested in chapter 3, these variations can be viewed as quantitative rather than qualitative differences, because they have to do with the effective degree of control exerted by the apical ruler rather than with fundamentally different conceptions of rulership. External variations in the forms of ancient Near Eastern society therefore need not imply radically different cultural assumptions but may simply reflect more or less centralized modes of operation within the same basic framework. Patrimonialism can appear in many guises, but historical and ethnographic sources reveal a striking uniformity in the way in which authority is understood and expressed in diverse pre-Axial societies. The evidence for a common patrimonial conception of the social order throughout the Bronze Age Near East accordingly buttresses the claim that the kingdom of Ugarit, in particular, is best understood as a patrimonial regime.

By using the term “pre-Axial” I am following Eisenstadt’s (1986) diachronic model of Near Eastern sociohistorical development, which is discussed in chapters 3.1 and 4.2 above. Eisenstadt highlights the historical development from patrimonial to more bureaucratic regimes that is implicit in Weber’s typology of legitimate domination, which distinguishes “traditional” and “legal-rational” types of domination. He presents us with the hypothesis that before the mid-first millennium B.C., the polities of the Near East, including Late Bronze Age Ugarit, were based on traditional rather than legal-rational domination and were therefore fundamentally different from later kingdoms and empires in the region, both structurally
and ideologically (although traditional elements of course persisted for a long time). I believe that this hypothesis is correct, and I endeavor to show that the data from Ugarit are best understood according to the PHM.

It is beyond the scope of the present work to demonstrate in a detailed way the validity of the PHM for the Bronze Age Near East as a whole. Still, no study of the kingdom of Ugarit would be complete without an attempt to place Ugarit in the broader context of the common cultural assumptions and social behaviors that were found throughout the Near East during the Bronze Age and into the Iron Age. For that reason, I provide here a selective review of the evidence and scholarly interpretations concerning social organization for a number of different third- and second-millennium Near Eastern regimes. Much more could be said about each of the examples given below, and in presenting such a broadly based survey I am well aware that no one author can hope to be cognizant of all the relevant data. But my purpose is simply to defend the proposition that traditional legitimization, and hence a relatively pure patrimonialism, characterized all known societies of the Near East before the first millennium B.C.

1. The Household Basis of Political Terminology

Following Max Weber, I have suggested that the patrimonial household model is a suitable description of ancient Near Eastern society because it conforms to the native understanding of the social order. This native model is revealed by the frequent use of household-derived terms such as “father,” “son,” “brother,” “master,” and “servant” to describe political relationships. The use of the terms “father” and “son,” in particular, is illustrated in the royal correspondence found at Mari, an important city which controlled the middle Euphrates River. The Mari letters (written in Old Babylonian Akkadian) show very clearly that in Mesopotamia and Syria in the eighteenth century B.C. a subordinate ruler called his overlord “father” (abum) as well as “master” (belum) and referred to himself both as “son” (marum) and as “servant” (wardum) of his political superior.1 Political equals who were more or less independent of one another addressed each other as “brother” (ahuum), especially if they were vassals of the same overlord (i.e., “sons” of the same “father”), or if a treaty relationship had been established between them.2

One text from Mari that illustrates especially well the basic conception of a hierarchy of households which underlay the political order is ARM 1.2 (Dossin 1950:22ff.), in which the vassal ruler Abi-Samar calls Yahdun-Lim of Mari his “father” (l. 6) and stresses his allegiance to him by affirming that “(my) house is your house and Abi-Samar is your son” (l. 13). Alliances between rulers are expressed in similar terms. In text A.4350 we read that “the house of Nihriya and the house of Mari have always been one house” (cited in Durand 1992:116 n.152). Likewise, in ARM 26/2.449:14f., Hammurapi of Babylon stresses the good relations between Mari and Babylon, stating that the two cities “have always been one house” (bitum išṭen).

Occasionally, an inferior might presume to call his political superior “brother” (or, more humbly, “elder brother”),3 but to deviate in this way from the norm was itself to make a political statement. Bertrand Lafont (1994) discusses a letter sent to Zimri-Lim of Mari by a political advisor which deals with the delicate diplomatic situation created when one of Zimri-Lim’s vassals, Sima-ile of Kurda, presumptuously addressed him as “brother” rather than as “father” (on this text see also Sasson 1998:462ff.). The advisor blamed this faux pas on the elders of Kurda, who had pressed their new king to assert the autonomy of their

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1 Note that Akk. belu(m) and its NWS cognates (e.g., Ug. bi-r‘lu) are first and foremost household terms, denoting the male owner or master of a patriarchal household and of the property and personnel (incl. wives, children, and servants) that make it up. Similarly, Akk. (wa)ruddum and NWS šabdum, “slave, servant,” pertain first of all to the household setting, and are then extended metaphorically to encompass other social and political relationships.

2 This familial terminology is discussed, with numerous examples, in Dossin 1938; Munn-Rankin 1956; Limet 1985; Kupper 1990; 1991; Lafont 1994. The same political terminology was used in Syria already several centuries earlier, as shown by an Ebla text of the 24th century B.C. in which the king of Ebla is declared to be the “brother” of the king of Hamazi (TM 75.G.2342 = MEE 1.1781; see Pettinato 1991:240ff.). Third-millennium (Early Bronze Age) evidence is much scantier than second-millennium (Middle and Late Bronze Age) documentation, but this text demonstrates the longevity of a political symbolism that was operative throughout the entire Bronze Age, especially among the Semitic-speaking peoples of Syria and northern Mesopotamia.

3 The term “elder brother” is used, for example, in ARM 26/2.404:17, where Atamrum of Andarig is reported as having sworn allegiance to Zimri-Lim of Mari and is quoted as saying to the assembled company, which included seven kings who were in his service (i.e., subvassals): “There is no other king than Zimri-Lim, our father, our elder brother (a-hi-ni gal)” (Joannès 1988:259). As Kupper (1991:181) has noted, Atamrum variously calls Zimri-Lim his father, his brother, and his elder brother, indicating that although he recognized Zimri-Lim’s authority, he wished to claim a slightly higher status than that of an ordinary vassal “son” or “servant.”
city (which had previously been independent of Mari during the reign of Yahdun-Lim) in his correspondence with Zimri-Lim. This breach of protocol was all the more galling because, as the writer of the letter reminds him, Zimri-Lim had just succeeded in persuading Hammurapi of Babylon to allow Simah-ilane to leave Babylon, where he had been living in exile, so that he could be “restored” to the throne of Kurda under Zimri-Lim’s control. And to accomplish this, Zimri-Lim had abased himself by addressing himself to Hammurapi as his “son” in order to win his assistance.

What is striking is that this one letter illustrates not only the political ploy in which an inferior “son” (Simah-ilane) claims the status of a “brother,” but also illustrates the calculated political flattery in which a “brother” (Zimri-Lim) refers to himself as the “son” of his peer in order to gain a favor. It seems that the writer of the letter reminded Zimri-Lim of his own manipulation of these political terms in an attempt to assuage his anger against Simah-ilane, suggesting that Simah-ilane was not really rebelling against Zimri-Lim’s authority by addressing him as “brother” but was simply trying to appease his domestic political audience in Kurda. It should be noted that this sort of manipulation of political terminology, within a basic framework in which father-son-brother signified superior-inferior-equal, does not indicate that the familial terminology failed to match political reality and so was merely euphemistic, for the political significance of such deviations from the norm itself depended precisely on the acceptance of the standard meanings, which were operative in most cases.

There is evidence that the same political terminology was employed in Canaan, to the south and west of Mari, during the Late Bronze Age (ca. 1500–1200 B.C.). Indeed, it is likely that this household-based terminology was used throughout Syria and Palestine in the second millennium B.C., even though the available textual documentation is sparse. In the Akkadian diplomatic correspondence found at el-Amarna in Egypt there are instances in which the pharaoh’s vassals in Canaan addressed a political superior as “father” (abu) and referred to themselves as his “son” (māru). An obsequious letter from Aziru of Amurrum to Tutu, an Egyptian official, is especially noteworthy in this regard, because of Aziru’s elaborate protestation of fidelity to the pharaoh and his representative:

To Tutu, my master, my father: Message of Aziru, your son, your servant. . . . Moreover, as you in that place are my father, whatever may be the request of Tutu, my father, just write and I will grant it. As you are my father and my master, and I am your son, the land of Amurrum is your land, and my house is your house. [EA 158:10ff., as translated by Moran 1992:244, but slightly altered; see also EA 164]

It might be argued that Amurrum was a special kind of “tribal” state, populated by outlaw <apiru>-people, so that its rulers had a distinctive kin-based conception of political relationships. But the terms “father” and “son” were also used by Rib-Hadda of Byblos in exactly the same way (see EA 73 and 82), in parallel to the more common pair of terms “master” (belu) and “servant” (ardu), which Rib-Hadda used in most of his letters to Egypt. And note that the same terminology is used on the Egyptian side, because Rib-Hadda is addressed in EA 96 as “my son” by the general, your father” (Moran 1992:170).

Thus it was not just the pharaoh but his representative who assumed the role of “father” in dealing with Egyptian vassals. In a letter from Ugarit found at Aphek in coastal Palestine, an Egyptian official named Haya is addressed as “my father, my master” by a Ugaritian official who refers to himself as “your son, your servant” (Owen 1981:7ff.). Note also the unpronounced Amarna-type letter recently published by Huhnergard (1996), which was probably sent by the successor of Rib-Hadda of Byblos, Il-rapi, to the city of Kömidu (written ku-mi-di in Amarna Akkadian) in the Biqa valley of Lebanon. This letter is addressed to the “great man (LÜ.GAL), my father” from “Il-rapi <your> son.” The “great man” in question was no doubt the local Egyptian governing official, as Huhnergard suggests (p. 99). In exactly the same fashion, a Hittite viceroy in Carchemish named Pišawalwi, a “son of the king” (i.e., the Hittite king), addressed Ibiranu, king of Ugarit, as “my son” (RS 17.247:3 = PRU 4, p. 191; for a recent translation, see Beckman 1999:127).

As in the Mari letters cited above, the native conception of political structure that is in evidence here entailed a hierarchy of households, in which the “son” governed a dependent household that ultimately belonged to the “father” (on this point see, most recently, Westbrook 2000). There is a hint of the same attitude from the Egyptian side during the Late Bronze Age in the use of the word ity as a standard term for the pharaoh in his capacity as the ruler of foreign lands. This word is usually translated “sovereign,” but it is obviously related to the word it.  

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4 See also the treaty ratifying an alliance between Zimri-Lim and Ibal-pi-El II of Ešnunna (Charpin 1991), in which the king of Ešnunna is consistently referred to as the “father” of Zimri-Lim, who was probably not, strictly speaking, Ibal-pi-El’s vassal, but who was nonetheless willing to acknowledge the greater power of his ally.
father,” as a number of scholars have pointed out; thus it might better be translated “patrarch” or the like (see Lorton 1974:7f. for text citations and references to the scholarly literature).

It is true that the terms “master” and “servant” are much more common than “father” and “son” in both the Mari letters and the Amarna letters, but the cases in which “father” and “son” are used show that master–servant and father–son forms of address were politically equivalent, both of them reflecting the household relationship that constituted the political link between a ruler and his subordinates. It is important to remember that the terms “master” and “servant,” no less than “father” and “son,” originated in ordinary household settings, even though they were also applied to broader political relationships. Moreover, it is clear from both the Mari letters and the Amarna letters that the pair “son” and “servant,” like “father” and “master,” could be used interchangeably to address the same person.

It should also be noted that the household model of political relationships was applied to sprawling empires as well as to the smaller city-states and kingdoms of the Bronze Age. Our sources suggest that a conqueror’s rule over subjugated lands was understood to be structurally similar to his domestic political authority within his own kingdom. In both cases, power was legitimized in household terms because conquered kings became part of the conqueror’s household. Likewise, political alliances spoke of one another as brothers whose houses were united within one overarching household.

William Moran (1992:xxiv n.61) cites various Late Bronze Age examples of this, including the treaty between the Hittite king Muwattalli II and Talmi-Sarrumma of Aleppo, in which Muwattalli II says: “We are all sons of Šuppiluliuma the great king and our house is one” (Weidner 1923:86, lines 8f.); or, following Gary Beckman’s (1999:95) translation, “For we are all sons of Šuppiluliuma, the great king, so let our house be one.”

At all levels in the political hierarchy, from the smallest city to the largest empire, political superiors were the “masters” and “fathers” of their subordinates, who were their “servants” and “sons”; similarly, political equals were “brothers.” Far from being merely banal or euphemistic, the use of such terms expressed a basic understanding of political and social relations that was derived from familiar household relationships. This model of society was quite simple, to be sure, but it was also sufficiently flexible and extensible to encompass personal relationships at many different levels, from the affairs of the humblest family to the dealings between kings.

Some scholars, however, have questioned the political significance of father-son terminology in the ancient Near East. In their detailed studies of Late Bronze Age international relations, both Guy Kestemont and Mario Liverani have argued that “father” and “son” in international correspondence were simply polite forms of address without political significance. Kestemont (1974a: 54f.), in particular, reduces not just father-son (aba-šarru) but also master-servant (bēlu-ardu) terminology to the status of “une formule politesse,” like modern French monseigneur or monsieur, especially in the salutations of letters.

But this interpretation is unlikely, for in most of the royal letters we possess, the terms “master” and “servant,” as well as “father” and “son,” clearly correspond to—indeed, they are expressive of—a relationship of political inequality, as we have seen. Kestemont does not cite any texts to substantiate his contrary opinion; moreover, it appears that he is led to it largely because of his concern to reject feudal terminology and, in particular, the notion that the terms bēlu and ardu denoted specific legal and political statuses and so correspond to the medieval feudal terms “seigneur” and “vassal” (ibid., pp. 43–55). According to Kestemont, a fundamental characteristic of feudalism was the personal link between the lord and the vassal, which he does not find in Late Bronze Age international relations. He argues that, despite the impression given by the letters and treaties that political alliances had a personal character, in actual fact political relationships were not formed between individual persons (i.e., kings) in feudal fashion but between “states” or “national communities.” As he puts it: “en matière juridique publique, le chef d’État et la communauté nationale constituent deux pôles d’une seule et même personnalité morale: la communauté nationale est le siège réel des droits et obligations et le chef d’État en est le représentant” (Kestemont 1974a:48).

In defense of this assertion, Kestemont points to the fact that treaties were not annulled at the death of the king who made them. For him this means that despite the verbal formulation of the treaty as an agreement between individual persons, the true participant in any treaty was the “state,” not the person of the king, because only the state was permanent. But Kestemont forgets that in the ancient Near East, as in many premodern societies, it was the ruling dynasty that was believed (or hoped) to be permanent, and the dynastic heir was responsible for maintaining agreements entered into by his forebears. There is no evidence for an abstract conception of the impersonal state as a political agent during the Late Bronze Age; this is an entirely anachronistic notion.
Kestemont also argues that the inhabitants of a country formed a national community or (in Akkadian) a mātu, a term which, in his opinion, denotes not just a territory but a political community. The national community was the true subject of international relations, according to Kestemont, because treaty texts sometimes assign sanctions or benefits collectively to this or that mātu. But in all of the texts he cites, pride of place is given to a delineation of the rights and responsibilities of the particular kings (and their dynastic successors) who are making the treaty, even if a king’s mātu is also mentioned. Late Bronze Age international treaties clearly were agreements between individual rulers, not between “peoples” or “states,” and the personal character of these agreements is plain, although each king’s subjects will obviously have been affected by their lord’s actions in making treaties and breaking them.

This is not to say that Kestemont is wrong to question the uncritical use of feudal terminology to describe Late Bronze Age regimes (see chapter 9.1 above). But the personal nature of politics and the personal link between political masters and servants is, in my opinion, the one feature of European feudalism that does correspond to what we find in the ancient Near East; it is hardly the basic point of difference, as he maintains. Kestemont also observes quite correctly that the terms bēlu and ardu, as well as abu and māru, had a wide variety of uses outside of international politics. But it does not follow that these terms had no political meaning in treaty texts and in international royal correspondence. Indeed, if political relationships at all levels were understood according to the household model, as I have suggested, the use of these household terms in political contexts was not only appropriate but quite necessary. No other terms could express so well the structure of political authority, mirroring as it did the universally familiar phenomenon of patriarchal authority within the household. Thus what appears to us as a rather confusing variety of uses of a few household-derived terms in diverse social and political contexts, large and small, can be explained by positing a native conception of the entire social order as a large collection of hierarchically nested households linked together solely by dyadic personal relationships between “masters” and “servants,” “fathers” and “sons,” and “brothers.”

Liverani, unlike Kestemont, acknowledges the political significance of the terms master and servant. He emphasizes, furthermore, the personal character of international political relations in the Late Bronze Age and the concomitant use of the familial metaphor of brotherhood in the correspondence between great kings—a metaphor, he argues, that served to emphasize the personal and voluntary character of political relations between peers (see Liverani 1979c:1323–33; 1990:187–202). In keeping with the ideal of brotherhood, kings speak of tābūtu, “good relations,” and rāʾimātu, “friendship” or “love.” Parallels to this treaty language can be found in the Hebrew Bible, as several studies have shown (e.g., Moran 1963a, 1963b; Hillers 1964b; Huffmon 1966). As Liverani (1990:199) points out, the idea of brotherhood “is perfectly fitting to the political needs in the atmosphere of the time: on one side the theoretical and quite idyllic model of mutual love, on the other side the practice of endless quarrels and contrasts.”

But Liverani, like Kestemont, denies the political significance of the terms “father” and “son,” which are also found in the correspondence between kings. In his opinion, the notion of brotherhood was the only really significant form of the familial metaphor; father-son language was simply a variant of brotherhood terminology and appears mainly in correspondence between political equals where a difference in age made the brotherhood metaphor inappropriate. Liverani asserts, accordingly, that the term “father” was used “with no technical implications at the political level, just as a sign of personal respect” (ibid., p. 198). It did not express political subordination, in his opinion; instead, “suzerainty is expressed by the lord/servant terminology” (p. 199).
Here Liverani refers to the work of Carlo Zaccagnini (1973:157–60), who enumerates several texts in which father-son language is found, in an attempt to show that these terms did not have political significance (although he acknowledges that they do correspond to a difference in rank in a few cases). Most of these examples, however, can be readily interpreted in political terms. Liverani singles out two letters found at Ras Shamra (RS 19.70 = PRU 4, p. 294 and RS 17.247 = PRU 4, p. 191), in one of which the king and queen of Ugarit address a Hittite official as “father,” while in the other a Hittite prince addresses the king of Ugarit as “my son.” Liverani rejects the possibility that “father” and “son” could have signified political subordination in these cases, apparently because he assumes that a Hittite official could not have been the political superior of the king of Ugarit, who was a direct vassal of the Hittite king. But the correspondents in these cases were not political equals any more than Aziru of Amurru was the political equal of the Egyptian official Tutu, whom he addresses as both “father” and “master” (see EA 158, cited above). Simply because a vassal ruler is dealing here with a lower-ranking official of the imperial court and is not corresponding directly with his overlord does not mean that the father-son terminology has no political significance, as EA 158 shows quite clearly. Indeed, it seems that the role of father and master was automatically transferred from the great king (Egyptian or Hittite) to his representative whenever the latter had dealings with political subordinates.

The other main counterexamples, according to Zaccagnini, involve correspondence between kings who were equal in rank; for example, between an unnamed king of Amurru and the king of Ugarit, his “son” (RS 17.152 = PRU 4, p. 214); between the king of Ušnātu and the king of Ugarit, his “father” (RS 17.83 = PRU 4, p. 216 and RS 17.143 = PRU 4, p. 217f.); and between the king of Ugarit and the king of Alašiya, his “father” (RS 20.168 = Ug 5.21 and RS 20.238 = Ug 5.24). The kings of Amurru and Ugarit were political equals, both being vassals of the Hittite king, and elsewhere a king of Amurru addresses a king of Ugarit as “brother,” as Zaccagnini points out (RS 17.286 = PRU 4, p. 180). But in the first example, it is possible that the king of Amurru in question was the grandfather of the king of Ugarit, giving him warrant to address him as “son,” as Nougayrol suggests (PRU 4, p. 214 n.1; e.g., Pendēšina of Amurru was the maternal grandfather of ḪAmmiṯtamurri II). As for the deference shown by the king of Ušnātu, it must be remembered that Ugarit’s southern neighbor Siyannu-Ušnātu was originally its subvassal under Hittite rule, and there is evidence that it retained an inferior status even after its secession from the kingdom of Ugarit.\footnote{See Nougayrol’s comments in PRU 4, pp. 15ff. and Ug 5, p. 123. Astour (1979:14) notes that Siyannu-Ušnātu remained “a vassal of second rank.”}

It is possible, of course, that the terms “father” and “son” could be employed simply out of respect for a great difference in age rather than always expressing political subordination. This may account for the fact that the king of Ugarit addresses the king of Alašiya as “father”—although actual kinship resulting from dynastic intermarriage cannot be ruled out in this case, as well. But it is more likely that the extreme deference of the letters to Alašiya, in which the king of Ugarit says “I fall at the feet of my father” (a formula quite rare in letters from Ugarit) and appeals for aid and advice, is related to the desperate military situation during the last days of the kingdom. One of these letters, in particular, appears to be part of an exchange of correspondence in which ḪAmmurapi,\footnote{Cf. the use of the term “father” when appealing for help to a political equal in the Mari texts, discussed above.} the last king of Ugarit, reports the sighting of enemy ships and the burning of his villages, complaining that his own troops and ships are engaged elsewhere defending the Hittites, while the king of Alašiya instructs him to stand firm (see RS L1 = Ug 5.23 and RS 20.238 = Ug 5.24). The latter apparently had some sort of authority or superiority (perhaps only informal) in the face of this emergency; thus the use here of the term “father” was not simply honorific.\footnote{In general, when the historical context and the particular relationships between the correspondents are taken into account, it is clear that most (if not all) of Zaccagnini’s examples can be interpreted in terms of social or political subordination of one kind or another.}

Furthermore, Liverani’s attempt to separate master-servant terminology from father-son terminology is contradicted by the more-or-less synonymous usage of both forms of address in the Mari and Amarna letters cited above. Father-son terminology should not, then, be associated only with relationships between equals and consequently be subsumed under the metaphor of brotherhood. On the contrary, the language of brotherhood itself can be seen as merely one expression, pertaining to the special case of relations between political equals, of a more general familial or household model of politics that is also expressed by father-son and master-servant forms of address. Moreover, without denying the importance of the idea of brotherhood as the means of expressing good relations between political equals, it must be said that master-servant and father-son terminology...
occurs much more frequently than brotherhood terminology in our sources. This is not surprising, of course, given the predominance in the archives we possess of communications between political superiors and their subordinates, as opposed to correspondence between equals.

Liverani not only fails to appreciate the political significance of familial terms other than “brother,” as we have seen; he also fails to include within the familial model (or, better, the “household” model, which includes the master-servant relationship) another set of terms belonging to what he calls the “ideology of protection” (Liverani 1990:187–96). Unlike the ideology of brotherhood, with its stress on equality, the ideology of protection or mutual aid, as he describes it, was characteristic of the unequal relationships between political superiors and their subordinates. The imperial overlord and each king who was subject to him had a reciprocal obligation to “protect” (naṣāru) one another and to give “aid” (rēṣātu), which often involved military assistance but encompassed any necessary act in support of a ruler and his dynasty. Great stress was placed on the element of personal fidelity (kittu) that underlay this reciprocity.

In his discussion of the ideology of protection, Liverani quite rightly draws attention to the fact that the entire political system of the Late Bronze Age consisted of numerous dyadic, highly personal relationships between great kings and lesser kings. As he puts it: “The king is the center of the system—therefore the essential feature for its functioning—since all the two-sided relations of protection spring from and converge toward him” (ibid., p. 187). Earlier in the same paragraph he goes so far as to say that “political relations are an enlargement of the mechanism of mutual protection and support typical of the small groups (family, local community).” As he says elsewhere, this requires a “primitivist” as opposed to a “modernist” approach in studying political and economic phenomena of the period (Liverani 2000). But this approach to Late Bronze Age society calls for the inclusion of Liverani’s “ideology of protection,” no less than the “ideology of brotherhood,” within a broader “household ideology.” It is worth repeating that the notion of brotherhood is only one aspect of the pervasive household model that underlay all political relationships, for brotherhood pertains only to relationships between political equals. Yet by restricting the political significance of the familial metaphor to brotherhood between equals, Liverani neglects the political significance of other forms of highly personal household terminology (i.e., “master,” “servant,” “father,” “son”) that were applied to unequal relationships, and he consequently fails to connect these terms with what he discusses separately as the language of “protection” used between political superiors and their subordinates.

In his discussions of Late Bronze Age political relations, Liverani also emphasizes the difference, as he sees it, between political thinking in Egypt and in Asia (including the Hittite empire). In his opinion, Egyptian administration was much more bureaucratic than that of Syria-Palestine, for example, where highly personal relationships of dependence and fidelity were the norm (see Liverani 1979c:1323–27 and 1990:194–96 for a summary of his views on this matter), and this difference led to a misunderstanding between the pharaoh and his Asiatic vassals that can be detected in the Amarna letters (Liverani 1983b). Liverani argues that the standard Egyptian exhortations to a subordinate official to pay attention to what he is doing and to care for the post entrusted to him were interpreted by the pharaoh’s Levantine vassals (having been clumsily translated from Egyptian into Akkadian and then from Akkadian into Canaanite) as instructions to protect themselves and their cities. But to their “Asiatic” way of thinking, protection was something that a political master should provide for his faithful servant, who ought not to be left to fend for himself. Liverani asserts that at the heart of this misunderstanding, and of the repeated complaints and pleas for help found in letters to the pharaoh, were two conflicting conceptions of what political subordination involved: one bureaucratic and impersonal and the other based on notions of personal allegiance and dependency.

This is an interesting hypothesis; but as I argue below, there is no evidence that Egypt itself was governed by an impersonal bureaucracy whose conceptual basis was qualitatively different from modes of royal administration found elsewhere in the Near East (see chapter 12.9). Pharaoh’s semidivine status was apparently more exalted and his domination of political subordinates more one-sided than that of Hittite and Canaanite rulers, but Liverani exaggerates the difference between the Egyptian and Asiatic mentality. After a careful re-reading of the Amarna letters with this issue in mind, both Moran (1995) and Na’aman (2000) have rejected Liverani’s hypothesis of a culturally based misunderstanding (see also Moran 1985 on the letters of Rib-Hadda of Byblos). In addition, Westbrook (2000:40f.) disputes the idea that “the Hittite and Egyptian conceptions of international treaties during the Amarna period were different in kind,” noting evidence that pharaohs entered into treaties and took oaths.

Nonetheless, Liverani’s emphasis on the personal character of international political relationships in
Late Bronze Age Syria and Palestine is undoubtedly correct. Moreover, it can be argued that it is fundamental also for the analysis of the inner workings of Levantine polities like the kingdom of Ugarit, because it seems unlikely that the familial metaphor and the personal ties between rulers and subordinates were confined to the highest political level. Indeed, I shall attempt to show in what follows that the patrimonial nature of politics during the Late Bronze Age was not confined to “international relations,” but reflected the internal structure of the smaller kingdoms that were incorporated within the great empires. Ugarit, of course, is the best documented of these, and accordingly constitutes the focus of this study. But it is possible to argue that the situation there can be generalized, in broad terms, to the rest of the Levant.

The overtly familial model of political relations in terms of “fathers,” “sons,” and “brothers” seems to have gone out of favor in the Iron Age. Echoes of this political terminology are preserved in the Hebrew Bible (cf. Fensham 1971), but these terms, so common in the Middle and Late Bronze Ages, are rare in extant Iron Age inscriptions and treaties from the Levant. Instead, the master-servant terminology is used. That is still household-based terminology, as we have seen, but the language of political subordination and alliance nonetheless appears to have become less vividly personal. On the other hand, what might be called a “genealogical” model of international relations is strongly evident among the biblical writers, who interpreted national similarities and political ties in terms of descent from a common ancestor. The latter phenomenon is also well attested in classical Greek sources (see C. Jones 1999 on “kinship diplomacy” in the Graeco-Roman world). In a future volume I will discuss the changes in political terminology that took place in the course of the first millennium B.C., in relation to the general theme of “Tradition and Rationalization in the Axial Age.”

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9 Note that King Hiram of Tyre addresses King Solomon as “my brother” in 1 Kings 9:13, indicating that this familial form of address to a political equal was known to the biblical writer.

10 A possible exception is the eighth-century Phoenician inscription found at Karatepe in Cilicia, in which a man named Azatiwada boasts that “every king considered me his father because of my righteousness and my wisdom and the kindness of my heart” (KAI no. 26; ANET, p. 653f.). But, contrary to earlier interpretations of this text, it is likely that Azatiwada was not a king but rather a local governor or regent in the service of Awariku, king of the Danunians, and of his (underage?) heir. In that case, his role as “father” was in the nature of adviser and caretaker rather than ruler (see Greenstein 1995 and the bibliographic references there).

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2. A Hierarchy of Households in Third-Millennium Mesopotamia

It is true that historians of the ancient Near East have rarely made explicit reference to Weber’s typology of domination or to patrimonialism in particular, but a number of scholars have recognized the importance of large households in the social and economic organization of ancient Near Eastern societies. I. J. Gelb, for example, collected a great deal of evidence to support his contention that the household—Sumerian é, Akkadian bitu(m)—whether “public” (belonging to the crown, a temple, or an official) or “private” (belonging to a kinship group or an individual) was the basic socioeconomic unit of third-millennium Mesopotamian society (see Gelb 1969; 1979b). Such households consisted of owners, managers, dependent workers, animals, buildings, fields, orchards, and other property; and they could be quite large. Almost all productive activity took place within these households, which coordinated and managed the entire labor force of the state. According to Gelb (1972; 1979a), most of the dependent workers belonging to such households were not slaves but “semi-free serfs” who had families and land allotments (“prebends”) of their own and usually worked only part-time for the master household to which they belonged (on part-time versus full-time service, see Gelb 1979b:23f., where he suggests that only supervisors and single women and orphans worked full-time). Against Diakonoff, Gelb argued that free peasants were relatively scarce, as were chattel slaves, who tended to be foreign prisoners of war. He also noted many cases of households within households, sometimes three levels deep; that is, the household of a lower-ranking person formed part of the household of a superior, which in turn was part of another household (Gelb 1979b:7, 39–45, and passim).

The Problem of “Public” versus “Private”

Gelb’s distinction between “public” and “private” households is somewhat confusing, however. At first glance it appears that he accepted Diakonoff’s two-sector model, but his oral remarks on a paper read by Diakonoff show that his own view was quite different. As he put it: “Private to me in this case means also privately public because it includes the large landowners who at the same time are officials of the state. They own land, and in that sense you call it private, but they are public in the sense that they are
at the same time officials of the state, participating in the activities of the state” (Gelb, in Diakonoff 1972:50). The reason for Gelb’s use of the term “private” was his belief, in agreement with Diakonoff, that third-millennium texts recording sales of land, especially the “ancient” kudurrus, attest to ownership of landed property that was completely separate from state or temple property (see Gelb 1969). But as I have argued above, at the beginning of chapter 10.3, “ownership” of land in ancient Near Eastern society was probably not exclusive, because various persons might have different traditional rights over the same property. Evidence for “private” sales of land therefore does not prove that the ruler had no control over the disposition of the land, and it does not justify the hypothesis of a “free” sector composed of persons with a separate legal status from that of the members of the great “public” households. In light of the evidence that Gelb presents for hierarchies of households, and in light of his observation that “private” households often belonged to state officials, his public-versus-private distinction may be less useful than the emphasis he placed on the existence of household-type organization at all levels in early Mesopotamian society (see also van Driel 1994 on the difficulty of distinguishing between “private” and “not-so-private” transactions in Ur III Nippur).

Eliminating the public-private distinction also shifts attention to Gelb’s argument that most of the workforce consisted of “serfs” who possessed the means of production (a category that included supervisors and other officials) rather than slave-like dependents. Even though they and their families belonged to larger households, for which they worked during part of the year, these serfs had land and households of their own, and even subserfs in some cases; thus they had the time and means to engage in private economic activity on their own account. In light of Gelb’s overall reconstruction, therefore, the terms “public” and “private,” if they are used at all, should be applied not to separate groups of households and personnel, as in Diakonoff’s two-sector model, but to the alternation of activity within each dependent household between work done on behalf of the master household (ultimately a royal or temple household) and work done for the direct benefit of the dependent household itself.

In Gelb’s reconstruction, the labor force in third-millennium Mesopotamia consisted largely of native-born serfs possessing families of their own and the means of production, to whom were added a much smaller number of foreign-born “chattel slaves” without families or means of production. Diakonoff (1972:52; 1976) disputes this, preferring to call these two categories “helots” (i.e., those with land allotments) and “patriarchal slaves” (i.e., the more directly dependent members of a household). These were two groups within the same socioeconomic class of nonfree laborers, according to Diakonoff, because the helots (Gelb’s serfs) did not “own” the means of production but merely received the use of land as an equivalent to rations. On the topic of rations, Diakonoff (1972:52) also questioned Gelb’s statement that the textual evidence shows that most serfs worked only part-time and that rations were distributed to them during only part of the year, arguing instead that all such workers were nonfree laborers who worked full-time for the master household and received rations year-round.

This issue was put to the test by Gelb’s student Piotr Steinkeller (1987b), who undertook a prosopographical study of 75 Sumerian texts dating to the Third Dynasty of Ur (the “Ur III” period) that mention workers who harvested trees and wild plants in the province of Umma. He found that the “foresters of Umma” worked for the state only during the second half of the year, in the period between sowing and harvesting when there was a lull in their private agricultural activities. Furthermore, the workforce was organized along family lines (in actual fact, not just metaphorically); that is, the head worker in each forest was assisted by his junior kinsmen. Both the specialized occupation of each worker and the land allotment that went with it were hereditary (which was also the case in Ugarit, as I have argued above).11 Contrary to Diakonoff’s reconstruction, there was apparently no social distinction between managers and the laborers working under them, who were frequently members of the same family. In one case, a man began as a manual worker and later became a supervisor. Managers (i.e., “various types of administrators, military officers, priests, etc.”) and laborers alike were state dependents who received land allotments in return for service (see Steinkeller 1987b:100f.).

More recently, Steinkeller (1996) has also studied the organization of craft specialists, in particular, potters, in Ur III Babylonia. After a close study of a variety of texts, he rejects the widespread notion that southern Mesopotamian craft specialists such as potters were organized in large, state-run workshops or factories managed by bureaucratic supervisors who

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11 Maekawa (1976) presents similar findings with respect to part-time service in Ur III Lagaš. Other data from the Ur III period suggest that administrative positions and occupational specializations were heritable, even during this highly centralized era in Mesopotamian history; see, e.g., Hallo 1972 and Zettler 1984 on the powerful “house of Ur-Me-me” in Nippur.
supplied pot-making facilities and central storage space. Instead, he concludes that “the Ur III potters worked in their own, home-based workshops, and . . . the state exercised but an indirect oversight over their production” (p. 251). Futhermore, pottery manufacture was a hereditary profession: “There are numerous documented instances of both a father and his sons having been potters, and . . . potters’ sons were designated as ‘potters’ already at birth” (p. 249). Steinkeller details the relationship between potters and “state” (palace and temple) organizations as follows:

The Ur III potter owed yearly a specific number of workdays to the state. In return, the state supplied him with a land allotment and, for the duration of his service, with rations of barley, wool, and fat. In this arrangement, each potter was affiliated with a specific institution, to which he rendered services year after year. For the work he did as part of his service, he was entitled to fuel and other pertinent materials, which he obtained from the state. The state would also provide him with unskilled labor, in the instances when, it appears, work-orders exceeded his and his family’s production capabilities. Although the service exacted from the potter by the state took primarily the form of pot-making, much of the labor he owed was used for completely unrelated projects. This was done at the state’s discretion, and depending on current economic needs. [Steinkeller 1996:249f.]

Steinkeller also points to evidence which indicates that most other crafts were organized in the same way, with the exception of stone-workers, gold-smiths, and other craftsmen who produced luxury goods. The latter worked with high-value raw materials which could only be obtained from foreign sources and which were thus usually controlled by state organizations. Many of these sorts of craft specialists were employed not in home-based workshops but in workshops that were directly controlled by temple or palace organizations: “Such workshops, which usually, but not always, were located within temple or palace complexes, were concerned with the manufacture of luxury or strategically important goods, such as jewelry, elaborate garments and other types of apparel, expensive furniture, weapons, wagons, chariots, and boats” (p. 252). Nevertheless, “the majority of other, more utilitarian crafts, such as leather-working (ašgāb), reed-working (ad-KID), carpentry (nagar), and possibly even metal-working (simug, tibira), were organized and operated very much like the pottery industry” (p. 251).

The important methodological point here, as Steinkeller notes, is that a superficial reading of the economic texts presents a quite different picture: “Since, in those sources, especially in ration lists, craftsmen are usually listed collectively, the impression is created that they were organized into workgroups, with each group permanently working in a specific location. However, as the case of the Umma potters suggests, this is but an illusion” (p. 252).

Clearly, Steinkeller’s results support Gelb’s view of the nature of the labor force in Ur III times and they contradict Diakonoff. But in spite of his emphasis on the household-based organization of the economy in early Mesopotamia, Gelb nowhere suggests that the entire state was seen as a single household, albeit composed of many individual households. His failure to do so no doubt resulted from his concern to distinguish public from private households, a distinction that must be questioned if everyone were in some sense a member of the same huge household. Yet, as we have seen, Gelb’s view of third-millennium society lends itself to an interpretation in terms of the PHM, especially if the public-private distinction is recast in the way that I have suggested. Certain activities of each householder can be considered “private” as opposed to “public” (or “privately public”), to use Gelb’s terms; but each household nonetheless took its place in a single hierarchy at the top of which stood the ruler of the state—and, ultimately, the gods. Varying degrees of freedom of action within such a hierarchy did not depend on the existence of a “private” versus a “public” (or a “free” versus a “nonfree”) sector of persons and property, which is an anachronistic distinction, in any case. Individual freedom of action was determined, instead, by the degree of centralization of the whole patrimonial administration in any given period; that is, by the ability of the heads of master households to supervise and control the activities of subordinate households.

12 See Sallaberger 1996 for more data on Babylonian potters and pot-making. Note also the archaeological work done at the fourth-millennium n.c. “Uruk Mound” of the southern Mesopotamian site of Abu Salabikh by Susan Pollock et al. (1996), who conclude that the patterns of distribution of “tools and manufacturing debris associated with making implements and ceramics” which they observed “do not lend support to the model of strongly centralized control of production, at least for pottery-making and a variety of subsistence-related activities” (p. 697). Instead, it seems that “the manufacture of pottery and the production and use of stone tools for processing plants, animals, and their products were widespread in the community, rather than organized or controlled by a few centralized institutions” (p. 683).
Bronze Age Bureaucracy: Functional Necessity or Anachronistic Assumption?

Patrimonial states may be highly centralized, as was the case during the Ur III period (see Steinkeller 1987a), or they may be decentralized, with greater independence on the part of patrimonial officials (i.e., the senior members of the ruler’s “household”) and their subordinates lower in the hierarchy. But a highly complex administration, as in the Ur III state, is not equivalent to a bureaucracy, in the sense of a system of government based on an impersonal constitution and legal-rational legitimation. Piotr Michalowski, noting Weber’s distinction between patrimonialism and bureaucracy, concludes that the Sargonic and Ur III states, despite their complex administrations, relied solely on traditional and charismatic forms of domination. As he puts it: “bureaucratic domination, with its reliance on strict rules and professional competence and training, never developed in these states” (Michalowski 1987:68). Jean-Pierre Grégoire had earlier described the Ur III king as a patrimonial ruler who regarded the state as his personal household. As he puts it:

Il gère l’empire comme sa maisonnée personnelle. Le pouvoir et l’autorité sont fortement personnalisés, de sorte que la sphère publique se sépare difficilement de la sphère privée. L’État s’identifie au palais, l’administration du palais se confond avec le gouvernement. La famille du souverain joue un rôle éminent dans la vie publique et dans les affaires de l’État.

More work must be done to substantiate this thesis, but these scholars are surely right to question anachronistic notions about the organization of power in third-millennium Mesopotamia and, by extension, the rest of the ancient Near East. Their view may be contrasted with that of Buccellati (1977; 1996), discussed above in chapter 9.3, who has argued for the existence of both bureaucracy and a public-private dichotomy in the earliest Mesopotamian cities. But the bureaucratic model of third-millennium Mesopotamia coexists uneasily with what we know of the nature of kingship in this period (and later). Buccellati argues that kingship, and the ideology that went with it, was not an inherent aspect of impersonal urban bureaucracy, which was simply a pragmatic, functional adaptation to increasing size. He points out that, unlike bureaucracy, kingship was couched in highly personal terms, and so provided a unifying symbol and motivation to the entire social group.

Like bureaucracy, however, kingship was also functional, according to Buccellati. As he puts it: “It is a way in which the system tries to reestablish the role of the person at the very apex of its function-oriented, depersonalized, bureaucratic hierarchy. . . . The nature of kingship appears in this light as a built-in device which balances what there is of impersonal, mechanistic and rigid [sic] in the bureaucratic structure” (Buccellati 1977:34). In this way, Buccellati attempts to relate the manifestly personal character of kingship to the impersonal bureaucratic mechanism that, in his view, governed the Mesopotamian city and served as a tool of royal power. The depictions of the king as good shepherd, wise judge, and benevolent father were in the nature of propaganda designed to promote royal authority and social cohesion; but these symbols were at odds with the actual structure of authority within the city. According to Buccellati, then, and to others who have adopted the functionalist model of ancient society, the personalized symbolism of the “house of the father” was an epi-phenomenal ideology which masked or (to speak more positively) facilitated the “real” underlying economic relationships and interdependencies on which the society was based (see the discussion of ideology in chapter 5.1 above).

A simpler hypothesis, however, and one that is more faithful to the evidence, is that the personal authority exercised by the king at the apex of the administrative hierarchy actually reflected the nature of political authority at every level. Buccellati himself cites an inscription of Sargon of Akkad which states that 5,400 men ate daily in his presence (for a transliteration and translation of this text, see RIME 2, nos. 11 and 12 = Frayne 1993:27ff.). As Buccellati says: “what [Sargon] boasts of is both the numerical size of the group (presumably functionaries and bureaucrats) who are directly dependent on him, and the fact that he has a personal contact with all of them on a permanent basis” (ibid.). At the highest levels of government it was apparently important that the king maintain a personal relationship as patron of each dependent official. But if the bureaucracy was a semiautonomous, impersonal “system” on top of which royal authority was simply grafted, why did the king need to foster these personal ties? Buccellati suggests, quite plausibly, that the king aimed thereby to ensure loyalty to his person; but the royal officials are then no longer operating as dispassionate bureaucrats whose only allegiance is to the rules of an impersonal system.13

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13 Compare the statement by Kirk Grayson (1995:963) concerning provincial governors 1,500 years later, in what
Furthermore, it seems likely that the personal link between the king and his officials was necessary, not simply as a means of securing their allegiance, but as a means of delegating the ruler’s personal authority to his staff. This is indicated by the practice of replacing the seals of officials, even if they retained the same rank, whenever a new king took power. A seal certified that its owner’s position and authority had been individually and personally confirmed by his master; but if administrators exercised authority simply by virtue of occupying established bureaucratic offices, it is hard to see why they should need individual reappointment by each king. If, on the other hand, there was no concept of an impersonal bureaucratic system, but only the belief that administrators were royal servants whose authority was personally delegated to them by the king, then the necessity of an ongoing personal relationship between the ruler and his officials is explicable.

Now, the use of administrative seals in ancient Mesopotamia might itself be taken as evidence of impersonal bureaucracy and the corresponding separation of public and private spheres. Buccellati (ibid., p. 31), for example, argues that, already in the preliterate period, administrative seals reveal the “impersonal” extension of authority (dispensing with face-to-face contact) that is characteristic of bureaucracy. In her discussion of the functions of seals in the Ur III period, Irene Winter (1987) also apparently assumes that there was a rigid distinction between “official” and “private” seals. But ancient Mesopotamian seals, even “official” seals, were specific to one person; there are no seals that simply represent a bureaucratic office. This point is well made by Nicholas Postgate, who argues that:

Certainly some officials had more than one seal, one mentioning their office, another not; and by their own choice they may have used one for private, another for official transactions. But it remains to be demonstrated that the state exercised any control of this choice, or that there was any legal significance in it. . . . I suspect that even in Ur III any seal represents the person, in all his roles. Even in today’s bureaucracies individual seals are rare—the bureaucratic instrument is the institutional stamp, usable only by authorized officials but not confined to any individual officer. “Seals of office” in this sense do not seem to occur in Ur III . . . [Postgate 1988b:186]

Consequently, seals (and many kinds of written documents) need not be viewed as “impersonal” at all. On the contrary, they served to represent a particular person in situations in which he or she was not physically present. There is no evidence that, in their personal affairs, administrators made use of “private” seals, employing different, “official” seals for “public” business. And even if such evidence existed, it would not prove that administration was bureaucratic rather than patronial, for, as I have suggested above, the distinction “public” versus “private” should not be made, in functionalist fashion, at the level of the entire social group, as if there existed a concept of the impersonal “state” composed of a “public” body politic. Rather, from the point of view of the PHM, the terms “public” and “private” awkwardly describe the two types of social action possible at the level of the individual household. “Public” refers to the actions of a householder, great or small, in his role as servant of his personal master; whereas “private” refers to the actions of the same person as master of his own household.

When this definition is applied to Buccellati’s examples, the inadequacy for ancient Mesopotamia of the modern dichotomy between what is “public” and what is “private,” as this distinction is normally understood, immediately becomes apparent. The construction of a monumental building—a temple or a palace, for example—was done by the divine or human ruler’s servants in order to provide him or her with a house, in exactly the same way that any household’s dependents labored on his behalf to build and maintain his household. The fact that the master’s dependents also benefited from it indirectly does not make such an action a “public service,” in any general sense; on the contrary, servants (including the human king as servant of the god) found their place in a household only by means of “private” personal relationships to the master.

It must also be said that the PHM is not at variance with the evidence, in certain periods, for a high degree of “practical” as opposed to “formal” rationality, to use Weber’s distinction (discussed in some detail in chapter 4.3 above). An example of practical administrative rationality is the Ur III practice of periodically relocating governors in order to prevent them from
building an independent power base. During the Ur III period, in particular, the rulers of Mesopotamia developed a variety of complex administrative procedures, accompanied by copious written records, in an attempt to control and supervise the activities of their subordinates. As Gelb and Steinkeller have argued, however, even here the management of productive activities took place within a hierarchy of households. Thus a highly developed practical rationality was in the service of the substantially rational goal of maintaining a traditional, and thus sacred, social order symbolized as a gigantic patrimonial household, and there is no need to posit the existence of an administrative system based on formal rationality—an impersonal bureaucracy disembedded from traditional networks of personal relationships between masters and servants or “fathers” and “sons.”

3. The Kingdom of Ebla in Early Bronze Age Syria

On the basis of cuneiform tablets found at Ebla, a city in inland Syria situated 55 km south-southwest of Aleppo, we can say that the PHM applies equally well to third-millennium Syria, if not better. Yet in describing the basic social and economic organization of Ebla in this period (ca. 24th century B.C.), Alfonso Archi (1992), the chief editor of the Ebla texts, has adopted Diakonoff’s two-sector “Asiatic mode of production” model, although he has not articulated this in detail in the way that Heltzer and especially Liverani have done for Ugarit. (Note Archi’s [1973; 1977] earlier application of the two-sector model to Hittite society, following Diakonoff.) Citing the ration lists, Archi emphasizes the large size of the palace establishment resident in the city of Ebla, which included “approximately one hundred members of the royal family; a few hundred high officials and members of the nobility and their families; and over 5,000 Palace male servants, craftsmen and simple workers” (ibid., p. 25). If we add the families of these personnel, we obtain a figure of ca. 20,000 people, according to Archi, who were “dependent on the Palace or indirectly bound to it” (ibid.), and who thus constituted the large majority (if not the entirety) of the population of the capital city (see also Archi 1982; 1986b; 1988b; 1988c; 1990a; a similar view may be found in Milano 1995).

Archi contrasts this urban-based palace establishment with the largely autonomous rural villages, which maintained their age-old way of life but were forced to turn over “part” of their land to the powerful palace organization:

The Eblaite system of government was one in which an urban center, which in fact corresponded to the Palace structure, was complemented by a rural territory with many villages that were obliged to surrender part of their land to the Palace. It is probable that it was precisely those villages which cultivated part of the Palace holdings through corvées. In addition, the villages were also obliged to contribute heads of livestock to the Palace... It was the type of agriculture practiced, that is, over extended areas of land, which guaranteed the individual villages a certain degree of autonomy from the Palace. This archaic phase in the history of the formation of the state presents a dichotomy: maximum concentration of power at the center of the structure and maintenance of the original social organization of the territory. [Archi 1992:27f.]

This, of course, is yet another application of the now-familiar two-sector model I have criticized above in chapters 9 and 11, which is founded upon the assumption of a fundamental urban-rural dichotomy in the Bronze Age Near East. Again we hear of a dichotomy between autonomous villages, which preserved their traditional communal social organization, and a powerful urban elite, who extracted surplus goods and labor from the villages to support themselves, their servants, and a large group of attached specialists. The implication is of a clear sociological separation, in terms of both way of life and ethos or ideology, between rural peasants and urban dwellers. No thought it is given to the possibility that this dichotomy is an illusion fostered by the modern concept of urbanism. As in the case of Late Bronze Ugarit, however, we should ask whether the textual and archaeological evidence from Ebla actually requires us to conclude that several thousand urban palace dependents worked full-time for the palace, so that they and their families subsisted on rations derived from the taxes and corvée labor imposed on rural villagers, who thus formed a distinct social class. Is it not possible that royal rations were merely supplemental to a mode of subsistence practiced in common by city-dwellers and villagers alike, namely, cultivation of hereditary landholdings which were granted by the king in return for service of various kinds, including both specialized and unspecialized service? Can the evidence be interpreted in terms of a single royal service system which encompassed urbanites and villagers alike, so that the city of Ebla would not have been sociologically distinct from its hinterland, but would have been characterized by the same sorts of agrarian subsistence practices and kin-based social relations as existed in smaller settlements throughout the kingdom?
Royal Ownership of Land and Grants of Whole Villages

In criticizing Archi’s acceptance of the two-sector model it should be noted that there is no need to challenge his (and others’) conclusions concerning the impressive scale of royal authority in Ebla and the strongly centralized management of the Eblaite kingdom by the king and his officials. Indeed, the impression we get from the texts is that the king owned all the land, at least in theory. As Steinkeller has described it, citing the work of Archi and others:

At Ebla, political and economic power rested, to the apparent exclusion of all other social groups, in the hands of the royal family and the aristocracy. . . . The primary possessors of land were the king and his family, who were free to dispose of it at will. There survive numerous records of land-grants that were made by the king and the members of his family, involving truly enormous acreages. Such donations often included whole villages with their resident populations. [Steinkeller 1999:299f.]

Very similar phenomena are attested in the second millennium B.C. at Mari, Emar, Alalah, and Ugarit, prompting Steinkeller (1993; 1999) to posit a “north Syrian” king-centered socioeconomic model, originating in the third millennium, which in turn resembled the northern Babylonian (Akkadian) system and was in marked contrast to the southern Babylonian (Sumerian) temple-based system. However we interpret the differences between Sumer and Syria in this regard, it is clear that the extent of royal land ownership at Ebla, including ownership of whole villages, calls into question the existence of a “free” village sector (see my discussion of ownership of whole villages in Middle Bronze Age Alalah in chapter 12.7 below, where I criticize Zaccagnini’s awkward explanation of this phenomenon as an exception to, or declension from, what he assumes to have been a prevailing Asiatic mode of production).

By rejecting the two-sector model I am not suggesting that the palace establishment was weak and unimportant. On the contrary, it is likely that the “house of the king” (É EN), incorporating the subordi-
“KI É GN” or “X KI É GN” “(X measures of) land of the house: GN”

“X GÁNA.KI GN” “X GÁNA.KI of land: GN”

“(1) è X GÁNA.KI GN” “(one) house (with) X GÁNA.KI of land: GN”

(A complete list of references is given in Milano 1996:153ff.)

Milano translates the logogram è in these texts as “farm” or “agricultural production unit,” not “house” or “household” (ibid., p. 153), implying that this term denotes an isolated rural farmstead located “near” but not actually in the named village. Like Archi, he thinks that such farms belonged to a special category of palace property that was distinct from the houses and land of ordinary villagers (cf. Heltzer and Liverani’s idea of a “royal landfund” in the kingdom of Ugarit separate from village land, discussed above in chapter 11). In an earlier article, Milano (1987b:540f.) suggested that: “The land property [in the kingdom of Ebla] is plausibly divided up between communal lands pertaining to rural communities and private estates owned by palace officials and members of the royal family.”

But if that were so, what are we to make of the cases in which entire villages were given to high-ranking persons, as shown in lists which are, “d’un point de vue rédactionnel, semblables aux listes d’unités domestiques,” as Milano himself has said? ARET 7.152 lists 22 villages that belonged to a man named Napha-II (URU.KI nap-â-î), and ARET 7.153 lists 8 villages of Giri, 9 villages of Irî, 5 villages of Napha-II, and 14 villages of Ingar. (Two of the four sections of ARET 7.153 simply list the village names followed by the phrase URU.KI URU.PN, “villages of PN”; the other two sections have just the relative pronoun “LÚ” in the phrase “LÚ PN” after the village names.)

Rather than inferring a structural distinction which is not explicit in the texts between “communal lands” worked by villagers and “private estates” owned and operated by high-ranking persons, we can account for all of the evidence by supposing that “ownership” of individual farms and of entire villages alike was a matter of possessing the authority to dispose of their surplus production, and probably also of the labor services of their inhabitants more generally. And there is no reason to doubt that these inhabitants were, in both cases, ordinary village residents who themselves possessed customary hereditary rights as sharecroppers of the land they worked. Indeed, regardless of the prevailing land tenure system, it is unlikely that the Ebla kingdom was characterized by highly dispersed rural workers living in small groups on scattered farmsteads. Because of the need for mutual aid and kin-based social interaction, the typical pattern for many centuries in the preindustrial Levant was for farmers and herdsmen to live, not in isolated estates, but in nucleated settlements.

“Houses” and Villages

On present evidence it is impossible to prove conclusively that royally granted estates were in ordinary village communities rather than isolated farmsteads in a special palace sector—and it is equally impossible to prove the opposite. The texts are ambiguous on this score. But the simplest interpretation consistent with the evidence is that each “house” mentioned in the land management texts was coterminous with the named village, or at least was an integral component of it, as in cases where a village was subdivided among several grant recipients. This explains why it was sufficient in summary texts such as ARET 7.152 and 7.153 simply to list various whole villages as one person’s property.

Furthermore, in addition to the highly abbreviated lists of individual estates and villages assigned to prominent members of the palace establishment, there are a few royal grants and “verdicts” which shed some light on the nature of these property assignments. One such text is TM 75.G.1766, written in the form of a letter to Ingar, the recipient of a royal grant (this text is published in Fronzaroli 1979, with a revised translation in Fronzaroli 1981:171; see also the comments on certain parts of this text in Kienast and Waetzoldt 1990:64f.):
Translation:

Thus (says) the king to Ingar:

“Listen. For ten years the king will set aside and give to you a house in Baytân which has 2,000 (measures of land).\(^a\)

Moreover, I will not take (it) [or: receive (revenue from it)] for ten years. It is set aside (for you) and it will be your residence outside the city (of Ebla).\(^b\)

From time to time you will reside outside the city (of Ebla).”

Year in which the sacrifice was made.

Notes:

\(^a\) The phrase which describes the property granted to Ingar was originally read by Fronzaroli as: ʾē in ba-da-a ša tab-li-im AB (= Sum.abbita), “the house” in Baytân of Tāb-Lim, the father” (or, in his translation, “la proprietà in Batayn, quella di Tāb-Lī’im, l’intendente”). In a later article, however, Fronzaroli (1981:171) reinterpreted the sign he had read as TAB as the numeral “2,” and he read the logogram AB as the Eblaite conjunction ʾap, “also, moreover” (cf. Ug. and Heb. ʾap), rather than as Sum. abba, rendering the text as follows: ʾē in ba-da-a ša 2 li-im ap an-na NU BA4.TI, “la proprietà in Batayn, quella di 2000 (iku). E quindi io non (la) prendo.” In this he followed Edzard’s (1981a) interpretation of AB as the conjunction ʾap in TM 75.G.1444 (see also the text references in Edzard 1981b:119). Inspection of the published photograph of TM 75.G.1766 (Fronzaroli 1979:fig.1) confirms the reading 2 li-im as opposed to tab-li-im. Bonechi’s (1997:479) alternative interpretation of this text is interesting but implausible. He takes ba-da-a as an ordinary noun, the dual of Eblaite ʾbayitu(m) (which it might well be even if it is a place name), and translates “LNA.SUM ʾē in ba-da-a ša 2 li-im” as “il t’è donné la proprietà dans les deux maison(nèe)s, celle des deux lî?mû,” where lî?mû means “gentilic group,” i.e., “clan.” But if this is correct then there is no identifying information concerning Ingar’s estate, for the grant would then mention neither a place name nor clan names. Furthermore, “2 li-im” is the usual way to write “2,000,” and in assignments of village property a land measurement is normally given.

\(^b\) Given the context, ĠIGAL here probably means “residence,” as Fronzaroli suggests on the basis of Akk. manzûzu, which is equated with Sum. gīšgal in some Mesopotamian texts. But ĠIGAL-kā URU.BAR should probably be translated “it is your residence outside the city (of Ebla)” and not “your residence will be the village,” as Fronzaroli has it.

This translation is uncertain at several points; in particular, the meaning of SAG.DU₈ is not clear. In lexical lists from Ebla this logogram is equated with Eblaite gu-rī-šu and gu-ra-zu-um (see Conti 1990:105ff.). Fronzaroli (1979:6f.) relates gu-rī-šu to the West Semitic root g-r-š, “to drive out,” and he compares the phrase “SAG.DU₈ wa LNA.SUM” in this text to the Akkadian formula “RN . . . ittiši . . . u iddinšu” (“RN . . . took . . . and gave it”) which is used frequently in royal grant texts from Ugarit a thousand years later (see Huhnergard 1989:251 for references and grammatical commentary). This leads him to translate SAG.DU₈ here as “reso disponibile.”

But Giovanni Conti (1990:105f.) notes that in Sumerian sag-du₈ has to do with the manipulation of physical material. For that reason, Conti prefers to read the Eblaite equivalent gu-rī-šu as /quršu(m)/; cf. Akk. qurrašu (qarašu), “to cut off, trim, carve.” Similarly, the variant gu-ra-za-um should be read /karašumu(m); cf. Akk. karāšu (qarašu), “to pinch off (clay), to break off (a piece of wood, cane, a bone, etc.).” In the context of the text under consideration here, SAG.DU₈ most likely means “to set aside,” in the sense that the king has separated the property in question from the rest of his holdings and has designated it for the use of Ingar for a period of ten years. Interpreted this way, SAG.DU₈ wa LNA.SUM can still be viewed as semantically parallel to the later Akkadian formula, despite the difference in literal meaning between Eblaite gu-rī-šu and Akkadian násû (see also the comments on SAG.DU₈=LNA.SUM as compared to násû—nadanu in Kienast and Waetzoldt 1990:64).\(^{17}\) The question is how to interpret the phrase “ʾē in ba-da-a.” Was it an isolated estate within the broader territory of the village of Baytân, as the two-sector model suggests? In view of the sizable land area allotted, a more likely possibility is that the grant to Ingar treats the entire village as a single “house” or estate assigned to one person.

This extended meaning of the term ʾē is clearly excluded in certain cases. For example, one land

\(^{17}\) Milano (1987a:187) has proposed that a GÂNA.KI is only one-sixth of an iku (i.e., 0.06 ha). Alternatively, Pomponio (1983), followed by Archi (1993b:121), argues that the GÂNA.KI (also written GÂNA.KEŠDA and GÂNA.KEŠDÂ.KI) is one-tenth of an iku, in which case 2,000 GÂNA.KI is only ca. 70 ha. Arguments for a larger GÂNA.KI, equivalent to the iku, are presented below.
management text (ARET 3.461) mentions “62 ∈ i-za-
ar”, which is probably a tally of the individual households in the village of i-za-ar, although this text is unfortunately too fragmentary to be interpreted with confidence (in any case, 62 private royal estates situated “near” the village seems highly unlikely). But if in contexts where only one house is mentioned, as in the majority of the land management texts, we actually have an entire village referred to as a “house,” then this would explain the cryptic references to “’GI GN,” “KI GN,” or “KI ∈ GN” in various texts, that is, “the ‘house’ of GN” or “the land of (the ‘house’ of) GN,” as opposed to “an estate near GN.”

For example, in ARET 7.155 there are many entries of the form “KI ∈ GN” in a text which deals (at least in part) with the assignment of property to the sons of Irik-Damu, himself a son of the vizier Iribium. In most of these entries no land measurement is given, implying that all of the land of the village had been assigned to a high-ranking person. In a few entries the amount of land is specified, but in most such cases it is clearly a matter of dividing the land of the village among multiple recipients. Of course, this kind of subdivision might have occurred in the royal grant of land at Baytán, of which Inur received 2,000 units, but in ARET 7.155 the amounts are considerably smaller than 2,000 in the case of multiple recipients, and, as we shall see on the basis of other Ebla texts, 2,000 GANA.KI is a plausible amount of land for an entire village.

The following translation of the first two sections of ARET 7.155 adopts this alternative interpretation of ∈ as a term which in this context refers to the whole village as one man’s estate:

**ARET 7.155** (obv. i 1–iii 1)

KI ∈ ma-la-du KI ∈ za-la-mak KI ∈ ša-da
ŠE ∈ SA.ZA.KI

KI ∈ UGULA lu-ub KI ∈ du-si-gā KI ∈ na-ki
KI ∈ ga-ra-ma-nuk 2 li KI a-ša-lu-gā KI ∈ UGULA mu-za-du
KI ∈ GIM-lu wa AB.SI GA a-ri-mu a-ri-mu EDIN
sā-gā-sī

Translation:

The land of the “house” M., the land of the “house” Z., (and) the land of the “house” Š.

(is assigned for the production of) grain for the palace household.a

The land of the “house” of the governor of L., the land of the “house” D., the land of the “house” N., the land of the “house” G., 2,000 (units of) land of A., the land of the “house” of the governor of M., the land of the “house” G., and, in addition, the Arimmu-settlements(?)/people(?) of the steppe,b (are all assigned to) Sagusi.

Notes:

a The term SA.ZA.KI (or, better: sa-saga KI) refers to the palace establishment in distinction from the rest of the city of Ebla; i.e., the palace “not as a center of political power but as a center for the control and distribution of goods” (Civil 1983:240).

b For this meaning of EDIN, “steppe,” see Archi 1993b:11 n.15. Concerning AB.SI.GA, see ARET 7 [glossary], p. 203. The repetition of the place name Arimmu (a-ri-mu a-ri-mu) may indicate a plural gentilic, as Edzard suggests in relation to ARET 2.27 §11 (p. 66), which mentions “1 li GANA.KI a-ri-mu a-ri-mu,” “1,000 GANA.KI of the inhabitants of Arimmu.”

After the assignments to Sagusi there are several similar sections of this text in which the property of various villages is assigned to the following persons: Dab-su-x-Damu, who was a dependent of Sagusi (in šu sā-gā-sī, “in the hand of Sagusi”), the vizier Iribium’s son Napha-Il (written nap-ha-ī), Iš-giba’īr, Síti-gilu, [GABA]-[Da]mu, Amur-Damu, Iram-Damu, Buda-Malik, Abu-Adda, and Zin-Damu, for a total of 1,000 measures; and 500 measures each in the village of ni-za-a-du go to Iram-Damu, Buda-Malik, Abu-Adda, and Zin-Damu, for a total of 2,000 measures. The latter division of 2,000 measures of land in ni-za-a-du among four men may explain the summary at the end of this text, which reads: “the quarter-shares of the sons of Irik-
Damu” (NIG.Á.GÁ-4 DUMU.NITA.DUMU.NITA /-rí-ik-da-mu). Does this indicate that 2,000 measures was the entire land area of this village? It is difficult to be certain, but it is worth noting that the land areas of villages mentioned in cases where there is no evidence of a division among several recipients tend to be on the order of 1,000–3,000 GĀNA.KI, although in a few cases the areas listed are as low as 200 and as high as 4,000 or more (see the texts cited in Archi 1980:9f.; 1982:215). A typical text is ARET 2.27, which has 13 entries of the form “X GĀNA.KI GN,” of which 8 entries have quantities of 1,000 GĀNA.KI. 3 entries have 400 GĀNA.KI. 1 has 500, and 1 has 600.

The colophon reads “tablet of the GĀNA.KI (land areas) of the brother(s) of Ibdulu” (DUB GĀNA.KI ŠEŠ.MU ib-du-lu).

An unpublished text (TM 75.G.1439+), briefly described by both Archi (1990a:53; 1992:26) and Milano (1996:150), documents the following estates of high officials: 30,600 GĀNA.KEŠDA.KI (= GĀNA.KI) measures belonging to Iriba, distributed over 21 villages, for an average of 1,457 per village; 19,160 belonging to Dubu-Išar in 17 villages, for an average of 1,127 per village; and “the even larger estate of a certain Enkilu [that] was made up of plots in more than 30 villages” (Archi 1990a:53). Unfortunately, it is not clear whether this text gives a single land area for each village, suggesting that the person named owned the whole village, as opposed to multiple references to each village, which would indicate that each man owned only a portion of the land in each village (which is Archi’s interpretation).

ARET 2.27a has similar entries and it records a total of 9,000 GĀNA.KEŠDA measures of land assigned to Tiša-Lim, who is known elsewhere as the queen of the city of Emar on the Euphrates River, to whom Irkab-Damu, king of Ebla, gave the land (amounts unspecified) and local landholders (NA.SE11.NA.SE11) of various entire settlements, as recorded in TM 75.G.2396 and TM 75.G.1986+ (published in Fronzaroli 1984a and Archi 1990b).18 ARET 2.27a §§ 2–3 is thus a more detailed record of Tiša-Lim’s landholdings in five villages, listing 2,000 GĀNA.KEŠDA in each of four villages and 1,000 GĀNA.KEŠDA in the fifth village; section 1 of the same text mentions 1,700 and 900 GĀNA.KEŠDA, respectively, in two additional villages. It would be odd if Tiša-Lim’s property in each of these villages were merely a small parcel among other such parcels granted to various palace officials or reserved for the villagers as their own communal land, for she was the queen of an allied city situated far away across the steppe, 117 km east of Ebla.19 It is more likely that ARET 2.27a describes the total area of some of the villages granted to her; indeed, two of the places mentioned in this text (ḫu-ra-zuški and a-bi-ha-duški, with 900 and 2,000 measures of land, respectively) are mentioned in the royal grant of villages and their NA.SE11-men to Tiša-Lim by Irkab-Damu (TM 75.G.1986+). Moreover, indications in other texts that some of the same villages belonged to sons of the vizier or other high officials of Ebla (noted in Archi 1990b:27) do not necessarily pertain to the same time period. Thus such evidence of multiple ownership does not imply that the land areas specified were only fractions of a larger territory, for we know that villages could be transferred from one person to another in their entirety.

How Big Is a GĀNA.KI?

If the term ē can refer to the whole village, especially in the absence of a measurement of land area, then this would account for the fact that in ARET 7.155 and other texts no information is provided concerning the location of the granted estate or its inhabitants other than the name of the village. It would also account for the fact that only one “house” is ever associated with a given village (i.e., there are no entries which mention the assignment to one person of two, three, or more estates associated with one place). Thus entries which record the “houses” and land of various villages assigned to certain persons would merely be expanded versions of the lists of villages that are recorded as having been assigned in their entirety to the same or similar officials (e.g., ARET 7.152 and 153). The amount of land associated with a given town or village in such texts would thus normally be the total area of the agricultural land of the settlement, not just one parcel within it. This is especially plausible if the GĀNA.KI at Ebla is more or less equivalent to the Sumerian iku (ca. 0.36 ha)—also written logographically as GĀNA—as most Ebla specialists had initially supposed.20 Land measurements

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18 On the meaning of NA.SE11 as “landholding serviceman,” see the discussion of “Households and ‘Work-teams’” below.

19 The reciprocal relations between Ebla and Emar are demonstrated by the clauses in the grant text TM 75.G.2396 (obv. v 3–rev. ii 2) which state that the trading agent (L.U.KAR) of Irkab-Dam, the king of Ebla, would be able to ship goods from the towns of Tiša-Lim, just as her trading agent would be able to ship goods from the towns of Irkab-Damu (ap mi-nu URU2, URU2 ti-ša-li-im MÁŠKIM EN L.U.KAR E ap mi-nu URU3, URU3 EN MÁŠKIM 2-ša-li-im L.U.KAR E).

20 The iku is 120 × 120 cubits = 60 × 60 m (3,600 m²), where 1 cubit = 50 cm. Powell (1989:480) notes that as a basic field measure “[t]he iku (c. 0.36 ha.) fills the same
typically range from 1,000 to 3,000 GÂNA.KI; that is, 360–1,080 ha at 0.36 ha per GÂNA.KI, which corresponds well to the expected area cultivated within a radius of ca. 1–2 km around a secondary settlement (ca. 300–1,200 ha; see Wilkinson 1989).

As Archi (1980:8f.) has noted, “at Ebla gâna-
keš-da-ki (and its variants) is usually preceded by
totalling several hundreds or even thou-
sands. Therefore, if the reckonings concern iku-
numbers totalling several hundreds or even thou-

ke
š
(300
–
ha) and that the AN.ZAMx at Ebla is equivalent to the
Sumerian sîla (ca. 1 liter), Milano determines that
the “average sowing rate” of ca. 4 AN.ZAMx of barley
per GÂNA.KI corresponds to the plausible figure of ca.
65 liters of seed per hectare. But this average masks a
wide variation in which the smallest sowing rates
reported in the texts are obviously too low (even ac-
cepting Milano’s assumptions), if we assume that all
of the land was sown.

Milano (1987a:185) admits that “such a high
degree of variation in the sowing rates raises some per-
plexing issues,” and he suggests that there was wide
variation in the productivity of the land or perhaps in
the distance between furrows. But he neglects to con-
sider a more likely source of variation: the presence
of fallow land. The variation in sowing rates raises
the question of whether all of the land area listed
in the texts was actually sown. It is possible that vary-
ing amounts of fallow land were included in the recorded
land areas, and if this is so, we must increase consid-
ernably Milano’s estimate of the size of the GÂNA.KI
unit.22 In light of the difficulty of determining actual
sowing rates from these texts, it is better to estimate
the total area of the zone of cultivation around a typi-
cal Bronze Age dry-farming settlement (i.e., 300–
1,200 ha within a radius of 1–2 km) and to compare
this with the land areas reported for various villages
in the Ebla kingdom. As we have seen, land mea-
surements typically range from 1,000 to 3,000 GÂNA.
KI per village, and there is no indication in the texts
that these areas do not correspond to the entire farml-
land of the village with which they are associated. At
0.36 ha per GÂNA.KI (= iku), these land measure-
ments correspond to 360–1,080 ha, which agrees very
well with the estimated area of cultivation.

This pattern is well illustrated in TM 76.G.188
(summarized in table 14 below), one of the texts
Milano uses to calculate sowing rates (ibid., pp.
181ff.). On the obverse of this tablet are listed
amounts of seed supplied to nine villages, and on
the reverse are land measurements for the same villages,
in entries of the form “N KI GN,” i.e., “N GÂNA.KI-
measures of land of GN.” Despite Milano’s assertion
to the contrary, there is no indication that these land
areas applied only to small parcels rather than to the
entire cultivated land of each village. Indeed, it is
explicitly stated that the seed was collectively

mensurational niche occupied by the N[eo-B[abylonian-]
L[ate B[abylonian] unit of 10,000 square cubits, the
Graeco-Egyptian aoura, the Roman jugerum, the Iraqi
mešara, and the donum (all c. 0.25 ha.), and the acre (c. 0.40 ha.).”

The relative sizes of these units are known, however: 1
giu-bar = 20 NLSAGSU = 120 AN.ZAMx. On capacity meas-
ures at Ebla, see Milano 1990b = ARET 9, pp. 349–52.

22 Note Steinkeller’s (1993:126 n.52) brief comment that
“both the figure proposed by Pomponio (1/10 of iku) and
that of Milano (1/6 of iku) are too low.”
charged to the account of the local landholders of the village as a group (alₖ NA.SE₁₁NA.SE₂ GN, “upon [charged to] the servicemen of GN”), not to an individual farmer. In many traditional Near Eastern villages, cropland has been managed collectively in this way, with scattered individual plots reapportioned to households each year in order to spread the risk of a low yield due to pests or poor soil. Note that for the households each year in order to spread the risk of a way, with scattered individual plots reapportioned to lages, cropland has been managed collectively in this individual farmer. In many traditional Near Eastern vil-

gal figure of 2,000 G

only 70 units of land). This is quite close to the typi-

the enigmatic entry for the ninth village, which has

charged to the servicemen of GN

We can compare the 1,000–3,000 GĀNA.KI areas recorded for a number of different villages in the kingdom of Ebla to the much larger quantity of 14,320 GĀNA.KI of land associated with Arimamu in ARET 2.26 (translated above). The anomalous spelling of that city’s name (a-rí-ma-muₖ), which occurs only here in the Ebla texts, is presumably a variant of Arimmu (a-rí-muₖ), as Bonechi (1993:51) has noted.

Table 14. Land Areas and Seed Quantities Recorded in TM 76.G.188

<table>
<thead>
<tr>
<th>Village</th>
<th>Land Area (GĀNA.KI)</th>
<th>Seed-grain (AN.ZAMₖ) a</th>
<th>Sowing Rate (AN.ZAMₖ per GĀNA.KI)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ša-gáₖ</td>
<td>3,000</td>
<td>7,920</td>
<td>2.64</td>
</tr>
<tr>
<td>da-guₖ</td>
<td>3,000</td>
<td>9,120</td>
<td>3.04</td>
</tr>
<tr>
<td>a-a-dóₖ and gi-duₖ</td>
<td>1,000</td>
<td>4,440</td>
<td>4.44</td>
</tr>
<tr>
<td>gi-wa-laₖ</td>
<td>700</td>
<td>2,400</td>
<td>3.43</td>
</tr>
<tr>
<td>za-ba-duₖ</td>
<td>630</td>
<td>3,120</td>
<td>4.95</td>
</tr>
<tr>
<td>ib-à-anₖ (= i-ì-di-à-da-kì ?)</td>
<td>5,000</td>
<td>8,400</td>
<td>1.68</td>
</tr>
<tr>
<td>ur-lumₖ</td>
<td>1,000</td>
<td>3,600</td>
<td>3.60</td>
</tr>
<tr>
<td>GIŠₖ (= i-žuₖ)</td>
<td>1,600</td>
<td>1,800</td>
<td>1.12</td>
</tr>
<tr>
<td>[x]-du-šₖa-maₖ</td>
<td>70</td>
<td>n/a</td>
<td>n/a</td>
</tr>
</tbody>
</table>


a The seed quantities are given in gi-bar units but Milano converts gi-bar to AN.ZAMₖ for purposes of calculation.

ARET 2.26

7  li GĀNA.KI KÚ URₖ
7  mi 20 GĀNA.KI ŠE.BA
3  li 6 mi GĀNA.KI KÚ SA.ZAₖ
3  li GĀNA.KI LÚ DUMU.NITA.DUMU.NITA I-gi
KI.KI a-rí-ma-muₖ (Farm)lands of Arim(a)mu.

Land Areas and Settlement Sizes

We can compare the 1,000–3,000 GĀNA.KI areas recorded for a number of different villages in the kingdom of Ebla to the much larger quantity of 14,320 GĀNA.KI of land associated with Arimamu in ARET 2.26 (translated above). The anomalous spelling of that city’s name (a-rí-ma-muₖ), which occurs only here in the Ebla texts, is presumably a variant of Arimmu (a-rí-muₖ), as Bonechi (1993:51) has noted. Arimmu is mentioned in several texts (see ARES 2, p. 109f.). If I am correct in thinking that the scribes of Ebla usually recorded the entire land area of a given settlement, then the farmland of Arim(a)mu was as much as ten times larger than that of the average village. At 0.36 ha per GĀNA.KI, the total quantity of farmland given here (14,320 GĀNA.KI = ca. 5,150 ha) is the area within a radius of ca. 4 km (45 minutes’ walk) from a central settlement. Off-site sherd
scatters have shown that 3–5 km is the typical radius of the zone of cultivation farmed by urban residents in dry-farming regions of Syria and Upper Mesopotamia during the Bronze Age, as opposed to a smaller radius of ca. 1–2 km for secondary settlements (see Wilkinson 1989; 1994). Thus if we assume that the GÂNA.KI measure at Ebla is equivalent to the Sumerian iku, or at least falls within the cross-culturally attested range of 0.25–0.40 ha for basic units of field measurement, then we obtain absolute land areas of villages versus cities which are just what we would expect in this period. And if we accept the idea that land measurements in the Ebla texts usually pertain to the entire cultivated area of a settlement, then we obtain a ratio between the farmland of the smallest settlements (1,000–3,000 GÂNA.KI) and the farmland surrounding larger centers (here 14,320 GÂNA.KI) which is what one would expect in terms of the relative population sizes of such settlements.

Unfortunately, it is not clear what is meant by SA.ZÃ.ki in this text. That term is used in the Ebla texts to denote the governing establishment ("palace") in distinction from the rest of the city (Civil 1983), but the question is whether it is the administrative establishment of Ebla or of Arimmu that is intended here. There are strong indications that Arimmu was situated quite far from Ebla, near Emar on the Euphrates River (Bonechi 1993:51; 1997: 51ff.), in which case it would have been an allied or vassal city like Emar.23 If so, it is unlikely that the produce of 25% (3,600 GÂNA.KI) of its farmland would have been shipped overland to Ebla. More likely, this produce was intended for the use of the governing establishment at Arimmu itself. In that case, the categorization of the farmland found in ARET 2.26 probably parallels a similar subdivision in Ebla itself and in other cities of the kingdom. At Arimmu 49% (7,000 GÂNA.KI) was designated for the "city" (i.e., its ordinary residents), 25% was designated for the governing establishment and its staff, 5% was for rations (for garrison troops or temporary corvee workers?); and 21% for the "sons of I-qi," who was obviously a high official—perhaps the local governor.24

Thus the large share assigned here to the "palace" (SA.ZÃ.ki) should not be taken as evidence for Archi’s two-sector model of the Ebla kingdom, which divides all of the rural territory between a palace sector and an autonomous village sector. Arimmu was not a small village but a distant vassal city that supported its own local governing establishment. In the smaller settlements of the kingdom, however, which did not have large administrative establishments, it seems rather to have been a matter of assigning the surplus produce and labor of entire villages to this or that royal official, or, by default, to the king himself.

We have compared typical village land areas on the order of 1,000–3,000 GÂNA.KI with the 14,320 GÂNA.KI associated with the city of Arim(a)mu. But Archi (1990a:53) mentions in passing an unpublished text (TM 75.G.10217) in which a much larger land area of 151,020 GÂNA.KI is associated with what he originally assumed was "the village of Dunib." This figure, equal to 54,367 ha at 0.36 ha per GÂNA.KI, is hard to reconcile with my interpretation of the other land-management texts, unless the text in question is a summary account of the landholdings in an entire region that belonged to a major vassal city. But that is just what we have here, because "the village of Dunib" is almost certainly Tunip, as Archi (1993b: 13) has later acknowledged.25

Tunip was known as an important city in the second millennium B.C., located in the vicinity of Hama (on the textual sources and the general location of Tunip, see Klengel 1969:75ff.). It is probably to be identified with the very large site of Tell Asharne (Tell el-‘Asârina), 70 km south-southwest of Ebla, strategically situated on the bend of the upper Orontes River at the point where the Orontes valley opens up to the north into the fertile Ghab plain, which measures ca. 60 km north-south and 10 km east-west. It is quite likely that Tunip was an ally or vassal of Ebla in the late third millennium, in the period of the Ebla archive, and there is no difficulty in attributing to it a densely settled territory along the Orontes River consisting of 600 km² (60,000 ha). It might well have been the major power along the middle and upper Orontes, for we do not hear of other cities in this area in the Ebla texts.

For the sake of comparison, the Late Bronze Age kingdom of Ugarit covered 2,000 km², and the cultivable territory ruled by Ebla in inland Syria east of the Orontes valley must have been at least as large. Archi (1992:24) concludes that the hegemony of Ebla extended from Carchemish in the north to Hama in

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23 In this regard it is worth noting the reference to the “Arimmu-settlements?/people? of the steppe” (a-ri-maš: a-ri-maš: EDN) in ARET 7.155 obv. ii 12.

24 This name is probably to be read /Yiqqi/, a hypocoristicon for Yiqqi-Ilum or Yiqqi-DN, “the god/DN has poured out” (see Pagan 1998:149f.). Compare the substantial landholdings assigned to the sons of the vizier Ibirium at Ebla.

25 This city is mentioned in a few dozen Ebla texts, in the same contexts as major cities such as Ursaum (Uršum) and Mari. The name is usually written du-ne-Šb, but also du-ne-Šb, du-ne-Šb, and perhaps du-na-na-Šb, du-na-Šb, and du-na-na-Šb (see ARE 2, pp. 212ff.).
the south, a distance of more than 200 km. Even if the land of peripheral vassal cities was not directly attributed to the king and vizier of Ebla and other high officials, the area they managed in the center of the kingdom was quite large.

We can also compare the 151,020 GÁNA.KI that belonged to Tunip to the total of 229,000 GÁNA.KI needed for the sustenance of the inhabitants of the city of Ebla itself (this is recorded in TM 75.G.10039, which is not published but is described in Archi 1982:214; 1988b:28; 1990a:54; Milano 1996:138, 152). According to Archi’s (1992:25) estimate, the 60-ha walled settlement of Ebla had a population of 20,000 or more, equivalent to ca. 330 per hectare, which is plausible (cf. Ottoman Aleppo, discussed in chapter 7.3 above). Of the total subsistence area of 229,000 GÁNA.KI tallied in TM 75.G.10039, 157,500 GÁNA.KI supported ordinary landholding servicemen (LÚ GURUŠ.GURUŠ KU) and the rest supported various occupational specialists, overseers, and aristocrats or “elders.”

Note that a sustaining area on the order of 200,000 GÁNA.KI for a large city of 20,000 people is what we would expect if the typical sustaining area for a small village of ca. 200 people were 2,000 GÁNA.KI. Jan-Waalke Meyer (1996:144ff.) has summarized the evidence from archaeological surveys and excavations in the Ebla region and concludes that Ebla was the center of a local settlement system in which there were two or three large walled towns (ca. 25 ha), about a dozen large villages (2–4 ha), and many small villages (ca. 1 ha). This distribution of settlement sizes is in keeping with the frequency distribution of land areas in GÁNA.KI of settlements recorded in the Ebla texts (assuming that the recorded land areas are the total cultivated areas belonging to each settlement).

An even larger land area consisting of 422,000 GÁNA.KI (151,920 ha or 1,520 km² at 0.36 ha per GÁNA.KI) is assigned to a man named Idanekimu in TM 82.G.266 (published in Archi 1993b:8ff.). But this man was a royal prince, apparently the son of Irkab-Damu, the penultimate king of Ebla, and the land in question probably constituted a large proportion of the entire kingdom. The same text lists a total of 118,715 sheep, 11,401 cattle, 1,500 minas (705 kg) of gold, 4,300 minas (2,021 kg) of silver, and 795,580 GÚ-bar of grain (ca. 95 million liters according to Milano’s calculation that 1 GÚ-bar = 120 AN.ZAM₄, and the AN.ZAM₄ is a Sumerian šílā = ca. 1 liter), as well as 498,900 GÁNA.KI of land, of which Idanekimu had 422,000. Clearly these figures represent a tally of much if not all of the wealth of the kingdom. The vast landholding (85% of the total) put under the control of Idanekimu was presumably divided among subordinate officials and their subordinates within his household, in a hierarchical arrangement the evidence for which is discussed below. Perhaps this assignment to him was made because he was acting as prince regent for his father.

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Translation:

And (herewith) I [= the king] give to Gir-Damu (the persons) Z. and B.

and the “house” A., the “house” Š., the “house” U., the “house” A., the “house” K., the “house” M., the “house” G., the “house” Z., the “house” S., the “house” D.

And (herewith) I give to Ir-Damu (the person) Ingar and (the place) L.,

the “house” H., the “house” N., the “house” G., the “house” G., the “house” M., the “house” M., the garden of M. of the ? of the lady of G. in B.

And (herewith) I give to Napha-II (the person) E.

and the “house” M., the “house” M., the “house” A., the “house” B., the “house” L., the “house” S., the “house” G., the “house” I.
Royal Grants of Landholders to Other Landholders

TM 75.G.1444 (published in Edzard 1981a) lists “houses” in various villages that were granted by the king to Gir-Damu, Ir-Damu, and Napha-II, the sons of the vizier Ibrium. Unfortunately, the cryptic formula “È GN,” with no further qualification, is used here as well, so the nature of these “houses” is ambiguous. What is noteworthy in this text, however, is that the king gives to the sons of Ibrium not just estates but other men (ibid., p. 50f.). The relevant sections of this text are presented above.

In col. vii, l. 8 of this text (in boldface above), we read that a man named Ingar was given to Ir-Damu. If, as seems likely, this is the same Ingar who receives an estate for a period of ten years in TM 75.G.1766, translated above, and who has fourteen villages in ARET 7.153 (which also mentions the same three sons of Ibrium, namely, Giri/Gir-Damu, Irti/Ir-Damu, and Napha-II), then we have here a hierarchy of households, from the king to the vizier to the sons of the vizier to a high-ranking servant of a son of the vizier.

Furthermore, this hierarchy goes even deeper, for in ARET 3.310 we hear of “1 É in-gär 4 É in-r[15]a-[s[i],” i.e., “one house of Ingar (and) four houses of his servants.”26 As Archi (1991:217) notes, Ingar was “un fonctionnaire dont le nom revient souvent dans les documents concernant l’administration agricole”; for example, in TM 75.G. 10250 there is a reference to “2 É UGULA ŠE UGULA l-GIŠ in-gär,” “two houses, (one of) the overseer of grain and (one of) the overseer of olive-oil, of Ingar” (cited by Archi, ibid., who translates this text differently). In other contexts these UGULA-supervisors, who were responsible for managing different aspects of Ingar’s estate and who presumably had servants of their own, were probably referred to as the “servants” (Ir[15]a[15]) of Ingar within a patrimonial hierarchy in which Ingar himself was a servant of Ir-Damu, the son of the vizier.

Another example of this sort of hierarchical structure, in which extensive property is held by, and could even be transferred by, a subordinate official, is found in TM 75.G.1470 (published in Archi 1981:9). This text lists several villages that a certain Is-Damu, a subordinate or dependent of Daba’u, gave to Giri (Gir-Damu) son of Ibrium (GN, GN, GN, GN, GN, GN, LÚ iš[i]-da-mu LÚ da-ba-à L.NA.SUM gi-ri DUMU.NITA ib-ri-um).

Prosopographical analysis provides further clues concerning the social position of Ingar, who was obviously a high-ranking member of the palace establishment but was not a member of the king’s family or the vizier’s family. (Note that both the kingship and viziership were hereditary at Ebla, and both the king and the vizier had many sons and, presumably, multiple wives; see Archi, Biga, and Milano 1988:218–62). Several texts identify Ingar as an UGULA E RIN BAR.AN, a type of overseer or commander who, according to Archi, “took care of the pairs of BAR.AN [i.e., equids, probably a hybrid of the donkey (Equus asinus) and the onager (Equus hemionus)] for the carriages of the king, the queen, the high officials and members of the royal family, and received fabrics and objects in precious metals from the palace administration” (ibid., p. 266; references to in-gär as UGULA E RIN BAR.AN, UGULA BAR.AN(BAR.AN), or LÚ BAR.AN(BAR.AN), are listed in Archi, Biga, and Milano 1988:268). One might speculate that such a person also had a military function as a kind of third-millennium B.C. charioteer.

In any case, the disbursements of textiles and precious items to Ingar the equid specialist indicate that he was a man of high rank. Ingar is called “overseer of the house of Ibrium” (in-gär UGULA E ib-ri-um) in ARET 3.468 r. vii 15–17. He is listed in illustrious company in ARET 4.14 §53, which records 25 measures of wool received by one Puzuri, apparently to make coverings for (among others) 6 teams of the king’s equids and 2 teams of Ingar’s (6 ÉRIN BAR.AN EN 2 ÉRIN BAR.AN in-gär; note also the reference to 6 ÉRIN BAR.AN EN and 2 ÉRIN BAR.AN in-gär in ARET 8.541 §45). A son of Ingar (DUMU.NITA in-gär) also receives textiles in ARET 1.10 §24.

Archi notes that a number of texts refer to “gold and silver objects (níg-anšé-ak, kú-sal, ma,) which we know were connected with the harnessing of equids and bridles, and other precious objects difficult to identify but always connected with wheeled vehicles and the harnessing of equids. Many of these objects were given by the king to officials of the Eblaite and other courts” (ibid., p. 285). This reflects the prestige accorded to these animals and vehicles, and presumably also to those who handled them.

26 Note that in-gär is a personal name here; it is not “one house of an engar-peasant”; thus Milano’s (1996:153) summary of ARET 3.310 is wrong to call it a “liste de fermes et leur personnel (a-ur[s], in-gär).” The term Ir[s], “servant, slave,” probably does not indicate an absolute social class or a particular function, as is suggested by the translation “servl.” It indicates a relationship of subordination or clientage: in this case, between Ingar and four subordinates who were householders in their own right (cf. later NWS abdu, which is how this term was probably read at Ebla).
Households and “Work-teams”

There is substantial evidence, then, of the organization of households in the kingdom of Ebla as a institutional hierarchy of houses within houses (for a similar interpretation see Grégoire and Renger 1988). The palace administration kept a careful count of thousands of households and their positions in the social hierarchy, that is, to which higher ranking households various subhouseholds had been assigned. In one important and unique text, TM 75.G.10250, which unfortunately has not been published but is frequently cited by Archi (1982:213; 1988b:27; 1990a:52; 1991:217), there is a list of “é NA.SE₁₁” groups, “households of servicemen,”27 with a colophon that says that 4,580 such households were part of the administrative district or “gate” (KÁ) of the town of Arur (DUB KÁ a-ru₂₁,-lu₆).28 As Archi describes it, in this text the “É NA.SE₁₁” units are listed according to the villages to which they belong, and each village presents an analogous landed structure. A first group of houses is formed by “workers” (guruš), others by “farmers” (engar), or by slaves (ir₁₁) of the Palace, or also by cattle breeders, or by wine producers, or by artisans such as smiths and carpenters. Thus, these houses were made up of personnel with different social status, and in some cases were specialized productive units. If one considers that each “house” would have been formed by an average of four people, one obtains a total of 18,320 people, all directly or indirectly dependents of the Palace, and all assigned to agriculture or connected activities. This is another element which shows how centralized was Ebla’s economy. [Archi 1982:213]

In a later article Archi gives a little more information, describing the same text as follows:

TM.75.G.10250 enregistre les “unités de travail de personnes,” é na-se₁₁, organisées en quartiers, ká, dans la ville d’A-ru₂₁,-lu₆, mais présentées aussi dans des villages voisins. Plus d’une fois on cite “une unité de surveillants de l’huile (aux dépendances) de Nuzar,” l’é u-gula i-giš Na-za-ar . . . Sous le contrôle d’Ingar . . . on trouve des “unités de surveillants des céréales et de surveillants de l’huile,” 2 e u-gula še u-gula i-giš In-gar . . . [Archi 1991:217; the reference to Ingar is discussed above, with a different interpretation, namely, “two households, one belonging to Ingar’s grain-supervisor and one to his oil-supervisor” rather than Archi’s “two personnel-units of grain-supervisors and oil-supervisors”]

Finally, there is Lucio Milano’s summary of this text as a:


It appears that we have here a census of households in the town of Arur and its surrounding villages. The total number of 4,580 households is plausible for a secondary town in the Ebla kingdom and its hinterland, amounting to a local population of perhaps 30,000, if we assume an average household size of 5–7 persons (see chapter 7.2 above concerning historically and ethnographically based demographic estimates for the preindustrial Levant). These households (i.e., their surplus revenue) were assigned to named individuals, apparently to high-ranking persons with widespread estates, like Ingar, or to persons who are identified only by their function or professional specialization. There is no reason to think that these were special palace-related personnel units staffed by full-time royal dependents, implying that there was a separate sector of ordinary households in Arur and its villages which is for some reason ignored in this text.

This interpretation conflicts with that of Archi and Milano, who translate the term é in this text not as “household” but as “unité de travail de personnes” (ibid.) or “unité de production” (Milano 1996:155). They see the É NA.SE₁₁ units as bureaucratically organized components of the palace sector, staffed by direct royal dependents, rather than ordinary households of farmers and professional specialists who gave part-time service to the king or to one of his high officials. But in that case, how does one account for the fact that TM 75.G.10250 is a record of the district of Arur, and says nothing about the palace at Ebla?

Archi and Milano are led to this interpretation because they see the É NA.SE₁₁ groups as similar to groups of “workers” called É.DURU₆₁. This term appears frequently in lists of personnel who receive rations. The É.DURU₆₁ groups are listed according to larger “gates” (KÁ) or districts, called ir-a-LUM in Eblaite (strikingly similar to the Old Babylonian bābtums discussed in chapter 12.4 below). In account texts, the standard format is “N É.DURU₆₁ Ki KÁ PN” or “N É.DURU₆₁ ir-a-LUM PN,” where N is the number of É.DURU₆₁ groups in the larger “gate” or district under the command of a named UGULA-overseer (see
This organization by “gates” leads Archi and Milano to interpret the 4,580 È NA.SE11 units in TM 75.G.10250 as squads of palace workers who are somehow associated with the “gate” of Arur. But there is no reason to assume that the thousands of individual households (È NA.SE11) recorded for Arur were regimented personnel units of this sort. The large number of È NA.SE11 units in that district, compared to the much smaller numbers of È.DURU3ki units recorded in various account texts (e.g., the number of È.DURU3ki units is 5, 7, 10, 12, 13, 14 in the 7 entries in TM 75.G.1973, cited in Archi 1988b:26f.)—not to mention the difference between the terms È NA.SE11 and È.DURU3ki—shows that these were quite different. Is it not more likely, for example, that the phrase “È UGULA l-гиš Nu-za-ar,” quoted by Archi from TM 75.G.10250, means “household of Nuzar’s olive oil supervisor,” consisting of that man’s family and servants, as opposed to “personnel unit consisting of olive oil supervisors”? In other words, TM 75.G.10250 is probably a complete census of landholding households in the district of Arur and is not restricted to production units of the “palace sector.”

Furthermore, as Waetzoldt (1986:433) and Milano (1990a) have shown, at Ebla È.DURU3ki very often simply means “20” (i.e., a “score”) as a numerical amount used in counting people (male and female) and does not refer to an actual work-team or squad. For example, ARET 4.13 §62 records the assignment of 3,952 measures of woolen textiles to personnel who are listed in entries of the form “N È.DURU3ki ir-a-lum PN” or “N È.DURU3ki GN.” We know that this means “N-score men in the city-quarter(?) of PN” or “in the village GN,” and not “N È.DURU3ki work-groups,” because some of the entries also mention a certain number of individual men (GURUS), e.g., “9 È.DURU3ki 10 GURUS ir-a-lum PN” (i.e., 9 score + 10 men = 190 men), and the total numbers of È.DURU3ki and GURUS given at the end of this section make sense only if È.DURU3ki means “20.” The recorded total is 131 È.DURU3ki and 15 GURUS, but the actual numbers in the individual entries amount to 127 È.DURU3ki units and 95 individual men. This total is correct, however, if we consider that 131 score + 15 = 127 score + 95 = 2,635 men.

Similarly, TM 75.G.1973 (cited in Archi 1988b:26f. and Milano 1990a:13) has entries which yield a total of 74 È.DURU3ki and 27 NA.SE11, but the recorded total is “75 È.DURU3ki 7 NA.SE11” (i.e., 75 score + 7 = 1,507 servicemen). Likewise, TM 75.G.308 (cited in Milano 1990a:13) lists various officials from whom sheep were due as a tax, amounting to 70 individuals, with no mention of È.DURU3ki units, but the total is recorded as “3 È.DURU3ki 10 NA.SE11” (3 score + 10). In still another unpublished text (TM 75.G.245, cited in Pettinato 1981:133 and Milano 1987b:540), a total of 350 È.DURU3ki corresponds to 7,000 NA.SE11 (7,000 = 350 score). It is clear that in at least some of these texts, È.DURU3ki cannot refer to an organized personnel unit at all but is simply a reckoning device, because in the recorded totals the same persons are distributed among a different number of È.DURU3ki than are given in the individual entries (for other examples of this in the Ebla texts, especially concerning female workers and wives in the royal palace, see Milano 1990a).

To the extent that there were actual 20-man personnel units who received palace rations, it is likely that these were companies of soldiers. ARET 4.13 §62, in particular, may refer to military personnel, recording the numbers of troops stationed in various places who received allotments of cloth or finished garments (although in this text È.DURU3ki may mean simply “20” and not an actual company of men). Eleven “ir-a-lum PN” districts are listed (neighborhoods within the city of Ebla?) with contingents ranging in size from 104 to 240 men, and there were also three village-based detachments of 80, 180, and 200 men, as well as smaller groups of specialists, including a unit consisting of 24 “charioteers,” i.e., commanders of equid teams (UGULA ERIN BAR.AN).

The question of military organization is neglected by Archi, who speaks of “palace workers.” But the far-flung Ebla kingdom must have maintained a substantial military force. Direct evidence is lacking, yet the practice of reckoning personnel by scores may well have arisen because the 20-man company was a standard military unit. This is suggested by TM 75.G.336 (published in Pettinato and Matthiae 1976; see also Pettinato 1981:136–42), a text that also indicates the social difference between landholding NA.SE11-servicemen, some of whom had the rank of commander (UGULA) in military contexts, and their lower-ranking servants or clients (IR11). The first three sections of this important text have the following form:

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20 Note that NA.SE11 does not mean simply “man” in general, for which the term GURUS is used. A NA.SE11-man could be called a GURUS, however, as in ARET 4.13 §62 and other texts. See Grégoire and Renger 1988:222f., where evidence is collected that indicates that a NA.SE11-man was “Oberhaupt einer Familien- oder Verwandtschaftsgruppe.”
Section 1:

PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.a
PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.
PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.
PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.
PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.
É.DURU,4 MAH,100 IR,11 IR,11 É.DURU,4 MAH,2 100 subordinates (are attached to) the first company.

É.DURU,4 UGULA,5 ki.MA /G64 x First [20-man] company (or “score”).b

PN PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.
PN PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.
PN PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.
PN PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.
PN PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.
É.DURU,4 MAH,2 98 IR,11 IR,11 É.DURU,4 MAH,100 IR,11 IR,11 É.DURU,4 MAH,2 98 subordinates (are attached to) the second company.

É.DURU,4 UGULA,5 ki-2 Second [20-man] company.

PN PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.
PN PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.
PN PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.
PN PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.
PN PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.
30 IR,11 IR,11 LÚ É.DURU,4 MAH,5 30 subordinates belong to the third [10-man] company.

MAŠKIM IL.ZI (Rations from? Men led by?) the MAŠKIM IL.ZI.c

Section 2:

PN PN PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.
PN PN PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.
PN PN PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.
PN PN PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.
PN PN PN PN PN PN PN UGULA 5 (4 named men and) PN, commander of five.
50 IR,11 IR,11 É.DURU,4 MAH,10 50 subordinates (are attached to) the [21-man] company.

MAŠKIM i-da-PALIL (Rations from? Men led by?) the MAŠKIM i-da-PALIL.

Section 3:

PN PN PN PN PN PN PN PN PN PN PN PN PN PN UGULA 10 NA.SE11,1 9 named men and)
PN UGULA 10 NA.SE11,1 PN, commander of 10 NA.SE11,1-men.
60 IR,11 IR,11 10 NA.SE11,1 60 subordinates (and) 10 NA.SE11,1-men;
A.AM É EN A.AM-personnel of the king’s house.

PN PN PN PN PN PN PN PN PN PN PN PN PN PN PN PN PN PN UGULA 10 NA.SE11,1 (9 named men and)
PN UGULA 10 NA.SE11,1 PN, commander of 10 NA.SE11,1-men.
55 IR,11 IR,11 10 NA.SE11,1 55 subordinates (and) 10 NA.SE11,1-men;
A.AM É.MAH,10 A.AM-personnel of the “first house.”

A.AM.AM A.AM-personnel.

Notes:

a The fifth man listed in each set of five was the “UGULA 5” (“commander of a five-man squad”), in charge of the other four men, who were thus not UGULAS, contra Pettinato (1981:136ff.), who thinks that all those named were UGULAS. In this text are mentioned both “commanders of five” and “commanders of ten” (compare Ug. rb sgrt, “commander of ten,” discussed in Heltzer 1982:152f.). Another Ebla text, TM 75.G.232 (= MEE 1.56), cited in Arcari 1988:127, shows that higher ranking commanders were called “UGULA Ê.DURU,4” (“company commander”).

b What I have called the “first,” “second,” and “third” company, Pettinato (1981:143) calls the “principal quarter,” “second quarter,” and “third quarter” of the city of Ebla. But the terms KA (“gate”) or ir-a-LUM designate such districts, while Ê.DURU,4 denotes a personnel group consisting of (theoretically) 20 persons, as noted above.

c A MAŠKIM was a representative of the king who had broad administrative responsibilities; compare the official called a “sākinu” at Ugarit (written skn in Ugaritic alphabetic texts and MAŠKIM in Akkadian texts).
The remainder of TM 75.G.336 has a similar format and demonstrates the same point, which is that the \textit{NA.SE11}-men were organized in 20-man companies (with some variation from this ideal size), and in 5- or 10-man squads within companies, under commanders (\textit{UGULA}) of various ranks. Attached to these companies of named men were larger numbers of unnamed subordinates (\textit{IR11}), who are thus distinguished from the \textit{NA.SE11}-men. Only the \textit{NA.SE11}-men received rations, a fact which is indicated again at the end of the text by the grand totals recorded for all the companies: 85 \textit{gu-bar} of grain for 170 \textit{NA.SE11}-men. On comparative-historical grounds, it is reasonable to suppose that the \textit{NA.SE11}-men were landholders, heads of households (\textit{E NA.SE11}), who for that reason were obliged to perform military or labor service and were assisted by lower ranking men. According to this interpretation, the term \textit{NA.SE11} corresponds to the later Ugaritic term \textit{bunušu}, with the meaning “landholding royal serviceman” (discussed above in chapter 11.1); and the equivalent term in Akkadian is \textit{avilium} (discussed below in chapter 12.4).

Indeed, the evident military success of the city of Ebla can be explained if such landholders devoted substantial time to developing military skills, entrusting the cultivation of their land to sharecropping tenants. The average number of subordinates (\textit{IR11}) per \textit{NA.SE11}-man in TM 75.G.336 ranges from 2 to 9, and this is a plausible number of servants, clients, or junior kinsmen who might assist their master as part of his military retinue. The fact that many of the landholders listed in this text may have lived in the city of Ebla and were given special duties in the palace establishment creates no difficulties for this interpretation, provided that we do not assume an unproven urban-rural dichotomy. For reasons discussed elsewhere (especially in chapters 6 and 11), it is likely that the majority of householders in the city of Ebla, like their counterparts in rural villages, were farmers who performed part-time service for the king or for a designated royal official as a condition of their landholding, and received supplemental rations during their period of service.

This kind of patrimonial hierarchy—for which there is evidence in the form of extensive royal land grants, as we have seen—would have been far easier to manage than the maintenance of a bureaucratically organized standing army. Given the limits of transportation and communication technology in Early Bronze Age Syria, even a relatively centralized, unitary regime must also have been characterized by a regionalized social and political hierarchy which would have placed practical limits on the absolute power of the monarch, especially if his rule spanned a wide geographical region, as in the case of Ebla, without the benefit of easy riverine navigation. Perhaps that is why the “elders” or “fathers” (written \textit{ABXAŠ} and the king’s “vizier” and his sons were so prominent at Ebla. These were presumably the highest ranking officials within the patrimonial hierarchy of the kingdom. As Archi (1993a:469) puts it, “the ‘house of the king,’ \textit{é en}, was not only the residence of the sovereign but also of his sons, of the other male descendants of the dynasty and of forty or so ‘elders who sit near the throne,’ \textit{abba-abba al-šu-tuš GIs-šudul}, a title which could refer to the most important officials.” Having said this, we must also account of the evidence that these dozens of high-ranking men, who were at least nominally dependent on the king and were regarded as members of his household, also had their own vast households which included large numbers of dependents. Their domains were smaller in scale but similar in structure to that of their overlord, the king.

At this point, it is worth drawing attention to the fact that in Sumerian \textit{é-duru}, means “village, small rural settlement” (see \textit{CAD} E, s.v. \textit{eduru}) and is equated with Akkadian \textit{kapru(m)}. Moreover, there is a similar equation of \textit{é-dur} with Eblaite \textit{ga-ba-ru}, = /kapru(m)/ in lexical texts from Ebla (Conti 1990: 118). Now, Archi (1988b:26) emphasizes that in the Ebla texts \textit{É.DURU}, has no territorial connotation and means instead a “squad of laborers.” But there may be a direct connection between villages and 20-man \textit{É.DURU} units, because a typical village would have had approximately 20 land-holding households (\textit{E NA.SE11}).\textsuperscript{30} If palace-administered “teams of workers” or military companies paradigmatically consisted of hereditary landholders, including occupational specialists, who lived and worked most of the time in their own separate households, then the use of the Sumerogram \textit{É.DURU} (“village”) is explicable. In many cases, such a company would have been inscribed from a single village in order to simplify the problem of recruitment and administration. Note that ARET 2.28 records six lists of named \textit{NA.SE11}-men who are identified by their village, or as dependents of another man (\textit{LÚ PN}), or both.

Furthermore, if \textit{NA.SE11}-men were landholders then their palace-supplied rations would have been only supplemental to the food they grew for themselves, which solves the problem of the small size of the

\textsuperscript{30} The Alalah IV census texts indicate an average of ca. 25 households per village in the kingdom of Mukíš in the fifteenth century B.C. (see chapter 12.7 below; cf. the estimate in chapter 8.4 of ca. 20 households in a small 1-ha village with 100–150 inhabitants).
rations that Milano (1987a; 1987b; 1996:146f.) has repeatedly stressed. Often the cereal ration recorded for an entire Ė.DURU₂ᵏⁱ unit was only 3 or 4 AN.ZAMₗ, per day, which is the normal amount for just one person (Milano 1987b:541; 1990b:210). It could not have fed 20 men, even if Milano is correct in arguing that the Eblaite AN.ZAMₗ was equivalent to a Sumerian šīlₐ (ca. 1 liter). Moreover, only the NA.SE₁₁-men, and not their servants (IR₁₁, IR₁₁) received palace rations, as we have seen. Presumably, a NA.SE₁₁-man on military or corvée duty fed his own servants in accordance with a hierarchical ration system, which is what one would expect if they were his own sharecropping tenants or kinsmen. In any case, the cereal ration was clearly supplemental to the NA.SE₁₁-men’s own diet, not only because of the small size of the ration, but because it did not usually consist of ordinary grain (SE), bread (NINDA), or beer (SE+TIN), but most often simply of malt (ŠE+TITAB) or “GIŠGAL”. TITAB) to be used for brewing beer.

Relative Terms: NA.SE₁₁ as UGULA, IR₁₁, and GURUŞ

If my interpretation of the term NA.SE₁₁ is correct, it increases the significance of another (unpublished) text, TM 75.G.1950, which “opens with a list of 9 ir-a-LUM employed by an equal number of high officials, and in those ir-a-LUM is included a total of 5942 persons (na-se₁₁)” (Archi 1988b:27). This might be a census, not of palace workers, but of heads of hand-holding, service-paying households in the city of Ebla and its suburbs. These 5,942 NA.SE₁₁-men can be compared to the 4,580 Ė NA.SE₁₁ units of Arur and its villages in TM 75.G.10250, discussed above. Certainly this is a reasonable number of households for the 60-ha city of Ebla and its immediate surroundings, allowing for several people per household.

Giovanni Pettinato had originally proposed that the NA.SE₁₁ had a status elevated beyond that of an ordinary worker, and he translates this term as “prefect” (see Pettinato and Matthiae 1976:6ff.; Pettinato 1981:139ff.). This interpretation was rejected by Edzard (1981b = ARET 2, p. 134) and Fronzaroli (1984b:147), followed by Archi and Milano, who all adopt the very general translation “people” on the basis of the Akkadian plural nišī. Despite the evident linguistic connection between NA.SE₁₁ and nišī (cf. Old Akkadian ni-se₁₁), context is more important than etymology in determining meaning, and the contextual evidence that motivated Pettinato’s interpretation (in TM 75.G.336, in particular) has not been adequately addressed by other scholars. This is not to say that “prefect” accurately captures the semantic distinction made in this and other texts, but it is clear that NA.SE₁₁-men were distinguished from servants or dependents (IR₁₁).

It is worth emphasizing that the same man could be called NA.SE₁₁ (head of a household), UGULA (in his capacity as a military commander or supervisor), or simply GURUŞ (“man”). The latter occurs in references to the total complement of men found in large groups of 40, 50, or 80 Ė.DURU₂ᵏⁱ (listed as “GURUŞ, GURUŞ N Ė.DURU₂ᵏⁱ’n”). In TM 75.G.10250, according to Archi’s description, the Ė NA.SE₁₁ groups (what I am calling “households of servicemen”) include various occupational specialists, but also those designated simply GURUŞ, or even IR₁₁; that is, dependents or clients of some other landholder. And the NA.SE₁₁-men registered within larger districts or neighborhoods (KA or ir-a-LUM)—some perhaps organized in Ė.DURU₂ᵏⁱ personnel units—themselves reported to a senior UGULA who was in charge of their district.

This illustrates the general conclusion I have drawn elsewhere in this book that in the Bronze Age such terms do not reflect absolute bureaucratic offices or functional roles but instead express relative social positions. The same man could be a master or overseer with respect to one group of people and a subordinate or dependent with respect to someone else. In a given context there would have been no confusion. Indeed, the flexibility of the terminology is part of the powerful simplicity of a recursive hierarchy in which the same relationships are repeatedly applied to different levels of the social order.

The basic question raised by the preceding discussion is whether the administrative texts create the illusion that Ė.DURU₂ᵏⁱ personnel units worked full-time for the palace in a single location. If we set aside the texts in which Ė.DURU₂ᵏⁱ simply means “20” as a numerical amount, the remaining texts can be understood to refer to corvée teams or military companies staffed by landholders fulfilling their part-time ser-

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31 Note that if Milano is correct, then the Eblaite šīlₐ (also called SÍ.LAS.GU and equal to 6 AN.ZAMₗ) is six times larger than the Mesopotamian and Mari unit of the same name. Moreover, the giₗ-bar (= 120 AN.ZAMₗ), the largest unit of capacity at Ebla, would be 120 liters in size rather than 20 liters, in which case an enormous amount of grain (795,580 giₗ-bar = 95 million liters) is recorded in TM 82.G.266. On the other hand, if the Eblaite šīlₐ was the same as the Mesopotamian šīlₐ, so that the AN.ZAMₗ was only one-sixth of a liter, then the “sowing rates” computed in Milano 1987a (discussed above) are impossibly low, showing that the texts in question do not give actual sowing rates and are useless for estimating absolute field sizes.

32 Perhaps it is only a partial census, however; cf. the 11 ir-a-LUM groups listed in ARET 4.13 §62.

33 Note that the term NA.SE₁₁ is a case of “frozen” syllables functioning logographically in Ebla texts.
vice obligations who received supplemental rations. This calls for the kind of detailed prosopographical study that Steinkeller (1987b; 1996) has undertaken for Ur III Babylonia. Unfortunately, that does not seem to be possible, in view of the highly abbreviated nature of the Ebla texts. In general, it must be admitted that these texts are often ambiguous. It is not easy to determine, for example, whether landholdings were hereditary, or whether there was a distinction in terms of heritability between different types of landholdings. Still, the available evidence suggests that third-millennium Ebla was a strongly patrimonial, albeit quite centralized, kingdom. Based on their own study of the texts Jean-Pierre Grégoire and Johannes Renger (1988:218ff.) describe it as a “patriarchal state” with an “oikos-economy,” with specific reference to Weber’s concept of patrimonialism. Piotr Michalowski (1988) notes the use of kinship terminology at Ebla and suggests that the Ebla kingdom was ruled by a “corporate” dynasty in which the power of the king depended on a network of kin-mediated alliances. Marco Bonechi (1997) likewise is struck by the prevalence in the onomasticon of Ebla and its neighbors of the words *ipumum (“clan”) and *damum (“blood,” i.e., an alliance based on blood). This kin-oriented ideology continues in force into the second millennium, where it is prominent in Amorite culture, in which kin-based social groupings continue to play an important role.

As Steinkeller points out, the political structure of Early Bronze Age Ebla and its Syrian neighbors was thus quite different from contemporary southern Mesopotamian city-states, which seem to have been less intensely patriarchal. Still, Ebla and Ur (for example) were fundamentally similar with respect to their unitary, hierarchical organization, which was based on the pervasive use of the household metaphor as a general template for political and economic action. I have marshaled considerable evidence to support the view that the metaphor of the “house” (ē) at Ebla was not a dead metaphor. We do not have here simply a frozen lexical form that had somehow come to denote a wide variety of different entities, including physical dwellings, landed property, “units of production,” and bureaucratically organized workteams. The ubiquity of this term did not nullify its quite precise meaning of “patrimonially organized socioeconomic unit,” that is, “household.” Such households manifested themselves in many ways at different social ranks and scales, ranging from the vast royal household to the simple households of rural sharecroppers. But they were all organized on the same symbolic basis, in terms of a father or master and his sons, servants, and female dependents. We can therefore question the view of Archi and Milano, among others, that there was a separate sector of “free” land and villages in the Ebla kingdom that was outside the realm of direct ownership by the king, his family, and his high officials, for this dichotomy is a matter of inference and is not directly attested in the texts.

4. The Old Babylonian Empire
Among Old Babylonian specialists, Johannes Renger (1990; 1995) has emphasized the structural similarity between the Ur III regime and the subsequent southern Mesopotamian dynasties that ruled from ca. 2000–1600 B.C. (using the conventional “middle chronology”). Following Gelb, he describes the prevailing mode of socioeconomic organization as a “household or oikos economy,” which persisted into the second millennium relatively unchanged. There were some modifications in the system, however, according to Renger. In particular, land allotments became larger and were increasingly heritable, while the distribution of rations decreased in importance. In the Ur III period, as he puts it, “for the majority of the population their livelihood was guaranteed through their integration into the redistributive system of the patrimonial household of the ruler encompassing the totality of all individual institutional households throughout the realm” (Renger 1990:24).

In the Old Babylonian period, on the other hand, agricultural production became more decentralized and individualized; thus land allotments were large enough to support the recipient and his extended family without recourse to rations. In both cases, however, the existence of an oikos economy meant that there was no real market in basic commodities, most of which circulated instead on the basis of reciprocity and redistribution (see the discussion of “reciprocity,” “redistribution” and “market exchange” with respect to the work of the economic historian Karl Polanyi and his impact on ancient Near Eastern studies in the section on “Substantive Rationality in Ancient Law and Economy” in chapter 4.4).

Another scholar who has been influenced by the oikos model is Norman Yoffee (1977), who describes the Old Babylonian empire under Hammurapi as a “patrimonial bureaucracy,” making explicit reference to Weber’s use of this term. According to Yoffee, the Old Babylonian regime gradually became decentralized under Hammurapi’s successors and increasingly was governed by officials recruited from “extra-

34 Note that the question of “usufruct” (hereditary or not) versus “ownership” is anachronistic and irrelevant here, if, in principle, every landholder held land in usufruct from a higher authority.
patrimonial sources” (i.e., outside of the royal household). By this he means that government officials became more and more independent of the king until the state finally collapsed.

Yoffee’s reconstruction is at odds with the PHM in important respects, however, because he apparently envisions a highly rationalized bureaucratic administration during the Old Babylonian period. Presumably, that is the reason for his adoption of the hybrid type “patrimonial bureaucracy” rather than pure patrimonialism. For example, he speaks of a “bureau of wool accounts” and a “bureau of agricultural affairs” within which royal officials had well-defined responsibilities. But much of the difficulty Yoffee has in explaining the diverse activities of these officials—in particular, those called abi šābim and muʾerrum—is due to the fact that he sees them as salaried bureaucrats employed by the crown and not as patrimonial officials in Weber’s sense (note that the very title abi šābim reflects the official’s role as “father” of the workers or soldiers in his charge). Thus Yoffee describes them as managers of agricultural resources of various ranks, and he struggles to account for their involvement in military, police, and judicial activities (see, e.g., Yoffee 1977:57ff., 68, 75f., 127ff.). Yet the evidence he cites suggests that Old Babylonian officials, whatever their titles, performed multiple administrative functions—simultaneously, and not simply in succession as they were “promoted” from one bureaucratic position to another. This multiplicity of functions is a problem, however, only if we assume that these men filled abstractly defined bureaucratic positions with specified spheres of competence. As Weber (1978:229) noted long ago, “the absence of distinct spheres of competence is evident from a perusal of the list of titles of officials in any of the ancient Oriental states. With rare exceptions, it is impossible to associate with these titles a set of rationally delimited functions which have remained stable over a considerable period.”

The reason for this apparent administrative confusion is that, in a truly patrimonial administration organized on the household model, an official does not exercise authority by virtue of holding an impersonal bureaucratic office with a well-defined set of responsibilities. His authority stems, rather, from his personal relationship to the ruler, and from the structurally similar relationships that he has with lower-ranking persons. Furthermore, it is difficult to distinguish an official’s “public” and “private” activities, because (as I have argued above with respect to third-millennium Mesopotamia) this distinction is misleading in a government that consists simply of a hierarchy of households, each of which pursues its “private” advantage as it discharges its duties within a “public” whole.

In a detailed review article, Dominique Charpin (1980) questioned Yoffee’s treatment of the textual evidence and criticized as anachronistic his attempt to reconstruct bureaucratic “careers” and “promotions” in the Old Babylonian period. In his sharply worded rebuttal, Yoffee (1982) argued vigorously for the use of social models based on cross-cultural analogy. He is certainly correct on this point, but from a Weberian perspective the problem lies in his acceptance of an anachronistic bureaucratic model of administration involving rationally defined spheres of competence and the strict separation of the “public” and “private” concerns of officials. Yoffee does not allow for the possibility that a hierarchy of administrative subhouseholds, each with its own “private” concerns, existed within the overarching “public” royal household throughout the Old Babylonian period. The breakdown of the empire can still be attributed to the increasing decentralization of an originally quite centralized regime, as Yoffee suggests, without supposing that Old Babylonian administration was ever bureaucratic, or that the mode of recruitment of officials or their relationship to the king underwent any fundamental change in principle. To the extent that it was patrimonial, the administration of the Old Babylonian empire was always effected through local elites, or through appointees of the king who quickly became part of local elites. Royal officials therefore tended to be embedded within traditional social networks. The freedom of action of these officials was limited under strong kings, like the active and vigilant Hammurapi, and their freedom increased as the central authority became weaker; but, by definition, they remained part of the royal household for as long as its rule lasted.35

Other monographs on Old Babylonian society have been less ambitious in scope than Yoffee’s, being limited to the analysis of particular archives or to evidence pertaining to a single city (e.g., Harris 1975 on Sippar; Stone 1987 on Nippur; Charpin 1986 and Van De Mieroop 1992 on Ur). This is not the place for a detailed assessment of scholarly work on this topic, but it is worth noting that none of these studies presents a new model of the society and economy of the period, although all of them contain much useful information. Thus, if we reject Yoffee’s version of

35 Note also that Ḥammurapi’s famous “lawcode” is not proof of the existence of an impersonal bureaucratic system of government. The laws of Ḥammurapi exhibit “substantive” rather than “formal” rationality, as I have argued above in chapter 4.4.
such royal dependents were known as private entrepreneurs, as in the Old Babylonian period (as opposed to the Ur III period that preceded it) expressed itself in the willingness of the palace to farm out productive activities to “entrepreneurs” rather than to manage them itself. But as Renger (1979:254; 1984:38, 64) notes, these “private entrepreneurs” were also members of the administrative hierarchy of the state, and their activities were meant to promote the interests of the palace, which were ultimately their own interests, of course. Once again, the “private sector” reveals itself to be an illusion. It is not a separate group of persons outside of the royal household who had property beyond the control of the state; it is simply an awkward description of the activities undertaken by “public” officials for their private gain. In the end, the public-private distinction is irrelevant and misleading, in my opinion. The real distinction is between more centralized and less centralized economies organized on the household model; that is, between economies in which most productive activity was managed directly by the palace, as in the Ur III kingdom, and economies in which royal dependents had greater freedom of action, as in the Old Babylonian empire.

Royal Servicemen and “Free Commoners”

Such royal dependents were known as awilā.36 A well-known problem in Old Babylonian studies is to understand the distinction between the social status of awilum and that of muskēnum (for a recent review see Stol 1997). In the laws of Ḥammurapi these form two of the three basic ranks in Mesopotamian society. A person of the rank of awilum enjoyed special legal privileges and higher social status than a muskēnum. The third and lowest rank was that of wardum, which means “chattel slave” in these laws.

Etymologically, the term muskēnum is a participle of the verb sukēnum meaning “to bow down, prostrate oneself,” and so denotes “one who prostrates himself.”37 The term commonly refers to someone who is poor or destitute (CAD s.v.). This led many earlier scholars to conclude that a muskēnum was a serf or dependent, in contrast to the “free” awilum, whom these scholars saw as a kind of feudal aristocrat. For his part, Igor Diakonoff rejected the feudal model but retained a similar distinction between awilum and muskēnum. In his Marxist two-sector model, an awilum is a free member of the “communal-and-private sector” who owns the means of production and is thus a “citizen in possession of all civil rights,” whereas a muskēnum is a royal servant or laborer and thus a member of the “state sector” who does not have property of his own, except perhaps a temporary land allotment held in usufruct (Diakonoff 1972:46; 1982:28; see the discussion of the two-sector model in chapter 9.2 above).

F. R. Kraus (1973:92–125), however, in his influential treatment of this topic, has pointed out that it is the awilum who was a palace dependent, in contrast to the muskēnum, who was a “private citizen” or “commoner” and thus not liable for royal service (Privatmann, Bürger ohne Amt). This interpretation is supported by Mari texts of the Old Babylonian period, in which a muskēnum is not a destitute serf but rather “[un] particulier n’appartenant pas au monde palatial,” as Jean-Marie Durand puts it (see Durand 1991:21 n.18; also Charpin 1988:19). Durand goes so far as to characterize the muskēnum as “libre habitant des campagnes (et non des villes)” (1988:186 n. 25). This is just the reverse of Diakonoff’s model, for now it is the awilum who is the royal servant in the urban state sector and the muskēnum who is a member of the “free” and politically powerful “private sector” inhabiting the rural countryside.38

But it is not necessary to conclude that the Mari texts display an urban-rural dichotomy which is simply the mirror image of Diakonoff’s two-sector model. The domain of the “palace” (Akk. ekallum; Sum. é-gal, lit. “great house”) should not be equated with the city as opposed to the countryside. As Jean-Robert Kupper (1985:464) points out, the nature of the Mari archives is such that we hear little about people at Mari who were not palace dependents, but there is no reason to assume that the muskēnū of the Mari kingdom lived only in the countryside and not in cities as well. In ARM 2.55:29ff., we hear of muskēnū who complain to a royal official that “the cities of your kinsmen and their fields are exempt, but you have given our fields to the palace.” The physical proximity of independent muskēnū to

36 Although the plural of awilum is often transcribed awilā, this noun has an irregular plural, properly transcribed awilā < *awilā tô (Huehnergard 1997: §20.3).
37 The II-3 verb sukēnum occurs in the rare ŠD stem, thus muskēnum < *muškā-num (Huehnergard 1997: §38.3).
38 The term muskēnum is used in the following Mari texts: ARM 2 55:29; 61:25; 80:10; ARM 3 79:9; ARM 5 25:7; 36:15; 77:10; 81:5; 86:2; ARM 10 89:11; 151:19; 25; 152:6, 10; ARM 14 12:3; 14:20; 39:13, 16; 48:4; 81:39; 83:33; 121:39, 43; ARM 262 287:6; 377:11 (not an exhaustive list).
dependents of the palace is indicated in ARM 2.61:25, and especially in ARM 10.151:18ff., where “field(s) of the palace” (eqel/eqlêl ekallim) are distinguished from neighboring “field(s) of the muškênum” (eqel/eqlêl muškênim). Royal and non-royal farmland alike would in most cases have been situated along the Euphrates River, often near a settlement.

Of particular interest in this regard is a legal text from Mari (ARM 8.85), recently reedited by Charpin (1997:343–47), in which we read that the city of Sapiratum was represented by 37 elders of the Yunhâmmites tribe or “clan” (gâyum).39 This city was located on an island in the middle of the Euphrates, downstream from Mari. Men of the Yabusite clan (recent immigrants to the area?) had laid claim to palace land (eqel ekallim) in the vicinity of Sapiratum, stating that it was their property. In response to this claim, the city of Sapiratum (a-lum sa-plirâtîm³⁵) was called into formal assembly, and Zimri-Lim, king of Mari, decreed that the “city of Sapiratum” should swear an oath before the god to decide the matter, whereupon the 37 named Yunhâmmites leaders (qaqqadât Yunhâmmit, “heads of the Yunhâmmites”) swore an oath to confirm that the land in question was indeed eqel šarrîm, “the king’s field” (note the equation of eqel ekallim and eqel šarrîm). Charpin rightly concludes (p. 347) that most, if not all, of the inhabitants of Sapiratum belonged to the Yunhâmmites clan. These tribesmen were integrated into urban society, while at the same time preserving some degree of local autonomy, including the authority to determine what was royal as opposed to nonroyal land in the area around their city. Although it is not stated explicitly in this text, it is likely that the men of the Yabusite clan who claimed royal land as their own were regarded as muškênu, their own fields would have been called eqel muškênim, as opposed to eqel ekallim. Presumably, there were other tribesmen living in or around Sapiratum who fell into the same category.

Taken together, the Mari evidence and the Babylonian evidence indicate that the basic distinction between avilûm and muškênum is not between urban and rural, or rich and poor. Rather, as Kraus has shown, the defining difference is that a muškênum was not a palace dependent and thus was not obliged to perform royal service on that basis, in contrast to an avilûm. At the same time, we must account for the fact that avilû generally had greater wealth and higher social standing than muškênu, except perhaps

at Mari. We can explain this by noting that in this period a simple but highly effective land-tenure system was in operation in which an obligation to perform royal service (ilkûm) was owed by an avilûm in return for the royal grant of a landholding, often a hereditary landholding granted long before to an ancestor of the current incumbent.40 A muškênum was free of the royal service obligation and was not a palace dependent precisely because he did not possess a royal land grant. In southern Mesopotamia this “freedom” usually meant poverty, for little if any cultivable (i.e., irrigated) land was beyond direct royal control; it was all distributed among palace-dependent grant recipients (avilû) or farmed directly by the palace or by temple households. A muškênum was therefore forced to subsist as a day-laborer, petty artisan, or footsoldier, or as the dependent client or sharecropper of an avilûm.41

In the Mari region, however, seminomadic pastoral tribes could exploit a vast expanse of steppe on either side of the Euphrates that was beyond direct palace control. Because they were outside the royal service system, they were technically muškênu; but their wealth in livestock and their military prowess gave them a measure of independence over against the palace establishment that was not found in Babylonia. They were thus able to win the right to grow crops on farmland that would otherwise have been part of the palace land-grant system. Presumably, there were other nonpastoralist muškênu in the Mari kingdom—also outside the palace land-grant system—who had no alternate sources of subsistence and so were of the typical dependent or impoverished type.42

40 On ilkûm-service see CAD, s.v., and Kienast 1976. Kienast notes that: “Als Dienstherr wird in der ab Zeit nur der König bzw. der Palast genannt, nicht aber auch andere Personen oder Institutionen” (p. 54). The royal service obligation applied to various occupational specialists—“palace dependents” par excellence—including administrative personnel (e.g., scribes, royal trading agents), temple or cultic personnel, military personnel, and craftworkers (ibid.).

41 Evidence that points to the possession of entire villages and their revenue by an Old Babylonian avilûm and his extended family is presented in a recent doctoral dissertation by Christian Dyckhoff (1999:93). The avilûm in question was a royal official named Balamanumhe, who appears in a number of administrative and legal texts. 42 Durand (1988:186 n.25) distinguishes the Mari muškênu from nomadic pastoralists, presumably because muškênu had fields; but comparative ethnographic evidence suggests that seminomadic sheep-owning muškênu would also have grown crops to supplement their diet, using the same arable land along the river for grazing during the dry summer months (for recent examples of this in inland Syria, see Wachholtz 1996). Note the reference to “the sheep of the Hanaeans and muškênu living on the bank of the Euphrates” (ARM 5.81:5).
The translation “commoner, private citizen” for muškēnum is convenient (German Bürger, Privatmann; see Kraus 1973:122f.; Stol 1997; CAD s.v.), provided we do not take it too literally in relation to the European social model it evokes. But it has perhaps too positive a connotation, especially if it is taken to imply the anachronistic concept of a free bourgeois citizen who subsists on manufacturing and trade. As I have argued in chapter 6, there is no evidence for the existence of this sort of independent bourgeoisie in the intensely agrarian and economically underdeveloped Bronze Age Near East. This translation also fails to capture adequately the sense that muškēnu were “outsiders” in a land-based economic system dominated by the palace, which accounts for their low social status and often parlous economic condition. As men who “prostrated themselves” they were not palace-owned slaves or serfs, but neither were they independent yeoman farmers who somehow owned property beyond the royal domain, let alone prosperous bourgeois “citizens.”

It is no surprise, then, that after the Old Babylonian period the word muškēnu came to mean “poor” (cf. Arabic miskīn, Italian meschino). The term avīllum (amīlu) also changed its meaning. It originally denoted a high-ranking person but in Middle Babylonian Akkadian amīlātu was used in reference to chattel slaves who could be bought and sold (Brinkman 1980a:21, 1982:7 n.49). This is not as odd as it seems if avīllum never meant “free man” but rather “palace dependent” or “royal serviceman.” It should be noted, however, that in Late Bronze Age Ugarit the word amīlu, which is equated with Ugaritic bunūšu in polyglot vocabularies, does not mean “chattel slave.” Indeed, in Ugarit the Old Babylonian meaning “palace dependent” or “royal serviceman” fits quite well, as we have seen (cf. Ugaritic bunūšu malkī, “man of the king,” discussed above in chapter 11.1). This is perhaps an indication of the conservatism of the Akkadian scribal dialect used in western Syria in the latter part of the second millennium B.C.

Urban Neighborhoods and Patrimonial Society

Archaeological work has revealed that the courtyard house, in various forms, was quite common in ancient Mesopotamia and Syria (Heinrich 1975:208–20; Margueron 1980b; G. R. H. Wright 1985:133; see figure 19 for an Old Babylonian example). Dwellings of this type were usually joined together in an agglutinative fashion, sharing common walls, which resulted in an organic town plan like that known in later cities in the region (see chapter 6.3 above). The comparison with traditional Middle Eastern towns has been made by a number of archaeologists, notably by Leonard Woolley (1965:175ff.) in his very readable account of his Excavations at Ur (for Woolley’s city plan of Old Babylonian Ur, see figure 19 below). Furthermore, there is Mesopotamian textual evidence (conveniently summarized in Postgate 1992:81ff.) which indicates that urban residential neighborhoods consisted of groups of households organized on a kinship basis, real or fictitious. Modern ethnographers have called such groups “patronymic associations,” in which the solidarity and cooperation that prevail among neighbors is expressed in terms of common descent (Cohen 1965; 1970; Eickelman 1989:155).

During the Old Babylonian period and later the Akkadian term for such an urban clan was bābtum. This word is derived from bābūm (“gate”); indeed, the term bābūm itself is sometimes used instead of bābtum to denote a city district (see CAD B, pp. 9ff., 22ff.). This suggests that each residential neighborhood was originally associated with a particular city gate, or (more likely, in my opinion) that each neighborhood itself was enclosed and had its own gate, which was shut at night and through which only inhabitants of the district were allowed to pass, as in later Islamic cities in the region. In Old Babylonian and later dialects of Akkadian, the term abullū(m) is used for a main city gate, as opposed to the term bābu(m), which could refer to any kind of gate.

In the Old Babylonian period, at least, the bābtum functioned like a village within the larger city, being governed by a “mayor” (rabiānum) and local “elders” (i.e., heads of leading households). The bābtum bore corporate responsibility for the actions of its members and had legal jurisdiction in certain areas. In the laws of Ḥammurāpi (§126), bābūm is a metonym for the local governing body to which an avīllum was responsible. The law in question states that: “If an avīllum’s property was not lost, but he has declared, ‘My property is lost,’ thus deceiving his bābūm, his bābūm shall set forth the facts regarding him in the presence of the god, that his property was not lost, and he shall give to his bābūm double whatever he laid claim to.” In his translation of this law, Meek (ANET, p. 171) noted the similar Hebrew metonym yā-šar, “gate,” where the elders of the clan met in judgment (see esp. Ruth 3:11 and 4:10, discussed above in chapter 8.5). We can also compare the similar use of the term “gate” (KĀ) in third-millennium Ebla texts to denote an urban administrative district of some sort (discussed above)—perhaps a socially integrated, semiautonomous urban neighborhood like the Old Babylonian bābūm.
Figure 19. City plan of Old Babylonian Ur (after Woolley and Mallowan 1976:plate 124) (A typical large courtyard house, bordered by smaller houses, is shaded gray.)
The analogy with the Islamic city suggests that the Old Babylonian *rabītum* was the head of a prominent household of the neighborhood—like the șayh in Ottoman Damascus—and was entrusted with the collection of taxes and the maintenance of order in his district. He also would have served as a patron of lesser households, representing their interests to the higher authority while enforcing the ruler’s edicts at the local level.43

A text from Tell Asmar, ancient Ešnunna, which was published by I. J. Gelb in 1968, is particularly informative about the Old Babylonian *bābtum*. This tablet lists 29 “Amorites” resident in the city, grouping them into five *bābtums* named after the ancestor of the first man listed in each group. Gelb concluded that these Amorites were recently settled nomads who had retained some elements of their original kin-based social organization. Moreover, because it was named after an individual, the *bābtum* here could not refer to a quarter of the city, in Gelb’s opinion, but instead denoted a “small encampment” (Gelb 1968:43). However, it is not stated in the Ešnunna text that the persons listed had only recently adopted urban life, nor is it clear that the term *bābtum* must have a different meaning here than in other Old Babylonian texts. Commenting on Gelb’s interpretation of this text, Yoffee has suggested that:

As an alternative hypothesis to the notion that corporate, familial groups in Mesopotamian cities are only vestiges of a nomadic past, we might consider that the existence of *bābtums* and other such groups in Mesopotamian cities . . . implies a certain normative network of kinship bonds in those settings. Now by kinship we mean not only exclusive biological descent relationships, but also the formulation of political and social alliances among groups and the allocation of special roles and property. [Donbaz and Yoffee 1986:67f.]

Viewed in this way, the *bābtum* is strikingly similar to the sort of residential neighborhood found in traditional Islamic towns and villages; that is, a patronymic association of neighboring households, possibly of quite diverse origins, which were allied together to form the basic spatial and socioeconomic units of the city beyond the level of the individual household. These urban neighborhoods were thus defined socially as well as spatially. Although Postgate, for example, objects to what he takes to be Yoffee’s definition of the *bābtum* as “a group of people, rather than a geographic unity,” arguing instead that “the members of a *bābtum* are held together by their residential proximity” (Postgate 1992:310 n.104), the ethnographic data discussed above suggest that this distinction is not particularly meaningful. It is true that residential proximity is primary, in some sense, because spatial propinquity leads to the formation of social groups whose solidarity is subsequently expressed in terms of (fictive) kinship. But the precise geographical boundaries of the quarter remain somewhat fluid, because the quarter is defined not in terms of absolute space but “pragmatically,” as the geographer Paul Wheatley (1976) puts it, by the existence of personal social ties among its constituent households.

Urban residential quarters are therefore not so much physical entities as dynamic social groupings in which the links between households, as well as the composition of individual households, tend to change over time; moreover, at any one time these links may be perceived differently by different persons.

In light of the analogies from later periods in the Near East it is therefore not odd (contra Gelb) that normal *bābtums* at Ešnunna should have been named after individuals; indeed, this is what one would expect if they were patronymic associations of households rather than spatially defined urban “wards” in the modern Western sense. Furthermore, the organization of the *bābtum* along kinship lines need not imply that it was occupied by recently settled nomads. Middle Eastern ethnography has shown that “tribal” or “clan” organization is as characteristic of long-settled townsfolk as it is of nomads. Finally, the kin-based organization of the *bābtum* tells us nothing about its size, which probably varied from one *bābtum* to the next, being dependent in each case on the changing fortunes and interrelationships of the households of which it was constituted.

The Old Babylonian *bābtum* can therefore be regarded as a “quarter” of the city in the same sense in which patronymic or clan associations of various sizes comprise “quarters” of Islamic towns, although the term “quarter” is misleading insofar as it brings to mind an externally defined and spatially fixed district. In the Ešnunna text, no more than ten Amorite men are listed for any one *bābtum*, which suggests that these residential groups could be quite small—

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43 On Ottoman Damascus, see Establet and Pascual 1994:15: “…la ville est une juxtaposition de quartiers (*hiṣra*) qui peuvent regrouper des familles au sens large, avec leur clientèle, quelquefois d’une même origine géographique ou ethnique. Ces *hiṣra* peuvent occuper un ou plusieurs pâtés de maisons, fermés par une porte gardée la nuit et dont l’accès est réservé aux seuls riverains. À la tête de chaque quartier se trouve un șayh, le représentant auprès des autorités, qui peut régler certains des différends surgissant entre les habitants.” Note also that the names of some quarters in Ottoman Damascus begin with the word “gate,” e.g., “Bāb al-Barid” and “Bāb al-Jābiya” (p. 166).
too small, according to Gelb, to be equated with an entire city quarter. The Amorite bābutum might not have been as small as they appear at first glance, however, for they could have included any number of non-Amorite households which were not listed, as Postgate (1992:81f.) points out. More specifically, the fact that the men listed are described as “free of claims” or “noble” (ellītum) suggests that only the highest ranking persons are mentioned, each of whom probably had retainers and clients with families of their own. Indeed, it seems likely that administrators preferred to keep track of only the leading men in each quarter, leaving the local rabiānum and the elders in charge of the humbler inhabitants of the bābutum, as we know from other texts.

Even if Gelb was correct in regarding the Amorites of the Ešnunna text as recent “tribal” immigrants to the city, this does not imply that the normal Mesopotamian bābutum was not kin-based (and it provides an explanation for the unusually small size, if such it was, of the Amorite bābutums, which in that case had not yet had a long history of growth and development). Hints of the social organization of the typical city quarter are found in the tale of “The Poor Man of Nippur,” in which the hungry hero, having traded his clothes for a goat, is reluctant to slaughter it because he has no beer to go with it in order to make a feast, and so is afraid, as he says, that:

The neighbors (ṣīqū) in my quarter (KĀ = bābu) would hear of it and be angry.
My kinsfolk (kimtu) and relatives by marriage (sallātu) would be furious with me.

[Gurney 1956:150, ll. 19f.]

Clearly, it is expected that meat (a rare treat) ought to be shared with one’s neighbors, many of whom were also one’s kin. This text therefore demonstrates some kind of kin-based coresidence within the quarter, which may in large part have been a result of intermarriage among neighbors. Indeed, a preference for marriage to an inhabitant of one’s own quarter (bābu) is implied in two mid-second millennium Nuzi texts.46 This custom may have been peculiar to the Nuzi area, but the lines quoted above from the “Poor Man of Nippur” suggest that endogamy within a clan-quarter was quite common in ancient Mesopotamia, as one might expect on the basis of later Middle Eastern and Mediterranean ethnographic analogies.

There was no doubt much variation in the precise form of kin-based coresidence that existed in different places and periods in Mesopotamia. Evidence is still scarce concerning the prevalence of joint-family households, for example, as opposed to clusters of related nuclear-family households (on this issue see chapters 7 and 8 above). The large courtyard houses of Old Babylonian Ur (see figure 19) would have been well suited to house joint families of the sort found in similar Near Eastern dwellings of more recent times, but a contrary indication appears in an Old Babylonian account text from the city of Kiš (Ki 1056) which records rations allotted to the members of various households (Donbaz and Yoffee 1986:57–69). Twenty-two households are listed in the portion of the tablet that is preserved, and it appears that all of them consisted of nuclear rather than joint families (although female slaves and their children are listed in many cases together with the head of the household, his wife, and her children). It is worth noting, however, that some of these households were agnatically related—three household heads were sons of the same person—which indicates the existence of kinship ties among neighbors (ibid., p. 66f.).

More detailed information concerning kin-based coresidence in ancient Mesopotamian cities (at least for the Old Babylonian period) is provided by Elizabeth Stone’s (1981; 1987) analysis of residential neighborhoods in Old Babylonian Nippur, in which she employed both archaeological and textual evidence. Stone compares two relatively small areas excavated at Nippur, assigning them to distinct neighborhoods. She prefers the term “neighborhood” to “quarter,” defining “neighborhood” as the minimal social and residential unit of the city beyond the level of the individual household, and reserves the term “quarter” for large districts composed of a number of neighborhoods (Stone 1987:3 n.21). She notes that in preindustrial Islamic cities neighborhoods (ḥārūt)

44 Gelb (1968:43) argues that ellītum here means “deputies,” but this is based on a highly speculative reconstruction of the semantic evolution of the word from the meaning “pure” to “trustworthy,” “entrusted,” and finally “deputized.”

45 For bābu here with the meaning “quarter,” equivalent to bātu, see CAD B, p. 23a. The story of “The Poor Man of Nippur” probably dates to the Old Babylonian period, although it is extant only in first-millennium copies. In any case, this text contains valuable information about ancient Mesopotamian society of a sort not found in other genres.

46 These texts are cited in CAD B, p. 23a: (1) mārti ʾ PN šimmma ana marika ʾu šimmma ina bābu ana aṣṣū ʿidin: “Give my daughter in marriage either to your son or (to somebody else) in (your) city quarter” (RA 23[1926]:151, no. 35:22); and (2) ḫāṣṭ šī u ana aṣṣū ina bābu inṭandinaʾššima: “If he wants, he himself (may take her as a wife) or may give her as a wife (to somebody else) in the city quarter” (HSS 9 145:11).
ranged in size from 500–1,000 persons, and she suggests that this was also the case in ancient Mesopotamia. But she remains undecided about whether the Mesopotamian bābtum was equivalent to a neighborhood or an entire quarter, in her terms, although she appears to favor the former option (ibid., p. 5).

In my opinion, however, there is no need to distinguish “neighborhoods” and “quarters” in this fashion. Clearly, the basic unit in the Islamic city is what Stone calls the “neighborhood,” and insofar as there is a valid analogy between the Islamic city and the ancient Mesopotamian city (an analogy upon which she, like many others, relies heavily), it is reasonable to identify the bābtum with Stone’s “face-to-face community” or kin-based “neighborhood” of some hundreds of persons, which Middle Eastern ethnographers have also called a “patronymic association” or “quarter.” Postgate (1990a:236ff.) voices a similar criticism of Stone’s terminology, arguing that there is no need to posit a level of organization (i.e., the “neighborhood”) between the extended family and the administrative unit called the bābtum (which he equates with a large “quarter”). Postgate emphasizes the structural equivalence between the bābtum and the rural village, inferring that they were approximately equal in size and therefore larger than what he is willing to call a “neighborhood.” In my view, however, the latter term has merit, provided that it is treated as a synonym for “quarter” or bābtum.

Setting aside debates about terminology, it must be said that Stone’s analysis of the textual and archaeological evidence from Nippur highlights important features of urban residential organization in ancient Mesopotamia. In area TA, in particular, tablets found in various houses suggest that for at least 150 years members of the Ninlil-zimu family occupied the largest house and dominated their neighbors, who were their kinsfolk and clients. House K, which was occupied by the head of the Ninlil-zimu lineage, was a large, well-built courtyard house similar to contemporary houses at Ur. It stood apart from the other houses in the vicinity, which were smaller and were built and modified haphazardly in an agglutinative manner, with frequent rearrangements of their floor plans. One of Stone’s most striking findings is the correlation between the property transactions among kinsmen recorded in various texts and the architectural modifications in the houses in which the texts were found (see Stone 1981). Her work clearly demonstrates the existence of kin-based residential patterns in an Old Babylonian urban setting. In particular, she describes a mixture of interrelated joint-family and nuclear-family households similar to that found later in Islamic cities, where joint families were often numerically in the minority although they remained the ideal. Stone summarizes her results as follows:

Virilocal extended family residence was apparently practised when possible, although nuclear family residence was not uncommon. Extended families tended to occupy large, square houses with rooms on all four sides of a courtyard, while nuclear families occupied smaller, linear houses with rooms on two or three sides of a courtyard. On the average each nuclear family lived in three rooms, one of which was a large living room, and occupied an area of around 23 m², or 5.3 m² per person. Extended families were apparently more likely to break up at the death of the father, but coresidence of brothers existed. At times of family transition, the mud-brick structures were easily modified by the addition or subtraction of a wall or door; such activities were only constrained by the owner’s ability to exchange property with his neighbours. [Stone 1981:32]

Certain aspects of Stone’s analysis are questionable, however. Commenting on the long-lived dominance of the Ninlil-zimu family in the neighborhood that is partially represented by excavation area TA, she offers the following explanation for this phenomenon: “It is not easy to understand how such wealth had been accumulated and how such a family was able to take over a section of the city, but what evidence there is suggests that this was a rural lineage which moved into the city at this time, lured at least in part by gifts of property from the king” (Stone 1987:128). Here Stone finds it necessary—despite her use of analogies from traditional Islamic cities, which provide abundant evidence of kin-based urban neighborhoods organized around the household of a wealthy patron—to invoke the movement of a “rural lineage” into the city. This is pure speculation, however. There is no evidence that the Ninlil-zimu family had a rural origin; indeed, it appears that this interpretation rests entirely on the fallacious assumption that groups united by kinship and by personal ties of clientage must be essentially “rural” rather than “urban.” Thus Stone posits a contrast between “kin-oriented” and “institutionally oriented” neighborhoods at Nippur and tries to trace the transformation of the former into the latter, taking it for granted that kinship ties must eventually wither in an urban environment.47 By her own reckoning, however, the

47 This is not the place for a full review of Stone’s book, but it is worth noting that some of her conclusions are excessively speculative (see the detailed review in van Driel 1990; also Charpin 1989–90 and Postgate 1990a). In particular, her attempt to contrast area TA, occupied by a kin-oriented lineage, with the nearby area TB, which was occupied (in her view) by institutionally oriented temple-offi-
Ninlil-zimu family retained its “rural” kin-based orientation for almost two centuries, which indicates that there was no fundamental conflict between kin-based social organization and urban existence. In the absence of clear evidence for the rural origins of urban lineages, it is erroneous to assume that such groups consisted of immigrants who “attempted to maintain village ways in an urban setting,” as Stone (1987:129) asserts. On the contrary, the evidence suggests that “urban ways” in this period themselves included kinship and clientage; or, to put it another way, the typical Mesopotamian city was always an agglomeration of urban “villages.” However one wishes to express it, the fact remains that Stone’s work actually indicates the absence of a strict urban-rural dichotomy in Old Babylonian Nippur, at least with respect to the social organization of households and neighborhoods.

Aside from Nippur, there are archaeological hints that kin-based urban neighborhoods existed in various periods in other ancient Mesopotamian cities. In the earliest Early Dynastic phase at Abu Salabikh, for example, excavators found walled residential quarters that may have housed separate kin groups (Postgate et al. 1983; 1990; see also Postgate 1992:91f.). A thousand years later, the extensively excavated residential areas of Old Babylonian Ur were characterized by the narrow streets and blind alleys typical of urban neighborhoods in later Islamic cities (see figure 19 above), and there are complementary indications in the texts from Ur that extended families resided together or in neighboring dwellings, as appears to have been the case in the same period in Nippur (see Woolley and Mallowan 1976:15f. and plate 124; Van De Mieroop 1992:213–20 provides a useful survey of the evidence).

Obviously, more research is needed on the organization of urban life in these and other cities of ancient Mesopotamia. In particular, close social ties among neighbors might be demonstrated archaeologically in the future through the careful study of the use of shared facilities such as courtyards, ovens, and (most importantly) intramural tombs by the occupants of adjoining houses. Where possible, prosopographical analysis of written documents in relation to their spatial contexts, as Stone has done for Nippur, provides complementary evidence concerning urban social organization. A good example of this sort of analysis is found in Charpin’s (1986) study of the temple personnel of Old Babylonian Ur, in which he used prosopography to reconstruct their activities and family trees. Charpin confines himself to presenting the data and to drawing quite specific and limited conclusions, avoiding any attempt to provide a general picture of Old Babylonian society. But his observation that scribal “schools” in Old Babylonian Ur were not necessarily public institutions but were family-based, so that knowledge was passed from fathers to sons within private households (ibid., p. 485f.), is entirely in keeping with the view that kinship was important in Old Babylonian urban society—even in “official” circles.

A word of caution is in order here, however. The foregoing discussion has largely been restricted to Old Babylonian evidence; in other periods patterns of residence in Mesopotamian cities might have been quite different. During the preceding Ur III period, for example, joint-family households and other forms of kin-based coresidence may have been less common, reflecting a more complex and highly differentiated system of political and economic administration in which the central government assumed greater responsibility for legal and economic matters that in other periods were left to semiautonomous kin-based associations like the babārum.48 There is evidence even in the Ur III texts, however, that occupations were hereditary and that most productive activities were organized on the basis of domestic economic cooperation among the members of extended families, as is shown by Steinkeller’s prosopographical study of the “foresters of Umma” (Steinkeller 1987b, discussed above on p. 263), and by his subsequent study of the pottery industry of third-millennium Babylonia, in which he demonstrates that the occupation of potter (and most other crafts) was hereditary.

48 Steinkeller (1989), for example, discusses a text recording a survey of several neighboring houses in Ur III Umma (YOS 4 300) that varied widely in floor area (ca. 35–250 m²). The owners of these houses had very diverse occupations and socioeconomic standings: “Thus we find living next to each other a fisherman, a cowherd, a shepherd, a carpenter, a priest, a high military commander, and possibly the son of a majordomo” (p. 5). These occupational and social distinctions indicate to Steinkeller that “the houses in question were occupied by nuclear families” rather than “extended family groupings” (p. 6). It is possible, however, that the lower-ranking persons living in the smaller houses were patrimonial clients (if not kinsmen) of the “general of Umma,” who owned the largest house. At the very least, the “general” himself might have headed an “extended” or joint-family household, in view of the floor-space available (250 m²). A joint-family structure is usually possible only for the wealthiest households, even when it is the ideal.
and was normally a family enterprise conducted in private, home-based workshops (Steinkeller 1996). As for the later periods, I have not discussed urban life in first-millennium Mesopotamia at all, restricting myself here as elsewhere in this book to third- and second-millennium evidence relevant to the Late Bronze Age kingdom of Ugarit.

In general, it must be admitted that the existing Mesopotamian evidence for the structure of urban households, both archaeological and textual, is still only suggestive rather than conclusive, although the various hints of kin-based co-residence (or, at the very least, kin-based economic cooperation) point to a generally patrimonial rather than bureaucratic social order. Furthermore, when discussing ancient Mesopotamian urbanism it is necessary to allow for broad temporal and geographical differences in sociopolitical organization, such as the distinction proposed by Steinkeller (1993; 1999) between the theocratic city-state system of third-millennium Sumer and the more secular and authoritarian “Semitic” regimes that emerged in northern Babylonia, Upper Mesopotamia, and Syria. According to Steinkeller, the southern system was more decentralized and egalitarian, consisting of “largely self-sufficient temple-households which controlled most of the resources of the city-states, including nearly all of their agricultural lands” (ibid., p. 122). The northern system, on the other hand, was more stratified socially and was dominated by powerful royal and private households, which were characteristic of both the third-millennium kingdoms based in Kiš, Ebla, and Akkad, and of the second-millennium polities attested later in Mesopotamia and Syria, including the kingdom of Ugarit. But profound and long-lasting though these broad regional differences may have been, Steinkeller’s “southern” and “northern” systems can both be understood as variants of the patrimonial household model.

5. Kassite Babylonia

A Kassite dynasty took over Babylon in the wake of the Hittite raid on that city in 1595 B.C. The Kassite rulers of Babylonia, from a non-Semitic ethnic group of uncertain origin, were soon assimilated to Babylonian culture. This dynasty reigned a long time, until 1155 B.C., and succeeded in unifying Babylonia as a cohesive national kingdom (for useful overviews of the Kassite period see Brinkman 1980b and Sommerfeld 1995b). Many of the political and economic traditions of Mesopotamia were maintained, but “one of the most characteristic phenomena of the Kassite era is the king awarding favored subjects with extensive parcels of land, a practice that had not previously existed and that has often been compared with feudalism” (Sommerfeld 1995b:920).

The main evidence for these royal grants are the so-called kudurru (“boundary marker”) texts, more accurately called narû (“stela”) texts, inscribed on stone stele that were displayed in temples (see Brinkman 1983; Slanski 1997:24–38). Land grants, like other legal transactions, were presumably registered formally in sealed clay tablets, now lost. The kudurru was thus “a documentary monument intended to strengthen or confirm the efficacy of the legal action” (Brinkman 1983:270). Only about sixty kudurru of the Kassite period have been found to date, and of these, many are badly damaged or were unfinished in antiquity; thus the sample of data concerning Kassite royal land grants is quite small (see the convenient tabular summary of the 21 legible kudurru in Sommerfeld 1995a:472f.).

The practice of recording large royal land grants to elite individuals was apparently new in southern Mesopotamia (or is simply better attested), although this practice was well known already in Syria in the third millennium B.C., as the Ebla texts demonstrate. In Kassite Babylonia the recipients were, for the most part, members of the royal family, temple officials, high-ranking military officers, and other high officials and courtiers. Moreover, the grants were hereditary, “for all time” (ana ūm šāti; see Slanski 1997:108ff.). This fits the pattern known from texts found in Early Bronze Age Ebla, Middle Bronze Age Alalah, and Late Bronze Age Ugarit—the latter in the period roughly contemporary with the Kassite dynasty.

In all of these cases, however, it is not a matter of “feudal” political decentralization, in which the king was forced to delegate large portions of his realm to aristocratic landlords, but just the opposite. It is because the palace was strong and effective in its control and administration of the royal domain in the Syrian and Kassite regimes that we find formal

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49 Here, as elsewhere in this book, I have adopted the conventional “middle chronology.” This is done for the sake of convenience with no intention of defending this chronological reconstruction over against any other. An argument has recently been made for dating the Hittite raid on Babylon considerably later, perhaps as late as 1499 B.C. (see Gasche et al. 1998).

50 Earlier scholars emphasized the analogy between Kassite political and economic innovations and European feudalism (e.g., Balkan 1986 [1943]). Subsequently, however, Kassite specialists such as John Brinkman (1974; 1980b; 1983) and Walter Sommerfeld (1995a; 1995b) have avoided feudal terminology, although they acknowledge certain feudal characteristics in this period.
documentation of large-scale land grants. John Brinkman (1983:274) has pointed out that the Kassite royal land grants (or royal ratifications of earlier land grants) reflect a period of centralized rule, especially after 1400 B.C., when the king in Babylon was able to assert his control over his domain. That is why the average size of the land parcels granted by the king decreased greatly during the period of political decentralization that followed the breakdown of Kassite power in the twelfth century B.C. The Kassite regime may thus have been typical of what Steinkeller has called the “northern” or “Syrian” political system (discussed above), in contrast to the more decentralized and egalitarian “southern” system, in which large temple-households were dominant. Such a conclusion remains to be confirmed, however, by additional data and analysis that would provide a clearer picture of Kassite administration.

Kassite Royal Land Grants and Related Villages
In the Kassite period, the attested royal land grants ranged from 10 to 494 kurru in size, that is, ca. 80–4,000 ha, where one kurru is ca. 8.1 ha. An amount at the low end of this range (30, 50, or 70 kurru = 243, 405, or 567 ha) was most common, however. In the Ebla kingdom a typical land grant to a high official—representing the farmland of one village, in my view—was on the order of 350–1,100 ha (i.e., 1,000–3,000 GANA.KI, assuming that this land measure is more or less equivalent to the Sumerian iku of 0.36 ha, as I have argued above in chapter 12.3). But we must allow for the difference between irrigated farmland in Babylonia and the less productive cropland of dry-farming Syria. In southern Mesopotamia 240 ha of cultivable land was sufficient to support at least 200 people, and perhaps twice that number, while somewhat more land would have been needed to support the same population in the Ebla kingdom. The greater productivity of irrigation agriculture is reflected in the fact that Babylonian settlements were larger on average than settlements in Bronze Age Syria, although it is worth noting the apparent decline in both gross settled area and average settlement size in the Kassite and post-Kassite periods, summarized by Brinkman (1984) on the basis of archaeological surveys. Allowing for these differences, it is striking that in both Kassite Babylonia and the Ebla kingdom typical land grant amounts corresponded to the cultivated area needed to support typical rural settlements. Some high-ranking members of the Ebla palace establishment (sons of the “vizier” Ibrium) received multiple village grants totaling thousands of hectares, and the same is true in Kassite Babylonia, especially in the case of grants to members of the royal family and to temples.

The correspondence between the size of land grants and village settlements is demonstrated in the grant made by Nazi-Maruttaš (who reigned 1307–1282 B.C.) to the temple of Marduk and to a man named Kašakti-Šugab. This grant included arable land associated with several settlements. The land areas recorded for these settlements are just what we would expect in terms of the sustaining area required for village communities. The beginning of this kurru text is translated as follows:

Nazi-Maruttaš, king of the world, son of Kurigalzu, descendant of Burna-Buriša, king without equal, has given land opposite the city of Babylon to Marduk his master. He has given to Marduk the town of M-šuqrnī, which had belonged to the House of Muktarissāt, together with four villages and its (i.e., M-šuqrnī’s) 700 kurru (ca. 5,670 ha) of land, for which the mayors compensated the House of Muktarissāt (by giving it the following land):

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51 In kurru texts the land measurement is typically given in kurru (GUR) of seed-grain, assuming a sowing rate of 3 sūtu (BAN) of seed per ikā “measured by the big cubit” (see, e.g., the kurruus published in Arnaud 1972). In the Kassite measurement system, 3 sūtu of seed = 1 “yoke” of field = 1 ikā “measured by the big cubit” (Powell 1989:482). This is the distance covered by an ox team in one day, i.e., the amount of land/seed planted in one day’s work with a seeder plow. The Kassite ikā measured by the “big” (= 75-cm) cubit is equal to 2.25 Sumerian-Old Babylonian ikā measured by the original (= 50-cm) cubit, i.e., ca. 0.81 ha (the Sumerian-Old Babylonian ikā = 14,400 square cubits ≈ 0.36 ha). In the Akkad-Old Babylonian system of capacity measures, 1 kurru = 30 sūtu = 300 qū (SIJA), and this remained the standard in most cases in the Kassite system, especially where surface area was measured in terms of seed (ibid., p. 498). If 3 sūtu = 1 ikā then 1 kurru = 30 sūtu = 10 ikā = 8.1 ha.

52 Adams (1981:86f.) reviews ethnographic and documentary evidence and proposes a fairly conservative estimate of ca. 1 ha of cultivable land per person, allowing for fallow land, human dietary requirements, loss of grain in storage, and the use of some grain to feed plow oxen and other farm animals. Others adopt estimates as low as 0.5 ha per person. Wilkinson’s (1994:495) model for Upper Mesopotamian Bronze Age settlements (without irrigation) assumes 1 ha per person with biennial fallowing.

53 Grants of only 10 kurru (81 ha) are rare, and may pertain to parcels of land within the arable area of a larger town or city; e.g., Sommerfeld’s (1995a:472) kurru no. 7 (= King 1912:7ff. [no. 3]) refers to 10 kurru “which is in the midst of (ṣa ina qereb) the arable land of the irrigation district of GN,” and his no. 20 (= Scheil 1900:95) refers to 10 kurru near the city of Padan.
70 kurru (567 ha): irrigation district (uçaru) of the village Risnu on the bank of the canal Suru-rabû.
30 kurru (243 ha): irrigation district of the village Tirigû on the bank of the canal Tavan.
Total: 100 kurru (810 ha), at 3 sütu per ikû measured by the big cubit, in the province (pihâtu) of the House of Sin-mâgir.
70 kurru (567 ha): irrigation district of the village Alû-Sa-Sâ'idî on the bank of the canal Tavan.
30 kurru (243 ha): irrigation district of the village Dûr-Sarru on the bank of the canal Tavan.
Total: 100 kurru (810 ha), at 3 sütu per ikû measured by the big cubit, in the province (pihâtu) of (the city) Dûr-Papsukkal.
60 kurru (486 ha): irrigation district of the village Tallari on the bank of the Royal Canal in the province of Hudadu.
100 kurru (810 ha), at 3 sütu per ikû measured by the big cubit: irrigation district of the village Dûr-Nergal on the bank of the canal Migatu in the province of Tupliyaš.
50 kurru (405 ha), at 3 sütu per ikû measured by the big cubit: irrigation district of the village Dûr-Samaš-ilu-bâni on the bank of the canal Sumundar in the province of the House of Sin-âšarêd.
84 kurru (680 ha), at 3 sütu per ikû measured by the big cubit: irrigation district of the village Kârû on the bank of the Royal Canal in the province of (the city) Upî.

For the 700 kurru (5,670 ha) belonging to the town of Mûr-ûqnû, (the House of Muktarîsû was given?)
494 kurru (4,001 ha). The remaining 206 kurru (1,669 ha), Nazi-Maruttaš, king of the world, son of Kurigalzu, king of Babylon, has given to Kašakti-Šugab son of Ahû-bâni, his servant.


The eight settlements listed by name, totaling 494 kurru (4,001 ha) of land, were located in several different provincial districts. The most plausible interpretation of this text is that 494 kurru of land belonging to these eight settlements was given to the House of Muktarîsû in compensation for the loss of the town of Mûr-ûqnû near Babylon, with its four dependent settlements and 700 kurru of land.54 The remaining 206 kurru (1,669 ha), making up the total of 700 kurru given in compensation for the transfer of 700 kurru from the House of Muktarîsû to Marduk, was granted to a person with the Kassite name Kašakti-Šugab, whose relationship to the House of Muktarîsû is not specified.

It seems that the attribution of village land to “Houses” was quite complex and cut across territorial boundaries, perhaps being based on personal ties of authority rather than corresponding to our notion of neatly defined provinces (the use of the term “House of So-and-so,” Akk. bit PN, in the Kassite period is discussed further below). The House of Muktarîsû received land in the provincial districts of the House of Sin-mâgir and the House of Sin-âšarêd, as well as in the provinces of the cities Dûr-Papsukkal and Upî, the province of Hudadu, and the province of Tupliyaš. The House of Muktarîsû is not called a “province” (pihâtu) and was perhaps more like a “tribe,” as Nashef suggests (1982b:125). By removing from the House of Muktarîsû a large territory close to Babylon, consisting of a town, four satellite villages, and 5,670 ha of farmland, and by replacing it with several smaller landholdings in widely scattered provinces, the king was able to dissipate the potential political power of this House.55 Note that the land and settlements near Babylon were not transferred to another House but to the temple of Marduk, which was presumably easier for the king to control.

Setting aside questions of political geography and related political strategies, it is likely that Kassite land grants on the order of 30–100 kurru (ca. 240–810 ha) per village were intended to support more than just “a sizable extended family, including service personnel, slaves, and so forth,” as Walter Sommerfeld has it (1995b:922; 1995a:476). Unless we picture these estates as slave-worked latifundia that were physically and socially detached from ordinary settlements, we must conclude that the people who worked the land were organized in households of their own and resided in nearby settlements. It is probable that the inhabitants of the granted villages remained in place and it was their surplus production which was transferred to new owners.

Sommerfeld disputes the notion that the normal practice was to grant an entire village, pointing to the fact that kudurrû often do not explicitly name village settlements. But the failure to mention a village name does not prove that local sharecropping farmers were not included in the royal grants. For one thing, that, despite the case ending, bažármutû is here taken to be the plural subject of the verb niarrû.55 I owe this insight to my colleague McGuire Gibson.
without a labor force the granted land would not have been of much value. Moreover, we should not expect that the cultivators of such land were necessarily “villagers,” for in some texts it is clear that the land being granted was beside a walled city, and those who cultivated it would have been inhabitants of that city. For example, in a grant given by Nazi-Maruttaš to Puzru, a “chief singer” (nargallu) in the Ebabbar temple, were 30 kurru (243 ha) of land that was the “irrigation district of ‘the narrow reed marsh at the gate of the wall,’” on the canal Nā ru-ša-ša-ša-šu-nūrī” (line 9 in Arnaud 1972:164). Another estate that was granted to Puzru in the same text was “beside(?)” the wall of the city of Larsa, next to the palace orchard (line 22 in Arnaud 1972:164; see also Slanski 1997:75).

Furthermore, the names of villages are given in quite a few instances—and not just in the case of large multiple-village grants to temples or members of the royal family. For example, in a kudurrû of Kudur-Enil (1254–1246 B.C.) that records a royal grant to a man named Mušallim-Ekur⁵⁶ are listed the following nine estates:

1. 70 kurru (567 ha): irrigation district of the village Šašūr on the bank of the canal Hîlû-ta-bitu in the province of the House of Šin-mâgir.
2. 50 kurru (405 ha): irrigation district of the village Alū-ša-Dunga on the bank of the canal Namkâr-Šamaš in the province of the Sealand.
3. 50 kurru (405 ha): irrigation district of the village Alū-ša-ikkû on the bank of the canal of Alû-ša-ikkû, [district of] Ilâa-Šamaš in the province of the Sealand.
4. 30 kurru (243 ha): irrigation district of the village Râpî on the bank of the Tigris River in the province of the Sealand.
6. 30 kurru (243 ha): irrigation district of the village Dûr-Galzu on the bank of the canal Tabbîltu in the province of the House of Šin-mâgir.
7. [x] kurru: irrigation district of the village [X] on the bank of the canal Saḫartu, beside Tarbašu, in the province of Ru[x].
8. [x +4 kurru: irrigation district of the village [X] on the bank of the canal Namkâr-ilâni in the province of the Sealand.
9. 2 kurru (16.2 ha) at the great gate of the city called “Their Judge.”

[Published in Arnaud 1972:169ff.]

Yet another example is the grant made by Meli-šîpak (1186–1172 B.C.), towards the end of the Kassite dynasty, to Hasardu son of Šumē, a high royal official (sukkal mu’reṣirî). The grant consisted of “50 kurru (405 ha), irrigation district of the village Šalu-luni on the bank of the Royal Canal in the province of the House of Piri’a-Amurru” (published in King 1912:19ff.).

From these texts it seems quite clear that Kassite royal land grants were usually grants of one or more entire villages, or landholdings of equivalent size worked by urban residents. In practical terms, the recipient of a given piece of property 30–100 kurru (ca. 240–810 ha) in size would have received the revenue from land cultivated by the inhabitants of an entire small settlement, or cultivated by a group of similar size living within a larger town or city, perhaps in the particular urban neighborhood closest to the land in question.⁵⁷

The geographical scale of such land grants can be pictured by recalling that 240 ha is the area of a circle with a diameter of 1.75 km, while 810 ha is the area of a circle with a diameter of 3.21 km. In recent times, the cultivated territory of preindustrial villages in southern Iraq was of similar dimensions.⁵⁸ And the actual territory belonging to villages in Kassite Babylonia would have been somewhat larger than the land areas recorded in the kudurrû texts, which do not include the space occupied by roads, canals, dwellings, and uncultivable wasteland. Moreover, the farmland was not necessarily arranged in a compact circle around the settlement, but may often have been strung out along linear canals. A major constraint on

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⁵⁶ This man is not identified by profession but was probably an official of the Ebabbar temple (Arnaud 1972:164).

⁵⁷ As McGuire Gibson has suggested to me, some of the granted settlements may have been unoccupied and only recently made cultivable by a royally sponsored extension or renovation of the canal system. The recipient of the grant would then have been responsible to rebuild and populate the village, in return for which he was entitled to the revenue from that village after the newly settled villagers had dug feeder canals and brought the land under cultivation.

⁵⁸ Poyck (1962:57, table 4.12) reports that the amount of land (including fallow land and wasteland) cultivated by tenant farmers in the 1950s in the Hilla-Diwaniya area of Iraq, before the introduction of mechanized agriculture, was ca. 4–16 ha per family (15–65 mešaras, where 1 mešara ≡ 0.25 ha), with a median size of ca. 12 ha per family. Twenty families would therefore have cultivated ca. 240 ha. The land area farmed per family might have been considerably smaller in antiquity, for these Iraqi tenant farmers had to give half of their crop to the landowner (p. 55), and half the land was left fallow each winter. See also Postgate 1990b on an agronomic study done in the late 1950s on a small village of 13 families (in 1955) located on the middle Tigris, whose land measured ca. 1.8 × 1.8 km, totaling 1,068 mešaras (ca. 267 ha).
the size of an agrarian settlement’s territory is the distance that must be traveled (with slow-moving plow oxen) to the farthest fields, and the time thereby required for a daily roundtrip. For this reason, the distance traveled to the outermost fields belonging to secondary village settlements in the Bronze Age Near East did not usually exceed 1–2 km, as I have noted above (Wilkinson 1989; 1994).

In any case, there is not enough evidence available to allow us to conclude that “normally, the donation of land did not confer authority over the local populace,” as Sommerfeld has done (1995b:922; 1995a: 477). This is part of his general contention that Kassite royal land grants, although a striking phenomenon, were relatively small in scale and were exceptional occurrences, and thus were not a dominant economic factor in Babylonian society. He notes that much larger land areas were granted to temples than to individual persons, implying that temple households must therefore have remained dominant. But we have so few Kassite land-grant texts to work with that it is impossible to say how common individual grants were or how much of the kingdom they encompassed.

Moreover, one can reject the feudal model, as Sommerfeld wishes to do, without having to minimize the importance of royal land grants. In opposition to the feudal model, Sommerfeld stresses the fact that the king respected long-established property rights as much as possible. In other words, the king would not simply expropriate land, as a rule, but would compensate the landholder for property he wished to take. But this prudent practice—presumably undertaken by the king to avoid alienating his subjects—does not nullify the notion, fundamental to the royal ideology of the time, that the king had rights over all of the land, so that in theory, at least, all landholdings were royal grants or subject to royal ratification (on the concept of royal “ownership” of all the land in relation to a customary hierarchy of property rights, see chapter 9.3 above). Indeed, the large grants to temples recorded in a few kudurrus, which are formally identical to the smaller grants made to individuals, indicate not so much the independence or power of the temples as the claim of the king to dispose of all the land, even vast temple domains—land which, in a striking reversal of earlier conceptions, the king gave to the god (see the grant made by Nazi-Maruttaš to “Marduk his master,” translated above).

In this regard, it is significant that in a post-Kassite kuduru text from the reign of Marduk-nadin-abī (1099–1082 B.C.), a text that records the purchase of 5 kurru (40.5 ha) of arable land from the House of Hanbu by a royal official named Marduk-naṣar, we read that thereafter no official of the House of Hanbu would be permitted to claim the land by any means, including the device of saying “these fields are not a gift of the king” as a way of getting them back from the purchaser (King 1912:37ff. [no. 7:ii 7]). As Piotr Steinkeller (whom I thank for drawing this text to my attention) has put it: “The implication is that one became a legitimate ‘owner’ of arable land, and acquired the right to ‘alienate’ it, only through royal grants. In other words, unless a field had been granted to an individual by the crown, he could not rightfully ‘own’ it” (Steinkeller, personal communication). Presumably, despite its post-Kassite date, this kuduru text reflects the conception of landownership that was current in the immediately preceding Kassite period, when the Babylonian king’s power and prerogatives were, if anything, greater than they were under the succeeding dynasty.

There is some evidence, then, that the Kassite kings of Babylonia were patrimonial rulers who headed a quite centralized political regime. The royal domain was the property of a powerful king, who bestowed land on favored individuals and temples alike. We have no way of knowing what proportion of the arable land in Kassite Babylonia belonged to temples, as opposed to being granted to individuals or directly administered by the palace. But the kudurrus demonstrate that the royal land-grant system encompassed both individual and institutional recipients.

“Houses” and Provinces

Another patrimonial feature of the Kassite kingdom is evident in the use of the term “House of So-and-so” (Akk. bit PN) to designate sociopolitical groups of various sizes, including entire Babylonian provinces, as we have seen in the kuduru texts cited above. According to Brinkman:

Each of these Houses was named after an eponymous ancestor (sometimes fictitious). Individuals belonging to these Houses were usually referred to as “sons” of the eponymous ancestor, no matter how many generations they were removed from the founder of their House. A House was composed of persons related in the male line, and not infrequently such a group would involve one or more villages and a sizeable amount of agricultural land possessed in common. Each House, in so far as can be determined at present, was subject to a single man termed the “Lord of the House” (Babylonian bel bitti), who served as head of the group and could summon the senior males into executive assembly. [Brinkman 1980:465]

This particular use of household terminology was perhaps introduced into Babylonia from native
Kassite tribal society, although it was by no means alien to the similar “Amorite” political conception attested centuries earlier among Semitic-speaking residents of Mesopotamia and Syria, especially in the Mari letters. Regardless of their origin, these Houses seem to have functioned quite well in Kassite Babylonia over a long period as political units within a strongly centralized system of government, although the relationship of such Houses to the central government and to one another is not well understood.

The phrase “province of the House of PN” (piḫat bit PN) occurs frequently in the kudurru texts in order to specify the location of settlements, and these designations are parallel in form to the political-geographical designation “province of the Sealand,” used in similar contexts, which obviously denotes a large territory (cf. also “province of the city Dūr-Papsukkal” and “province of the city Upī”; see the texts cited above and the catalogue of “bit PN” names in Nashef 1982a:52–74). On the other hand, some Houses were not territorial provinces but sociopolitical groups that owned settlements scattered among several different provinces (e.g., the House of Muktarissah, discussed above). Perhaps prominent Houses as social groups gave their names to certain geographical regions in which they had extensive holdings and these names survived as territorial designations long after the decline of the House. Or perhaps there was a hierarchy of Houses, one within another (on this possibility see Brinkman 1968:255). Or there may have been a complex network of property relations in which the landholdings granted to a House cut across regions in way that was calculated to minimize its regional power base. Or, indeed, all three of these phenomena may have occurred together.

Cross-cutting landholdings would account for the fact, noted by Brinkman (1980b:465ff.), that during the period of the Kassite dynasty there was a distinction between the political role of a “master of a House” (bēl bitti) and a “provincial governor” (sakin mātī) or lesser provincial official (bēl piḫati). And even if Houses were normally localized in particular geographical areas, the distinction between bēl bitti and sakin mātī is explicable if we assume that in periods of political centralization a strong patrimonial ruler would appoint his own inspector or administrator in each region—a member of his staff who was not a local patrimonial ruler with his own ambitions—in order to ensure that royal policies and edicts were carried out. In a weaker or more decentralized patrimonial regime, however, the apical ruler is forced to work through local patrimonial rulers, who enjoy greater freedom of action. This does not mean that local Houses were politically unimportant in the period of the Kassite dynasty, but their masters’ freedom of action would have been curtailed by strong kings, who could even transfer property from one House to another. After the fall of the Kassite dynasty it seems that the distinction between “master of a House” and “provincial governor” was lost, at least in some cases, perhaps reflecting a situation in which the Babylonian king’s ability to intervene in the internal administration of certain provinces had declined.

6. The Middle Assyrian Kingdom

In the fourteenth century B.C. the city of Aššur on the upper Tigris River reasserted its independence from the waning Mittannian empire and began to carve out an empire in northern Mesopotamia. Various synthetic treatments by Middle Assyrian specialists indicate that in this period the kingdom of Assyria was very much a patrimonial regime, although most scholars do not use that term. This does not necessarily mean that the Middle Assyrian regime was decentralized, such that great freedom of action was enjoyed by local administrators in comparison to earlier quite centralized polities based in southern Mesopotamia. As I have already noted, the degree of effective control exercised by the central political authority had little structural significance in Bronze Age kingdoms in which political power was implemented and legitimated in a patrimonial manner. In such circumstances, central control varies unpredictably in relation to the political and military skill (and sheer luck) of particular rulers. It is worth making the point again here that patrimonialism as a mode of legitimating and structuring political authority does not necessarily imply weakness or lack of detailed oversight on the part of the king and his immediate household staff or “palace” (ekallitu).

59 The title bēl bitti is directly attested only in the post-Kassite period and is unattested in the Kassite period proper, but there is every reason to suppose that under the Kassite dynasty the heads of Houses would have used the same title.

60 John Brinkman and his students Shelley Luppert-Barnard and Daniel Nevez were kind enough to read and comment on an earlier draft both of this section and the preceding section on Kassite Babylonia. Of course, they do not necessarily endorse my interpretations, and any errors that remain are my own. Note that here, as elsewhere in this chapter, I am not presuming to provide my own sociohistorical synthesis de novo, but rather I am reacting to syntheses and related evidence supplied by leading scholars of the subject under discussion.
Setting that question aside, the evidence for Middle Assyrian patrimonialism is clear. Paul Garelli (1967:18–20) cites textual evidence which suggests that Middle Assyrian provincial governors (in the twelfth century, at least) collected revenue from their domain as if it were their own property and then used some of it to make payments to the king. As he puts it: “le gouverneur se conduirait comme un fermier général: il verserait à l’État les redevances prévues, et utiliserait le surplus à son profit personnel” (ibid., p. 19f.). In other words, little distinction was made between the “public” and “private” affairs of royal officials.

Nicholas Postgate has made the same observation, although he asserts, nonetheless, that “the distinction between the public and the private sector did exist” (Postgate 1986:27; also 1988a:xxiii–xxv). Following Koschaker, he argues that public administrative documents were formally distinguished by the absence of witnesses, which were necessary in private legal texts, and by the use of the phrase ša qāt as opposed to simple ša (see Koschaker 1928:142 n.3). But these formal criteria and others like them are not entirely reliable, and in distinguishing what was public from what was private, scholars have often relied on inferences concerning the social context of a given transaction—inferences that are influenced by the a priori assumption that a sharp public-private distinction must have existed.61 As Postgate (1986:27) puts it: “Often we are forced to make a decision on this point by an intuitive assessment of the background as reflected in the document itself, and it is particularly hard to know whether a man’s public obligations were technically distinguished in any way from his private debts.”

Although some sort of distinction seems to be evident in certain texts, the frequent ambiguity on this score raises questions about the suitability of the modern dichotomy of “public” versus “private.” Serious consideration should be given to the possibility that, while official and nonofficial roles were sometimes distinguished, there was no fundamental dichotomy between public and private sectors in the Middle Assyrian kingdom, and indeed elsewhere in the Bronze Age Near East. Thus, as Garelli suggests, the revenues collected by provincial governors (for example), seem to have been their personal property in the same way that their payments to the king were his property. This can be explained if the administrative hierarchy was not based on an impersonal “state” constitution in which public and private affairs were strictly separated, but rather was constructed on a simpler basis as a chain of personal relationships that was characterized, for underlings, by mandatory gift-giving and obligations to deliver goods and services, and by the patronage bestowed in return upon underlings by their masters.

Even Postgate, although he insists on using the heavily freighted and potentially misleading terms “public” and “private,” emphasizes the degree to which “Middle Assyrian government was entrusted to ‘houses’ or private households,” within which provincial governorships and other offices could pass from father to son (Postgate 1988a:xxiii). The study of Middle Assyrian administrative archives shows that: “Apart from the palace, with the ‘tablet-house’ belonging to it, the only secular institution of government in the Middle Assyrian texts is the household (ē = bētu) of individual high officials” (ibid., p. xxiii). To be sure, all administrative offices were subject to royal ratification, in keeping with the king’s theoretical omnipotence as chosen ruler of the land on behalf of the god Aššur. But there was a natural hereditary tendency inherent in this form of political administration, whether the heir of a high-ranking “house” inherited his father’s office or some other comparable office.

This situation was different, Postgate argues, from what existed later in the Neo-Assyrian empire (see Postgate 1974; 1979; but cf. Grayson 1992; 1995). Furthermore, he qualifies his view of the public-private distinction in the Middle Assyrian period by noting that it was actually not a matter of two separate organizations. In his view, the administration of the kingdom was effected through existing households, which had long since developed the requisite administrative skills because of their involvement in far-flung commercial activities. As he says:

The needs of government can have differed only in degree from those of a large private household . . . The skills and duties of the staff will have been the same in each case, and hence . . . it is often impossible to know if a particular official is engaged on public or private business: just as the state employed the expertise of the existing households to realize its administrative aims, so the households themselves no doubt used their staff equally for private and state affairs. [Postgate 1988a:xxiv]

At most, then, the terms “public” and “private” refer, not to separate socioeconomic sectors, but to different actions by the same persons—actions that may be classified from our point of view as official versus nonofficial but were not always distinguished

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61 Machinist (1982:28 n.108) discusses the difficulty of distinguishing the public and private transactions (and corresponding documents) of Middle Assyrian officials, in relation to various criteria used by J. J. Finkelstein (1953), as well as by Koschaker.
as such by those who performed them. As I have concluded above in chapter 12.2 with respect to the Ur III period, the dichotomy here is not between two broad sectors of property and personnel, but between the two possible orientations of the manifold personal actions undertaken within every household at every level in the hierarchy; that is, between externally or “publicly” oriented actions taken by a dependent householder on behalf of his master (who at the highest level was the king), and internally or “privately” oriented actions undertaken for the direct benefit of the dependent householder himself.

This brings us to the old question of Bronze Age “feudalism,” with all that this term implies in terms of local autonomy and the decentralization of power. Garelli (1967:20) rejects the term “feudal” as a description of the Middle Assyrian kingdom, arguing that evidence for “la prépondérance de l’État” excludes this. This opinion is shared by most Middle Assyrian specialists of the past few decades, including Postgate, who sharpens the point by arguing that what Garelli treats separately as “private” land should not be distinguished from land that was held as a concession from the crown. Against Igor Diakonoff’s two-sector model, Postgate argues that there was no separate category of land owned by village communes. Rather, the Middle Assyrian king was the “ultimate owner” of all of the land in his kingdom (Postgate 1982:310ff.; cf. Diakonoff 1969b). Thus Postgate concludes that, in theory at least, “all ‘normal’ private land was granted to free men by the crown, to be held in direct line from father to son only, in return for the performance of ilku services” (Postgate 1971:517; cf. Garelli 1967:6–14). Both the land and the service obligation were inherited, and when a patrilineal family line died out, the land reverted to the king, who could then grant it to another person.62 The system that Postgate describes is identical to what I believe to have been the situation in Ugarit in precisely the same period (14th–12th centuries B.C.).

Land Tenure and ilku Service
Postgate (1971:518; 1982:307) suggests, further, that when land was sold in the Middle Assyrian kingdom, the ilku obligation remained with the sellers, as was the case at Nuzi (a secondary town in the small provincial kingdom of Arrapha, east of Ashur, where several thousand cuneiform texts were found). He sees in this a weakening of the original connection between land tenure and state service in Assyria because, having sold their land, newly landless persons would still have been saddled with an ilku obligation.

But the extant Middle Assyrian land-sale texts do not mention ilku at all, as Postgate acknowledges, so there is no direct evidence to contradict the alternative view that the service obligation was transferred to the buyer of the land and was not separated from land tenure.63 The problem with the latter interpretation is that a buyer of much land would have accumulated a very large ilku burden, assuming that the ilku obligation was additive. Without more detailed information about the way in which ilku was individually assessed, we can only speculate about the precise workings of the system. It seems likely, however, that the holder of a great deal of land who consequently owed much ilku would have been able to furnish large numbers of subordinates (i.e., servants or subtenants) in fulfillment of his service obligation.

If a buyer of land assumed the ilku obligation, what are we to make of the evidence from Nuzi (cited by Postgate) that the sellers of land were responsible for ilku? It is important to note that the “sale” of land at Nuzi was not a simple real-estate transaction but was in the unusual form of the adoption of the buyer by the seller, permitting the buyer to inherit the land. There were thus peculiarities in the system at Nuzi that may make it inappropriate for comparison in this context. Nuzi had a mainly Hurrian population and so was heir to cultural and legal traditions quite different from those found elsewhere in Mesopotamia and Syria. (On social relations in the kingdom of Arrapha, which itself exhibits many patrimonial char-

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62 Note also Postgate’s (1971:509) suggestion that a nava-ia-lu in Assyria and Ugarit was a man who had no male heir to inherit his landholding, causing it to revert to the king. Postgate (1982:311) later retracted this interpretation, accepting Nougayrol’s interpretation “default.” But one wonders whether the term might not have encompassed both situations; i.e., defaulting on the hereditary service obligation because of the lack of an heir, and defaulting through sheer nonfulfillment of the service requirement (see chapter 11.1 above).

63 In one instance, it is true, the service obligation remained with the sellers of land also in Ugarit (see RS 16.156 = PRU 3, p. 61f.). This seems to have been an exception, however (note that the buyer was a woman), because the service obligation is not mentioned at all in land-sale texts from Ugarit unless, as frequently happened, the king gave an exemption—presumably to the recipient of the land, as was the case in the land-grant texts—specifying that there was no service due on the purchased land (see, e.g., RS 15.119:r. 17 = PRU 3, pp. 86ff.; RS 15.136:18f. = PRU 3, p. 121f.; RS 16.133:13f. = PRU 3, p. 59f.; RS 16.139:13 = PRU 3, p. 145f.; RS 16.147:17 = PRU 3, p. 90; RS 16.154:17 = PRU 3, p. 127f.).
acteristics and was characterized by extended patriarchal households, see Dosch 1993 and 1996. For a more general overview, see Maidman 1995.)

But if Postgate were proved correct in his view that the formal ilku obligation remained with the seller of land also in the Middle Assyrian kingdom, this could be explained by assuming that the seller normally stayed on the land as a tenant of the buyer, as was the case at Nuzi. If so, the sale of land would not really have destroyed the connection between land tenure and royal service, for such a sale amounted to the formation of a subtenancy arrangement in which the buyer as chief tenant—although ultimately responsible for the royal service owed on all of the land that had come into his possession—was legally entitled to call upon his subtenants, as well as members of his own immediate household, to provide the ilku service assessed by the palace on all of his holdings. Without some such connection between land tenure and ilku, it is hard to see how the royal service system could long survive.

This, in effect, is Postgate’s own solution to the problem. If we imagine a hierarchical land-tenure system consisting of both royal tenants and their subtenants, and assume that the sale of land normally resulted in the subtenancy of the seller under the buyer (who, after all, needed workers to cultivate his newly acquired land), then it was in the buyer’s interest to make sure, as a condition of the sale, that the seller was legally obliged to furnish the ilku service associated with his former landholding. Those who had sold or otherwise lost their land no longer earned their livelihood from land held directly from the king, but in an agrarian society they must have been forced into some form of dependency on those who did, as Postgate points out. Landless persons either subsisted as day-laborers or household servants within a larger household, in which case they did not themselves owe ilku service to the king, but instead were dependent on a householder who did (and may well have been sent as substitutes to perform his service); or else they maintained their own households as subtenants of a greater householder, in which case they agreed to provide the ilku service assessed on their subholding.

Postgate (1971:496ff.; 1982:307f.) suggests that this kind of subtenancy arrangement is reflected in the phrase ilku ša alāyuttī, “the service of villagers,” which is found in a Middle Assyrian legal text (KAJ 7:24). A translation of the relevant passage, following Postgate’s interpretation, is as follows: “PN and her children will remain villagers (alāyūtī) of Amurru-naṣîr and his sons and will perform the service of villagers (ilku ša alāyuttī) on behalf of (ana) Amurru-

It is apparent from this text and from the Middle Assyrian laws that an alāyū was a “dependent villager,” dependent either on the palace or on another person—in this case, a man named Amurru-naṣîr. It was in his interest to allow such villagers to work his land as subtenants only if they assumed legal responsibility for the royal service assessed on their land and performed it on his behalf. As Postgate puts it:

It seems therefore that the “villager of Amurru-naṣîr” (or “of the palace”) must be a dependant whose prime characteristic is that he (or she) lives in a village (as opposed to one who lives in the family house or in a town, viz. Assur). We may fairly assume that his master does not live in the village himself, and that the function of the villager is to cultivate the land which Amurru-naṣîr (in this case) owns. We need not suppose that he owned a whole village, but he may well have had property in a number of different places, in all of which he may have had “villager dependants”; the existence of families in Assur which owned land in outlying villages is well attested in the KAJ texts. [Postgate 1971:497f.]

Note that in imperial contexts there is evidence of a hierarchy of ilku-service involving great kings and lesser kings. This is attested at Ugarit, for example, where the king of Ugarit was owed (p)ilku service by his landholding subjects, but himself owed ilku to his Hittite overlord (see RS 20.212′ = Ug 5.33, in which the Hittite king refers to the ilku obligation of the king of Ugarit). Indeed, the ilku owed to kings and by kings to emperors constituted the peak of a “manorial” hierarchy of dependent land tenure that was widespread during the second millennium B.C., and served as a simple but effective means of coordinating political authority with economic production and distribution.

See CAD A/1, p. 391, s.v. *ālû. Although the translation “villager” for alāyū is based largely on the etymological connection to alû (“city, village”), this interpretation seems plausible and has been widely accepted (e.g., Diakonoff 1969b:231f.; Garelli 1967:14).

It is possible that the ilku ša alāyuttī, “service of villagers,” was performed directly for Amurru-naṣîr rather than for the palace, in which case there was a hierarchy of ilku service corresponding to the hierarchy of land tenure. But ilku is usually interpreted as specifically royal or “state” service (see Postgate 1971:497f.). The practical result is the same, in terms of the duty owed to a higher master which is entailed in both tenancy and subtenancy, whether or not the meaning of the term ilku can be extended to lower rungs in the social ladder.
7. Ugarit’s Syrian Neighbors: Alalah and Emar

In addition to Ugarit, two other Late Bronze Age Syrian sites have provided us with cuneiform archives that yield clues to the social and economic organization of Levantine states of the time. One of these sites is the city of Alalah in the Amuq plain, not far from the mouth of the Orontes River, which was excavated by Leonard Woolley in the 1930s and 1940s. Alalah was the capital of the prosperous kingdom of Mukiš, Ugarit’s neighbor and competitor to the north. The tablets from Alalah level IV date to the fifteenth century B.C., during the period of the Mittannian empire, a century or two earlier than the archives of Ugarit. Nonetheless, they are close enough in time and space to permit comparison. As for archaeological evidence, a number of private houses in this city were excavated and published by Woolley. Regrettably, most of them were poorly preserved, except for the latest specimens found in Levels II and I, dated by Woolley to the latter part of the Late Bronze Age (ca. 1350–1190 B.C.) and thus contemporary with similar houses at Ugarit discussed below in chapter 13 (see Woolley 1955:172–200, and p. 399 for his dating of the levels). In his discussion of the Level II houses, Woolley commented on their haphazard construction and the lack of a standard building plan, observing that “neighbouring houses might be of very different sizes—rich and poor lived cheek by jowl—and so far as the fabric was concerned rich and poor houses were much alike” (ibid., p. 185). The thickness of the walls suggested to him that many of the houses had a second story, and he believed that the arrangement of the rooms indicated that some houses, at least, had open courtyards or light-wells. Courtyard houses, an irregular street plan, and the association of large and small dwellings are expected features of traditional Near Eastern urban neighborhoods, as I have noted elsewhere. Unfortunately, Woolley’s conclusions about the Alalah houses were necessarily tentative because only the foundations were preserved and he could not identify the locations of doorways. Furthermore, he uncovered only a small portion of the domestic architecture of the site because his focus was on the palace and temple area in the northwestern portion of the tell. Enough evidence is available, however, to establish clear architectural similarities between the Alalah houses and contemporary dwellings at Ugarit, as Olivier Callot (1983:77) points out in his study of house design at Ugarit.67

The other Syrian site to be considered is the city of Emar, on the great bend of the Euphrates River in inland Syria, which was excavated by French archaeologists in the 1970s.68 The Emar texts, although they come from a site more distant from Ugarit, were written from the late fourteenth to the early twelfth centuries (the period of the Hittite empire in Syria), and so are contemporary with those from Ugarit. Again, little has been published concerning domestic architecture at this site, although the cuneiform archives provide much valuable information. Despite their geographical and chronological proximity there are striking local differences evident in the archives of Ugarit, Alalah, and Emar, especially in the terminology used to describe various social groups. Alalah, for example, had come under much greater Hurrian influence as part of the Mittannian empire, and the sociopolitical terminology used there was quite different from that in Ugarit and Emar. But it is not unreasonable to expect that these neighboring polities shared certain basic features of social and economic organization, and the texts bear this out.

Land Tenure and Social Status

Thirty-three tablets from Alalah level IV contain lists of names of the inhabitants of different villages, grouped according to various categories of personnel (see Dietrich and Loretz 1969a). Another 33 census lists (published in Dietrich and Loretz 1970) list different categories of “households” (έ) in various villages, usually listed according to the names of their owners. The latter group of texts do not mention specific professions and so are less useful, although they broadly confirm the picture provided by the lists of individual villagers. The largest group in each village are called sēbē namē; that is, personnel living in the countryside, whom Speiser (1954:21) describes as “rural retainers” (also Dietrich and Loretz 1969a:91). These are subdivided into two subcategories: ḫupšēna (sg. ḫupšē) and a smaller group known as haniahhena (sg. haniahhe).

67 Callot also draws attention to Late Bronze Age houses at Enkomi in Cyprus (see Dikaios 1969), where ashlar masonry was used extensively, as in the private houses of Ugarit (unlike the Alalah houses, which had mudbrick walls).

In distinction from these šābē namē were a small minority of villagers called ehelena (sg. ehele), a Hurrian term that apparently means “released- or freed-men,” judging by the use of the Akkadian synonym šüzubūtu (from ezēbu; see Speiser 1954:21; Dietrich and Loretz 1969a:92). Very likely these were men who had been exempted from the usual royal service requirement, in contrast to the šābē namē, who were not exempt from such service. In many of the villages there was also a small group of elite maryannena (sg. maryanne), who were chariot warriors.69

In villages without maryannena the breakdown by personnel category is roughly 90% šābē namē (80% hupšena plus 10% haniakhēna) and 10% ehelena. In villages with maryannena the breakdown is 80% šābē namē, 10% ehelena, and 10% maryannena (Ser-angeli 1978:116f.). By averaging the thirteen best preserved census lists of individuals we obtain an overall estimate for the kingdom of Mukiš of 65% hupšena, 13% haniakhēna, 11% ehelena, and 11% maryannena—although other social groups that may have existed are not included in these figures (von Dassow 1997:118).

The term hupšu is known also from the Amarna letters and the Nuzi archives, as well as from Mesopotamian texts (CAD s.v.; cf. Mendelsohn 1941; 1955; Lacheman 1942). In these diverse sources people called hupšu seem to have been relatively poor, low-ranking persons who were subject to corvée labor and military service but nonetheless had landholdings of their own. In the Alalah IV texts the term hupše is sometimes replaced by the term purrē, which seems to refer to a specific type of landholding (Dietrich and Loretz 1970:119), or by the term unuššahuli, “one who owes royal service” (Dietrich and Loretz 1966:194ff.; von Dassow 1997:141ff.).70

For the term haniakhē, on the other hand, Dietrich and Loretz (1969a:91f.) suggest a meaning “impoverished, destitute,” based on the synonymous use of Akkadian ekā (so also Speiser 1954:21; AHw, p. 195). It is likely that the haniakhēna of the kingdom of Mukiš lacked hereditary landholdings, in contrast to the hupšena, and therefore subsisted as artisans, soldiers, or day-laborers, or as subtenants of landholders. The term literally means “Hanaeans,” that is, people of Ḥana (the steppe region near the middle Euphrates), a description earlier used in the Mari texts for a type of soldier. This has led Liverani (1975:152) and Serangeli (1978:109f.) to describe the haniakhēna of Mukiš as seminomadic pastoralists, as opposed to settled peasants, who were the hupšena. But this interpretation is unlikely, not only because the haniakhēna were clearly villagers, but also because their professions include atkuppu (“reed-mat weaver”), aškāpu (“leatherworker”), pahāru (“potter”), nāru (“singer”), and nappāhu (“smith”), as well as namattanni (“herdsman”). These are all occupations that were shared by hupšena, and even by ehele na in some cases (see Dietrich and Loretz 1969a:87ff.).

That haniakhēna lacked hereditary landholdings is supported by the fact that the Akkadian term muškēnu, which is rare in the Alalah IV texts, seems to be used as a synonym for haniakhē (see von Dassow 1997:384ff.). The idea that in Mesopotamia a muškēnu was a landless person who therefore did not owe royal service is discussed in some detail above in chapter 12.4. Such a person was often a subordinate or dependent of a landholder. If many of the haniakhēna were originally immigrants from the steppeland of Ḥana near the middle Euphrates, as the name suggests, then it is not surprising that they lacked royally granted landholdings, in contrast to longer established inhabitants of the kingdom, and so gave their name to the general category of landless personnel. It is interesting that in the Mari letters we have references to muškēnu in conjunction with Ḥanaeans (e.g., “the sheep of the Ḥanaeans and muškēnu living on the bank of the Euphrates,” ARM 5:81:5).

A more common synonym for haniakhē in the Alalah IV texts is bidallenni (or pitallenni), which may refer to a particular subtype of haniakhē-personnel (Dietrich and Loretz 1970:122; cf. von Dassow 1997:134, 142). This term is probably derived from Hurrian *p/bid(d)-, “to help,” with an agent suffix -ē, and means something like “helper, assistant, deputy” (Márquez Rowe 1998:372; note the similar terms bidalu at Ugarit and baddālam at Ebla, discussed above in chapter 11.1). The equivalence of bidallenni and haniakhē is shown by comparing the household list AT 192, which has 36 purrē (i.e., hupše) households followed by 19 bidallenni households and 5 ehele households, to the similar list in AT 195, with 35 purrē households followed by 13 haniakhē households and 3 ehele households (see von Dassow 1997:128). On the other hand, bidallenni and haniakhēna both appear in AT 189, which suggests that bidallenni personnel could be distinguished as a subcategory of haniakhēna—perhaps this term refers specifically to landless men who served landholders as their assistants or deputies.

69 This word, like hupšena, haniakhēna, and ehelena, was written at Alalah with a Hurrian plural ending; cf. the Ug. plural mrynm; i.e., /maryannuma/.
70 In Akkadian texts from Ugarit the Hurrian term unuššu is sometimes used in place of (p)ilku to denote royal service.
In general, it can be said that the social groups called ēhelēna, ḫupšēna, and ēhelēna by palace scribes in Alalah were not defined in terms of economic function or profession but in terms of their place in the royal land-grant system as, respectively, landless persons, landholders who owed royal service, or landholders who had been exempted from royal service. The additional category of maryanne-chariotiers is explicable because of the special privileges (and political role) no doubt accorded to this important group of military professionals.

This interpretation conflicts with the view advanced in a recent doctoral dissertation by Eva von Dassow (1997). Her detailed study of the sociological terminology used at Alalah contains much useful information, but her emphasis on the metaphor of “social stratification,” implying a layer-cake of self-contained social classes, tends to neglect the “vertical” ties between persons of varying social ranks. What seem to be discrete social groups (“strata”) from an external perspective may have been held together by quite complex personal interrelations based on patronage and clientage at various levels of the social hierarchy. It is true that we lack direct evidence (e.g., letters and legal documents) for such relationships in ordinary village society, but we should not therefore conclude that they did not exist, as von Dassow does when she says “there is no indication that dyadic relationships of mutual obligation and dependence were the basis for organizing the Alalah IV social hierarchy” (p. 86). What we have in the available textual documentation is the royal palace’s perspective, as von Dassow acknowledges, and the personnel categorization used by the palace was not concerned with local social relations but with the relationship between the king and his subjects in terms of land tenure, in-kind rental (“tax”) payments, military and labor service obligations, and royal patronage (in the form of grants and exemptions) bestowed on favored subjects.

In particular, to say that the ēhelēna were the “professional and service sector” (ibid., p. 353), as if this were the defining characteristic of that group, is contradicted by the fact that not all ēhelēna had specialized occupations and those who were not ēhelēna did have such occupations. Von Dassow calculates that 62% of ēhelēna had specialized occupations and 50% had an “employment relationship” with another person; “for altogether 85% of the class, either an occupation or an employer, or both, is recorded” (p. 345). The fact that a relatively high proportion of them had special skills or were employed in some fashion by high ranking persons (especially by maryanne-chariotiers or the king himself) is presumably what led to their elevation to the rank of ēhelē, as a reward for their services. But their special skills and service to the elite are not what defined the ēhelēna as a group, for there were ḫupšēna and even ēniahēnna with similar professions.

The occupations mentioned for members of the four different social groups are summarized by Dietrich and Lorentz (1969a:87ff.). There was a leatherworker (aškāpu) in each group, including the maryanne. The following professions are found among both the ēhelēna and the sābē namē (ḫupšēna or ēniahēnna): aškāpu (“leatherworker”), ušandû (“fowler”), naggāru (“carpenter”), namattannu (“herdsman”), nāru (“singer”), nukarību (“gardener”), kizū (a type of servant), and nappāhu (“smith”). Note also that two of the ḫupšēna were diviners (bārū) and one was a physician (ašā), presumably fairly prestigious specialties, whereas 8 of the 59 ēhelēna for whom professions are mentioned were mere herdsmen of one kind or another (compared to 1 of 18 ḫupšēna and 2 of 12 ēniahēnna).

Two Sectors or One in the Kingdom of Mukiš?

Mario Liverani (1975:151ff.) has distinguished the lower ranking groups in the kingdom of Mukiš from higher ranking groups in a different way. He has applied the two-sector or “Asiatic mode of production” model to the data from Alalah, arguing that ēhelēna and maryanne were “men of the king” belonging to the palace sector, who therefore did not own the means of production; whereas the sābē namē were “free men” who owned the means of production and so belonged to the communal village sector.71

The Alalah texts do not support this interpretation, however, especially in light of Liverani and Heltzer’s own two-sector interpretation of the data from Ugarit (discussed in chapter 11). Based on the Ugarit material, these scholars have argued that palace dependents lived in the capital city or on royal farms, for the most part, where they received royal rations and engaged in specialized work. The inhabitants of the villages, on the other hand, are viewed as simple farmers who did not have specialized professions and were not known individually by the palace (see, e.g., Liverani 1975:147ff.). But the Alalah IV census lists

71 Other scholars who accept the two-sector interpretation of the Alalah data include Gaal (1976; 1978; 1988) and Klengel (1979), who have discussed the changing role of the “state sector” between Alalah VII and Alalah IV. The increasing popularity of the two-sector (Asiatic mode of production) model in research on Bronze Age Syria-Palestine, even in non-Marxist circles, is evident from its use by a number of contributors to Schwartz and Falconer (eds.) 1994; e.g., Magnes-Gardiner, who applies it to Middle Bronze Age Alalah in particular.
contradict both hypotheses, because a substantial number of rural villagers were palace dependents (ehelena or maryannena), according to the two-sector model, and the šabê namê, whom Liverani places in the communal village sector, not only were known individually by name to the palace administration at Alalakh but also had professions in many cases. Furthermore, as we have seen, the professions of the šabê namê overlapped considerably with those of the ehelena—and even with the maryannena, in one instance (aškāpu, “leatherworker”).

I do not deny that the ehelena, and especially the maryannena, often possessed greater wealth and higher rank than the šabê namê. In the extant census lists, three of the maryannena and one of the ehelena are called ḥazānu (“mayor”), and one man from each group was a ūpšarru (“scribe”), titles not found among the šabê namê. Only among the ehelena is there mention of seal-cutters and a cup-bearer. Moreover, the proportion of ehelena who had specialized occupations was higher than the proportion among the šabê namê, as both Liverani (1975:148 n.3) and von Dassow have emphasized.

But it is incorrect to argue that the ehelena were “palace dependents” in a way that the šabê namê were not. The census lists show that both groups lived together in villages, and presumably all villagers subsisted largely by means of agriculture on land granted by the king. There is no reason to suppose that the ehelena and maryannena were absentee landlords; and it is hard to imagine that these rural “men of the king” were fed royal rations, which was the main source of support for palace dependents at Ugarit, according to Liverani.

Moreover, he admits that specialized occupations must have been part-time rather than full-time for the ūpšena and the haniakhena (his “free” farmers). But the same can be said for the ehelena, many of whom had exactly the same occupations. There is no conflict between professional specialization and farming, as I have argued in chapter 11. Indeed, evidence from Ugarit shows that even the maryannu-chariotteers had agricultural estates and resided in their home villages, in certain cases, although most of the farmwork may have been done by their kinsmen and clients. Furthermore, the extensive system of royal land grants that is especially well documented at Ugarit suggests that agriculture was important for everyone, not just for “unspecialized” villagers. Thus Liverani greatly exaggerates the difference in professions between the ehelena and the šabê namê when he says: “Complètement différent est le cas des ehelešûzûbu (dépendants du palais), qui ont normalement une qualification de travail particulière et même très spécialisée (jusqu’à ‘scribe’)” (Liverani 1975: 148 (jusqu’à ‘scribe’)) (Liverani 1975: 148 n.3). In fact, only one of the 59 ehelena for whom a profession is mentioned was a scribe, and most of them had relatively menial occupations similar to those of the šabê namê.

In contrast to the two-sector model, then, we can regard most of the villagers listed in the Alalakh texts as “royal dependents,” in the sense that they held land from the king and so could be called up for service unless specially exempted. (The exceptions to this were the 13% who were “destitute” or landless haniakhena, who probably entered royal service only as deputies for landholders for whom they worked.) The names and occupations of these villagers were therefore of interest to the palace administration, and their classification into different groups was related to their rank and privileges. It is likely that the ehelena were those who had received exemptions from royal service, which is why they were called šûzûbu (“saved” or “freed”) in Akkadian. Such exemptions were quite frequent later in Ugarit, as we know from legal texts recording royal acts, and they were often granted in return for payments of silver or gold. The ehelena therefore tended to be wealthier and more influential (and were also far less numerous) than the ordinary šabê namê, who are identified by that very term as personnel living outside the capital city who were subject to conscription for corvée labor and military service.72 But the fact that the ehelena and the šabê namê shared many of the same professions (including, most basically, that of “farmer”) indicates that they did not belong to separate “sectors” of society. Indeed, it is probable that ūpšena were occasionally granted exemptions from royal service and thereby became ehelena, just as a man could be elevated to the hereditary rank of maryanne (see, e.g., AT 15 = Wiseman 1953:39). The maryannena, of course, occupied the highest rank, and were presumably exempt from ordinary corvée service because they served the king as elite chariot warriors. At the other end of the social scale, the ūpšena and haniakhena were differentiated within the šabê namê apparently because the haniakhena did not have landholdings of their own; hence the Akkadian synonym ekatu (“homeless, destitute”). As a result, most haniakhena probably had to attach themselves to wealthier patrons as dependent clients.

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72 The Akk. term šabu (ERIN) is usually used in reference to gangs of corvée workers or military troops, or people subject to such service (see CAD s.v.).
More valuable than his application of the two-sector model to the Alalah texts is Liverani’s use of the census lists to calculate certain overall demographic characteristics of the kingdom of Mukiš. A similar demographic analysis has been carried out in greater detail by Flavia Serangeli (1978). Although villages varied greatly in size, there was an average of 26 households per village. At the same time, the lists of individual men by their villages indicate an average of ca. 45 adult males per village, which yields an average of ca. 1.7 men per household. If we include two other texts that list households in larger towns, we obtain an average of 31 households per settlement (counting both villages and larger towns), or 170 persons per settlement, assuming 5–6 persons per household. The total number of towns and villages known from the Alalah IV texts is 215, thus Serangeli estimates the total population of the kingdom of Mukiš at ca. 40,000 in the fifteenth century B.C., adding 5,000 for the population of the city of Alalah itself.

Von Dassow (1997:58–66) questions the assumption that the census texts list all of the households in each village and include all of the social groups that inhabited the kingdom. We can also question the assumption that there were only 5–6 persons per household, in view of the average of 1.7 adult males per household. Many households must have had two or more adult males in order to produce this overall average, thus a mean household size of 7 or 8 persons might be more likely, especially if we allow for servants and more distant kinsfolk. But even if some of her assumptions are wrong, it is unlikely that Serangeli’s figures need to be increased by very much. Estimates on the order of 25–30 households and 150–200 persons per village are quite plausible on ethnographic grounds, and the fact that many households must have had two or more adult males fits the Mediterranean joint-family household model discussed above in chapter 7.2. Unfortunately, it is not possible to determine the proportion of households with only one adult male as opposed to two or more, which would indicate the relative proportion of nuclear-family versus joint-family households.

Village Transfers and Patrimonial Society

The census lists from Alalah level IV contradict the two-sector model and accord better with a unitary model of socioeconomic organization such as the PHM. Indeed, there is clear evidence for the patrimonial interpretation from the archives of Alalah level VII. These date to the Middle Bronze Age, so they are less directly comparable to the texts from Late Bronze Age Ugarit and Emar; but they demonstrate features of second-millennium Syrian society that persisted as late as the end of the Late Bronze Age. In particular, the practice of granting whole villages to individual persons is well attested in the Alalah VII texts of the seventeenth century B.C., a practice that we know still existed as late as the thirteenth century in Ugarit.

Carlo Zaccagnini (1989:56–98) has discussed the transfer of villages in some detail, using evidence from Nuzi and Hittite Anatolia as well as from Alalah and Ugarit. Like Liverani, Zaccagnini believes that Bronze Age Syria was characterized by the Asiatic mode of production, which entails a two-sector socioeconomic structure. Evidence for individual ownership of entire villages contradicts the two-sector model, however, because the villages and their territory are supposed to have been communally owned by the peasants, who on that basis are said to have owned the means of production and so are regarded as “free” proprietors, in the Marxist sense. The fact that much of this evidence comes from Alalah VII is especially damning in this regard, because it dates from the first half of the second millennium, when—in contrast to the Late Bronze Age—the two-sector mode of production was closer to its pristine form, according to Liverani.

At first, Zaccagnini tries to solve the problem that the evidence for individual ownership of villages creates for the two-sector model by arguing that “the mention of one or more villages as belonging to a king, means on principle no more than that the members of that/those communities, as owners of the means of production, are included in the mechanisms of tributary dependence of an administration that is headed by the king” (Zaccagnini 1989:57f.). Thus, when a tablet records that the Hittite king gave certain villages to the king of Ugarit and to his heirs, this is not to be taken literally as signifying personal ownership; such texts “merely sanction the fact that a region . . . belongs to or is being handed over to a palatial organisation” (ibid.). The actual “ownership” of the villages and their land remained with the peasant families who lived there, according to Zaccagnini. Similarly, when a lesser king transferred a village to

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73 The actual frequency distribution as presented graphically by Serangeli (1978:114) is: 14 villages with 10 or fewer households; 12 villages with 11–20 households; 11 with 21–30; 5 with 31–40; 6 with 41–50; 3 with 51–60; 1 with 61–70; 2 with 71–80, and 1 with 81–90.

74 On the other hand, it is possible that at any one time most households would have been in the nuclear-family phase of the household lifecycle, thus lowering the average household size.
one of his subjects, that person did not “own” the village but merely received the right to its revenue.75

A clear distinction is made in the texts, however, between assignment of the rights to a village’s revenue and the outright grant of the whole village. Several documents from Ugarit record royal grants of villages to various persons, normally on a hereditary basis. These texts explicitly state that the grant includes the village itself, as well as the various types of revenue from it. In another text, however, the grant is limited specifically to the tithe (ma’šaru) of a certain town, which is assigned to a royal official for the duration of his life and is not granted on a hereditary basis (RS 16.244 = PRU 3, p. 93). If the village-grants were merely revenue-grants, like this one, it is odd that the texts should begin by stating that “RN gave GN to PN (and to his sons),” before going on to enumerate the kinds of revenue (including the tithe) that now belonged to PN. In two cases (RS 15.114 = PRU 3, p. 112f. and RS 15.147 = PRU 3, p. 125) the individual “ownership” of the granted villages is quite clear, because the recipients are charged with rebuilding and repopulating them, in return for which the inhabitants of those villages were exempted from the royal corvée, presumably because all payments of goods and services were made instead to their new owners.

Faced with this evidence, Zaccagnini tries to mitigate its effect on the two-sector model, but he is forced to admit that, in some cases at least, effective ownership of whole villages passed to individual persons and their descendants (Zaccagnini 1989:64–67). He interprets this, however, not as disconfirmation of the two-sector model in general, but as evidence of the gradual breakdown of the Asiatic mode of production toward the end of the Bronze Age. (Liverani’s version of this argument has been discussed above in chapter 11.2.)

As Zaccagnini puts it:

The transfer of villages can be interpreted as an evident symptom both of degeneration in the community structures and of breakdown in those administrative mechanisms on which the functioning of the Ugarit Kingdom was based. Even though all mention of a resident peasant workforce is lacking, it seems there can be no doubt that these concessions represent a sharp shift towards the subjection of the rural populations who find themselves faced with a precisely identifiable “lord” to whom they must pay taxes and dues and who has a right to corvées. This “lord,” in the short or long term acts as the exclusive/main owner of the means of production . . . [Zaccagnini 1989:67ff.]

The methodological weakness of this approach is demonstrated by the fact that there is no evidence that the two-sector mode of production was ever in force at an earlier period in Bronze Age Syria. Indeed, the opposite is true, as the texts from Middle Bronze Age Alalah (level VII) reveal. If the Late Bronze Age texts from Ugarit demonstrate the breakdown of the two-sector system, as Liverani and Zaccagnini have argued, then the earlier texts from Alalah VII, which document an even greater number of village transfers, must (paradoxically) be evidence of a more advanced breakdown—or, as is more likely, the two-sector model itself is incorrect.

Zaccagnini (1989:68–81) discusses the Alalah VII texts in some detail, and again he is forced to acknowledge the existence of personal ownership of entire villages, by the king and by others. A key text here is AT *6, the will of Ammitakum, ruler of Alalah, in which he bequeathed “his household” (ē-sū) to Hammurapi, his son. This household included “his house, his villages, his lands, and whatever is his” (Wiseman 1953:33f. and plate V). The text concludes by quoting Ammitakum directly: “Hammurapi is the master of my city and my household” (Ḫa-am-mu-ra-pī be-ē-ur.KI-ia 22 ū Ē ia ša-ū). It is clear from this text that the ruler’s villages were treated like any other personal possession, and a number of other texts show that the ruler could give, sell, or exchange them in transactions involving other kings and his own subjects (e.g., AT *11, AT *52–*58 [sales], AT *76–*80 [exchanges], AT *95, all

75 The same argument is made by Heltzer (1976a:48–51), in opposition to the feudal model of Ugaritan society, and by Klengel (1965:153f.), with respect to Middle Bronze Age Alalah.
77 Heltzer, on the other hand, refuses to accept individual ownership of any villages, suggesting that the village grants (which he interprets as merely the right to collect revenue) were not hereditary, “despite the fact that some texts stated that the granting of a village applied to the children and grandchildren of the favoured individual” (Heltzer 1976a:51). Zaccagnini, however, considers Heltzer’s view “too reductive” (p. 66).

78 The relevant portion of this text reads: ši-im-ti ši-su i-ši-im-ma ši-su URU.KI-ŠA-e-pi-ri-šu ū mi-im-mu-a-išu ki-ma a-bu-šu ū um-ma-šu a-na LUGAL ú-wi-id-di-šu a-na ŪŠU.BE-ŠA-si ŠA-a-na. The description of the king’s domain as “his house(hold)” accords quite well with the patrimonial household model. Note also the famous Idrimi inscription found at Alalah and dating to a slightly later period, in which “the city of Aleppo” is referred to as “the house of my father” (Greenstein and Marcus 1976:64 l. 3; also Dietrich and Loretz 1981:204).
published in Wiseman 1953). There was simply no distinction between “public” lands or “free” villages, on the one hand, and the king’s personal possessions, on the other.

Liverani (1983a:153ff.) also acknowledges that individual ownership of villages existed, in some sense, already in Middle Bronze Age Alalah. But neither Liverani nor Zaccagnini concludes from this that the two-sector model is faulty. Instead, they take it as evidence that the peasants had lost their original communal ownership of the means of production, and conclude that the crisis of the Bronze Age socioeconomic system had already begun. As Liverani puts it:

Déjà à l’âge du bronze moyen, comme l’illustrent les textes de Alalah (couche VII), sont attestés des villages qui “appartiennent” à quelqu’un, faisant l’objet de transmission héréditaire, de vente, de permutaton. . . Ces ventes de villages semblent donc configurer une situation de désagrégation de type “seigneurial,” plutôt qu’une concentration de type palatin. [ibid.; see also Zaccagnini 1989:75, 79]

Yet, if all we have is evidence of breakdown, what reason is there, other than a priori theoretical considerations, to think that the two-sector mode of production ever existed in Bronze Age Syria? Liverani and Zaccagnini maintain that the “crisis” of the two-sector mode of production developed slowly and reached a peak at the end of the Late Bronze Age; indeed, they argue that it was the underlying cause of the collapse of Bronze Age civilization in the Levant. But the evidence for the “breakdown” of the two-sector system is, by their own reckoning, greater for Middle Bronze Age Alalah than for Late Bronze Age Ugarit; thus the historical trajectory they posit seems highly unlikely. At this point, proponents of the two-sector model might appeal to the third-millennium texts from Ebla, suggesting that the two-sector Asiatic mode of production was intact in Syria at least during the Early Bronze Age, the earliest urban period in the Levant. But grants of whole villages were common already in Early Bronze Age Ebla. Indeed, as I have argued above in chapter 12.3, the Ebla material can be more plausibly interpreted in terms of the patrimonial household model as opposed to the two-sector model, despite the claims of Alfonso Archi and Lucio Milano.

Horst Klengel, another advocate of the two-sector model, does not think that the two-sector mode of production had already broken down in Middle Bronze Age Syria, as Liverani and Zaccagnini have suggested; and he has offered a different explanation for the village transfers of Alalah VII. He acknowledges that villages were treated as the personal possessions of the ruler of Alalah in this period, whom he therefore describes as the owner of a “master-household” (oikos) within the larger kingdom of Yamhad based in Aleppo (see Klengel 1974; 1979:437; 1992:60). In defense of the two-sector model, however, Klengel explains this phenomenon by suggesting that because the ruler of Alalah was called “man” (LÚ) of Alalah in contexts where his overlord, the king (LUGAL) of Yamhad, is also mentioned, he was not a king in his own right but was simply the master of a household inside the territory of Yamhad. Alalah is thereby exempted from having an internal “state sector” in this period because it was, in toto, part of the state sector of Yamhad, although its territory (or, rather, the revenue therefrom) had been ceded to the “man” of Alalah (Klengel 1979:450).

But this interpretation seems excessively subtle, because it is clear from AT *6:10 (cited above) that, in local terms, the ruler of Alalah was considered to be a king (LUGAL), even if he was treated as a landholding serviceman (LÚ = avilium) by the “great king” of Yamhad. In fact, this demonstrates quite well a patrimonial hierarchy in which subordinate rulers are in precisely the same relation to their political masters as their own landholding subjects are to them. Moreover, Klengel’s explanation does not account for the fact that the ruler of Alalah engaged in the sale or exchange of villages with local landowners (who presumably were his subjects), including his own son, indicating that villages were individually held by persons other than himself (see, e.g., AT *52, *54; *78, *80, *86).

Still another solution to the problem of village transfers has been proposed by Erno Gaál (1976:41), who acknowledges that the ruler of Alalah was called a king and therefore had a domain structurally similar to that of Yamhad, including both a “state sector” and a “private sector.” Gaál argues, however, that the villages that were exchanged between kings or sold by kings belonged to the “land fund of the state sector,” as opposed to other villages belonging to the private sector (ibid., pp. 44ff.). The king of Alalah, for example, occasionally purchased private-sector villages to augment his royal landfund, because the size of a kingdom’s state-sector landfund was an important measure of its relative status. In the Alalah VII texts we read of transfers of villages between kings (all of whom were part of the greater kingdom of Yamhad) because, Gaál argues, an attempt was made to maintain a balance in the relative sizes of the various state-sector landfunds. The kings of Yamhad, in particular, made sure that their own landfund re-
mained the largest, in order to avoid losing the basis for their supremacy, and to enable them to reward their allies with grants of villages. As Gaal puts it:

The economic and territorial units of the large state sector of the royal estates functioned not only as a land fund supplying with food the royal household and in case of emergencies the entire community, but as a land fund of exchange they were also the basis of administrative power in case of revolt, separatist movements and so on, thus, under the given circumstances, the state sector, the royal household of kings residing in Halab maintained the territorial integrity of the state of Iamhad, the sovereignty of the Halab dynasty. [Gaal 1976:46]

This is an interesting hypothesis, and the sale and exchange of villages among kings was no doubt motivated by political considerations of the kind that Gaal suggests; but there is no evidence for a distinction between “state-sector” villages and “private-sector” villages in Middle Bronze Age Syria. Indeed, the king’s purchase of “private” villages from individual landholders, which were thereby transferred to the “state sector,” according to Gaal, shows that his “private sector” did not consist of communally held villages inhabited by peasant owners of the means of production. But this contradicts the two-sector model as it is usually understood. Furthermore, the fact that these “private” villages were held by persons other than the king does not prove that there was a semiautonomous private sector separate from the king’s own household, implying that the king was not the “ultimate owner” of all the land and persons in his domain. By “purchasing” rather than expropriating “private” villages, the king merely compensated those whose landholdings he took for himself, in order to avoid violating their traditional hereditary rights. As I have argued above, the “ownership” of land in the ancient Near East did not entail exclusive possession but involved a customary hierarchy of rights of use and disposition. In addition, all landholders, including the king, stood in the same relationship to the higher authority who had (in theory, at least) granted the land to them or their ancestors. Some landholdings encompassed whole villages and some were quite small; but the ruler was still owed allegiance and service (unless specially exempted) by all of the landholders within his domain.

In my opinion, then, Liverani and Zaccagnini have interpreted the evidence of village transfers from Alalah VII more correctly than either Klengel or Gaal, because they see them as a “seigneurial” violation of the two-sector system. But they conclude—wrongly, in my view—that this demonstrates the breakdown of an originally intact two-sector mode of production, rather than the breakdown of their model. I suggest instead that the textual evidence from both Alalah and Ugarit is better understood in terms of the patrimonial model, which views the king’s entire domain as his “household” (as is explicitly stated in AT *6), a household that consisted of a unitary hierarchy of subhouseholds which were smaller in scale but similar in structure to his own.

The Elders of Emar

The cuneiform tablets from Emar, a city in the Hitite-controlled territory of Aššur in inland Syria along the Euphrates River, are another source of textual information concerning Syrian socioeconomic organization during the Late Bronze Age. Daniel Arnaud, the editor of the excavated texts, has noted the prominent role of the “elders of the city of Emarr” (hi-melṣi-bu-ut u-na-E-marš) and the frequent reference to kin-based groups in general (Arnaud 1980b: 1981). He takes this as evidence of the seminomadic origin of the residents of Emar, and suggests that their ethos of familial solidarity was being eroded under the impact of urban life. There was also a king, but Arnaud and others have argued that royal authority was quite limited (see Arnaud 1975:89f.; Leemans 1988:236ff.; Beckman 1992:48f.; 1997; Fleming 1992b). Piotr Steinkeller has recently challenged this view, however. As he puts it:

On the basis of the fact that, at Emar, the most commonly documented “sellers” of immovables are jointly the god Ninurta and the city of Emar (usually referred to as the “elders of Emar”), some scholars have concluded that these elders held more land than the king (Fleming 1992[b]:65–66), and that there was “economic preeminence of city officials over the Crown” (Beckman 1997:106). This even led to a suggestion that the kingship of Emar was of “limited” nature (Fleming 1992[b] and Beckman 1997). But it appears that in reality the holdings of Ninurta and the city of Emar remained under the ultimate control of the king of Emar (or perhaps even under that of the king of Carchemish, Emar’s political superior). This is demonstrated, in my opinion, by the cases in which the property sold by Ninurta and the city of Emar had earlier been confiscated from its previous owner “because he committed a grave transgression against his lord” (Fleming 1992[b]:65, n. 43; Beckman 1996:30; Beckman 1997:105f.). I believe that, in these examples, the unnamed “lord” (beša) can only be the king of Emar (or that of Carchemish). Accordingly, it appears that the system of property rights at Emar was as follows: apart from the land managed directly by the king (which is documented independently), there existed a fund of “communal” land, classified as the property of Ninurta and the city of Emar. The latter land was, at
least in theory, owned and managed by the city organization, which had the right to convert it into manifestly individual property. Its ultimate owner, however, was the crown, which very likely was also the actual beneficiary of any proceeds that “sales” of such land would generate. [Steinkeller 1999:314f. n. 22]

Because of Emar’s strategic location on the frontier with Mesopotamia, its Hittite overlords apparently took an unusually active role in its internal administration. Indeed, it has been suggested that they stimulated the emergence of a settled population there during the latter part of the Late Bronze Age, installing a king for administrative purposes even though the lack of a recent native tradition of centralized rule tended to reduce his role to that of the first among equals.79 In that case, local government in Late Bronze Age Emar was in the nature of a rather decentralized patrimonial regime. It was headed by a titular ruler, but it also permitted a high degree of autonomy on the part of other leading residents of the city. On the other hand, if Steinkeller is correct, Emar fits the pattern of the typical Syrian Bronze Age patrimonial regime, like Ugarit and Alalah, in which the king was dominant and was recognized as the ultimate owner of all the land. Whether Emar was centralized under a strong king (or under an imperial viceroy in Carchemish working through the local king) or was more decentralized, with power dispersed among leading elders, there is no evidence of a fundamental socioeconomic separation between a bureaucratic “palace sector” and a free “village sector,” as Heltzer and Liverani have suggested was the case in Ugarit and Alalah during the same period.

8. Aegean and Anatolian Palace Economies

Late Bronze Age Ugarit traded with the Mycenaean kingdoms of the Aegean region, and after the mid-fourteenth century B.C. it was part of the Hittite empire ruled from Hattuša in central Anatolia. It is therefore worth comparing, however briefly, the social organization of Ugarit to what we know of similar palace-dominated regimes in Anatolia and the Aegean. There have been relatively few detailed studies of domestic architecture from Aegean sites contemporary with Ugarit,80 but there is some interesting comparative data in Ione Mylonas Shear’s (1987) report on three of the “Panagia Houses” (dating to the Late Helladic IIIB) located outside of the citadel of Mycenae, which were excavated in the 1960s. The typical dwelling here, as in other Mycenaean settlements, was a kind of courtyard house, and the houses were clustered together in an organic or agglutinative fashion. Moreover, the author concurs with the hypothesis, proposed by Tsountas a century ago, that the houses in the residential areas at Mycenae were divided into groups by families or clans. She notes that:

In our area, first one house was built, perhaps by the father or the head of the clan. Some time later a second and then a third house were added, possibly for the sons when they reached manhood. Later, when the houses were damaged by earthquake, the first house was not rebuilt, whereas the other two were, possibly reflecting a reduction in the size of the clan. At some stage, at least one room was subtracted from one of the houses and added to its neighbor, a possible result of one household becoming smaller whereas the neighboring household increased in size. This is just the kind of interplay we should expect to find in an area held by a single family group, whose size would alter through births and deaths, marriages, and the natural process of maturation. [Mylonas Shear 1987:4]

If we modify this interpretation to include not just actual blood relationships but also patron-client ties (often expressed in terms of fictive kinship), then the hypothesis of kin-based residential organization at Mycenae matches the picture presented by traditional Mediterranean and Near Eastern towns, in which residential quarters are inhabited by patronymic associations or “clans” (see chapter 6.3 above). Furthermore, the dynamic architectural history of the houses at Mycenae, with additions and rearrangements of living space to accommodate the needs of extended family groups, is duplicated in the same period at Ugarit (the evidence for this is discussed in chapter 13.2 below). Unfortunately, the cryptic Linear B texts from this period that have been found in mainland Greece and Crete reveal little about the internal structure and interrelationships of urban households, and they only hint at the overall organization of the apparently palace-centered Bronze Age Aegean kingdoms. Still, there is general agreement that the available evidence, both archaeological and textual, shows that strong kings with large palace establishments ruled small kingdoms in a manner similar to that found in the Levant at this time.81

It is true that the Linear B texts, like Near Eastern archives of the period, reflect the concerns of the

79 But note the existence of a kingdom of Emar already in the period of the Ebla archives in the third millennium B.C.
80 Dickinson (1994:50ff., 144ff.) surveys the domestic architecture uncovered at Bronze Age Aegean sites.
81 For a summary of recent research, including Linear B studies, see Shelmerdine 1997; also Bennet and Galaty 1997.
royal establishment and ignore local social relations and economic transactions that may have been very important in the day-to-day life of most inhabitants of the kingdom. This point has been emphasized with increasing force by Aegeanists, just as it has become the accepted wisdom among Near Eastern specialists. But acknowledging a realm of activity beyond that administered directly by the palace does not require us to posit a “private sector” to be placed in structural opposition to a “public sector” represented by the palace. In defining what we mean by a “palace economy” in the Late Bronze Age Mediterranean area, in what were obviously still strongly agrarian societies, the key issue is land tenure. The hypothesis of a “private sector” stands or falls on the question of whether there existed “private” land that was kept separate, in theory and in practice, from royal land. Igor Diakonoff, an influential advocate of this dichotomy, was well aware of this, which is why he placed so much stress on the existence of “free” village communities that owned private land apart from the palace (see chapter 9.2 above).

To rebut this view we should note that, incomplete as our textual sources may be, there is no evidence for such a structural distinction and none is likely to be found, for the notion of private land is inimical both to the logic of contemporary royal ideology and the practical reality of royal domination in highly militarized and politically centralized kingdoms, most of which were geographically quite compact in order to permit just this kind of royal domination. To take just one piece of evidence, the practice of granting whole villages to royal officials and other favored men, attested at Ugarit and elsewhere, contradicts the concept of private land.

I have challenged the alleged public-private dichotomy repeatedly in this volume, and there is no need to rehearse all of the arguments here. It is worth noting, however, that Diakonoff’s two-sector model has been recommended to specialists in the Bronze Age Aegean by Benjamin Foster (1987). Foster, like Diakonoff and Liverani, argues that the Late Bronze Age “palace economy” is defined by the structural opposition between “the community of free citizens” and “the king’s people” (p. 12). Foster does not defend this in detail, relying instead on Diakonoff’s own work on the subject. But when this model and its implications are examined carefully in light of all the evidence—especially the evidence from Ugarit and its neighbors—it is shown to be highly questionable. It is perhaps a gift from the Near East of which students of Bronze Age Greece should beware.

Hittite “Feudalism”

The powerful kingdom of Ḫatti in central Anatolia, to which Ugarit was subservient for the last 150 years of its existence, was greater in scale than neighboring Aegean kingdoms to the west, but it had a similar kind of redistributive “palace economy” managed by a large royal establishment. This palace establishment was concentrated at the capital city Ḫattuṣa on the north-central Anatolian plateau, with regional branches in various secondary palaces. Hittite society and government, led by an Indo-European-speaking elite, had many distinctive features not found elsewhere in the Near East; yet it seems clear that the Hittite king headed a relatively centralized patrimonial regime of a type well known in Bronze Age Syria and Mesopotamia. There are major gaps in our textual sources concerning the internal administration of Ḫatti; nonetheless, enough evidence exists to support the view that Hittite rule was legitimated and implemented according to a model of political authority based on personal “household” relationships rather than an impersonal bureaucratic model. As elsewhere in this period, the entire state and its administrative apparatus was regarded as the king’s “house” and royal officials were the king’s dependent servants or, at the highest level, his “sons.”

Having said that, there is no need to conclude that the personalized nature of Hittite government meant that the Hittite king was weak or had to delegate his authority to powerful nobles. The king closely supervised the activities of his subordinates and was called on to make decisions about even quite minor matters. In his survey of “Royal Ideology and State Administration in Hittite Anatolia,” Gary Beckman (1995: 539) notes that “all Hittite officials were bound by an oath to the person of the king, to whom they owed their appointment, directly or indirectly.” There was no feudal-style decentralization of political authority diffused through a hierarchy of vassal relations (at least not within Ḫatti itself, as opposed to the relationship between the Hittite king and his vassal kings and their subjects within the broader Hittite empire).

For this reason, it is necessary to qualify the description of Hittite society as essentially “feudal” that

82 Foster also distinguishes between Diakonoff’s two-sector model and the model of the Asiatic mode of production, which he rejects (p. 15), although other proponents of the two-sector model (e.g., Liverani and Zaccagnini) recognize that these are essentially the same.

83 See the overview of Hittite government in Beckman 1995, and the detailed study of Hittite administrative texts in Siegelová 1986. I am grateful to my colleague Theo van den Hout for drawing the latter work to my attention.
was proposed by earlier Hittitologists such as Hans Güterbock (1954) and Albrecht Goetze (1964). This term is misleading, for it is not decentralized feudal political relations that should be stressed, but rather economic relationships based on a “manorial” system of land tenure in the context of patrimonial political relations, which in this case entailed a quite centralized mode of government (see the discussion of manorialism versus feudalism in chapter 9.1 above). We can agree in substance with the picture of Hittite society that is intended by the use of the term “feudal,” but that picture is more accurately conveyed by Weber’s patrimonial household model, in which the king is not the suzerain of free vassals but the master of a house staffed by nonfree servants (slaves), whose own freedom of action might be quite limited.

Some scholars, however, in rejecting the feudal interpretation of Hittite society, do not simply question the suitability of this term, with its implication of political decentralization, but have a more deep seated objection to the basic model of society it conveys. Thus Igor Diakonoff published a long article in 1967 in which he rejected the feudal interpretation and argued for the application of his two-sector model to Hittite society (Diakonoff 1967; also Diakonoff 1982:85ff.). The two-sector model assumes that a bureaucratic “state sector” existed in opposition to the village-based “free communal sector,” and Diakonoff argued that this distinction is reflected in the Hittite Laws, for example, in the terms LÜ ÍLK, “serviceman” (i.e., a state dependent) and LÜ ÊLLU, “free man” (see Diakonoff 1982:86). This was part of his attempt to show that the two-sector mode of production characterized the Bronze Age Near East as a whole, as opposed to the feudal (and patrimonial) model of a unitary social hierarchy.

In terms of the classical Marxist theory of social evolution, the two-sector or “Asiatic” mode of production constitutes an intermediate stage between primitive communual and ancient slave society (see my discussion of this in chapter 9.2). Thus, apart from his appeal to the evidence, Diakonoff rejected the feudal model on theoretical grounds, because historical materialism holds that feudalism developed only later, out of slave society. Feudalism is seen as a total mode of production characterized by the ownership of the means of production on the part of exploited producers (i.e., “serfs”) who are personally dependent on their exploiters, whereas in the earlier two-sector (“Asiatic”) mode of production, producers are either dependent slaves without means of production who are employed within the “state sector,” or they are independent (and largely rural) proprietors in the “free communal sector.” Diakonoff’s approach to Hittite society was subsequently taken up by Alfonso Archi (1973; 1977), who explicitly identifies the two-sector model with Marx’s concept of the “Asiatic mode of production” (AMP). The two-sector model of Hittite society is also employed by Fiorella Imparati (1982) and Horst Klengel (1986), among others (cf. Haase 1983). Imparati argues that the two-sector mode was beginning to break down during the period of the Hittite empire (the “New Kingdom”) as the Hittite village sector became less autonomous and was increasingly subordinated to the palace, a modification of Diakonoff’s model that is similar to what Liverani has suggested for Syria-Palestine.

This is not the place for a detailed rebuttal of the two-sector model of Hittite society and a corresponding defense of the patrimonial household model. But it is worth suggesting that the Hittite legal and administrative texts which originally led to the notion of Hittite “feudalism” are most plausibly interpreted as indicative of a unitary hierarchical system of dependent land tenure (“manorialism”), in keeping with the patrimonial model. My detailed criticism of what I see as the erroneous application of the two-sector model to the economic data available from Hittite vassal kingdoms such as Ugarit and Mukiš—regarded by Liverani and others as parade examples of this model—casts doubt on the existence in Ḫatti itself of a communal sector of free village landowners separate from the palace sector. In the absence of clear evidence for such a structural dichotomy, in Ḫatti as elsewhere, we must conclude that it is simply an a priori deduction from the Marxist model.

As for the urban-rural economic dichotomy discussed at some length above in chapter 6, there is little evidence for such a dichotomy in Hittite Anatolia. The Hittite imperial capital of Ḫattuša (modern Boğazkale or Boğazköy)—vast as it was in its final architectural phase, when its walls encircled an area of some 160 ha—had a modest population of perhaps 10,000–15,000 inhabitants (see Mora 1977), not much greater than that of the far smaller 20- to 25-ha city of Ugarit. Ḫattuša was a special regal-ritual center largely occupied by palaces, temples, and tombs. Here is a case in which the scale of urban architecture and fortifications has little relation to population size or any other economic factor, but rather was motivated by ideological and political-military concerns that had little to do with our notion of economic efficiency.

Indeed, in Bronze Age central Anatolia, as elsewhere, it was singularly inefficient to move staple commodities over great distances to feed an economically specialized urban “central place,” in view of the prohibitive expense (in terms of the food
required by draft animals) and slowness of overland transportation where roads are poor and technology is primitive. As a rule, the widely scattered Hittite cities in Anatolia must have remained locally self-sufficient in most commodities, which implies that the integration of the Hittite domain was political and military, not economic.

This is not to deny that imperial capitals such as Ḫattuša received staple goods as tribute from distant places, regardless of efficiency. But the patently symbolic character of such inefficient imports merely confirms the point that the specialization characteristic of Ḫattuša and other political capitals of the period was not economic, in the modern sense, but rather political. Even the production of luxury goods by specialized urban craftworkers under royal or cultic patronage falls into this category of politically, as opposed to economically, motivated specialization, for such goods (textiles, metalwork, etc.) did not circulate freely within a depersonalized market system, but rather moved within a personalized, reciprocal or redistributive gift-exchange network in which they had an explicitly political meaning, as I have argued above in chapter 4.5.

In Hatti, as elsewhere in the Bronze Age Near East, we cannot speak of an economic system based on the structural distinction between urban craft production and rural agricultural production, even though a few political and cultic centers such as Ḫattuša might have been exceptional in the degree to which their inhabitants were devoted (for political and religious reasons) to specialized, nonagricultural occupations, and so relied on commandeered imports, as opposed to subsisting on their own local agricultural production. But we do not know what proportion of the populace in Ḫattuša—unusual as it was—were involved in farming the land around the city. In the next chapter, I will argue on archaeological evidence for this patrimonial hierarchy in various periods in ancient Egypt.

In general, householders in Egypt imitated on a smaller scale many of the attributes of the supreme ruler and his household; thus pharaonic Egypt had a “segmentary” structure, consisting of a hierarchy of households within households with little functional differentiation at each level. This phenomenon has been aptly named “The Fractal House of Pharaoh” by Mark Lehner (2000), who discusses the textual and archaeological evidence for this patrimonial hierarchy in various periods in ancient Egypt.

The essentially personal, patrimonial character of ancient Egyptian administration explains why the elaborate and numerous administrative titles conferred on royal officials defy rational analysis. As in other patrimonial regimes, official titles are more an index of the closeness of their bearer to the ruler than a description of a rationally delimited set of responsibilities. Indeed, the evident effectiveness of Egyptian administration should not mislead us into thinking that it consisted of a rationalized bureaucracy. Barry Kemp (1989:235) notes that: “The workings of the Egyptian administration at its various periods are reasonably apparent, but they seem not to be the product of an abstract concept of administration elegantly applied across a broad spectrum of activities.” He goes

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84 This idealization of the father did not preclude an important practical role for women and grown sons in household management, however, as Assmann notes (1991: 101f.). But even if reality often did not conform to the ideal, the symbol of fatherhood was nonetheless constitutive of Egyptian society.

85 It is clear that Strudwick (1985), for example, has difficulty discovering any real system in the use of titles by Old Kingdom officials. He acknowledges that “promotion” was largely due to the ruler’s personal favor toward particular persons (p. 342).
on to cite the Edict of Horemheb (ca. 1320 B.C.), which demonstrates that “ancient government consisted of an accumulation of individual institutional arrangements of very restricted scope” (ibid.). The royal decrees in this edict “imply that there was no codified system of revenue collection as we might understand it. In its place there was a range of individual practices sanctified by tradition” (ibid., p. 236).

Kemp’s work at the site of el-Amarna, in particular, provides archaeological evidence of patrimonial social and economic organization in the New Kingdom (see Kemp 1977a; 1981; 1987; 1989:261–317). Amarna is the site of ancient Akhetaten, the capital of New Kingdom Egypt under the “heretic” pharaoh Akhenaten. As a short-lived city founded by an unusual pharaoh who introduced many innovations, Amarna is perhaps not entirely typical of Egyptian settlements of the period. But the nature of Egyptian archaeology is such that we have little other evidence to go on, and certain basic economic and architectural features of residential districts of Amarna were probably to be found in other Egyptian cities. In particular, there is a striking lack of an urban-rural dichotomy. The houses of royal officials in the suburbs of Amarna reveal that these officials were also farmers whose main source of sustenance came from land that was granted to them by the king. The Amarna houses were substantial agricultural establishments whose size suggests that they were inhabited not just by the immediate family of the owner but by other kinsfolk and slaves, who no doubt did most of the work. Furthermore, there is evidence that smaller, poorer houses tended to cluster around a wealthy house, providing an architectural indication of a patrimonial hierarchy of dependency. Pottery-making and other basic manufacturing activities were not concentrated in special industrial zones but were performed within individual private households. Each household was therefore largely self-sufficient, while at the same time being integrated into a complex hierarchy of patronage and dependency at whose apex stood the house of the pharaoh.

The largest houses at Amarna were surrounded by walled compounds within which were typically grain silos, a well, a garden with trees, kitchens, workshops or storage sheds, stables, a shrine, and a separate smaller dwelling (perhaps for a steward or a married son). These compounds stood along the main streets where they could be entered by chariots, but there were also smaller houses, without compounds, many of which abutted the walled compounds of larger houses. The smaller houses appear to have been built over time in piecemeal fashion, gradually filling the space between the large houses. Furthermore, the inhabitants of the smaller houses must have made use of the wells and silos of their wealthier neighbors because they had none of their own. Kemp (1977a: 133f.) quite plausibly suggests that: “The fact that the houses were built in an agglomerative way instead of individually in the considerable space available, and the hint of shared facilities, strongly implies that the occupants shared some common background and ties. Social anthropology supplies an obvious explanation: a house group like this accommodated an extended family.”

The Amarna dwellings were not tightly packed courtyard houses of the type found in walled Mediterranean cities. A great deal of space was available inside the city limits—indeed, much open space still remained when the short-lived city was abandoned—and this permitted the construction of sizable, well-spaced compounds by the leading inhabitants of the city. But the domestic compounds of Amarna were functionally equivalent to the urban courtyard houses found elsewhere; furthermore, the clustering of the smaller houses of clients around the houses of their patrons, and the shared use of facilities such as wells and silos, are well known in Mediterranean cities of various periods.66 At Amarna itself there are signs that the gradual filling in of open space would eventually have resulted in the familiar “organic” pattern of narrow, winding streets and closely packed dwellings, broken only by the main thoroughfares. As Kemp (1989:294) says: “Instead of a grand unitary design we find a few broad but far from straight streets running more or less parallel to the Nile to join the suburbs to the centre, while narrower streets cross at right-angles. The overwhelming impression is of a series of joined villages. The individual house plots interlock in complex patterns, creating distinctive neighbourhoods. Sometimes groups of larger or smaller houses occur, but the two types are often intimately mixed. Within them rich and poor lived side by side.”

Kemp has demonstrated that the “extended-family” house-groups of Amarna practiced agriculture as their principal means of support, noting that “the larger Amarna houses look like little farms” (Kemp 1989:309). The grain silos found in domestic compounds at Amarna are quite large and probably had sufficient capacity to store the entire yield of the annual harvest from the house-owner’s landholdings. The amount of grain that could be stored by the inhabitants of the largest houses was therefore far in excess of their own subsistence needs, and so constituted a fund that they could use to support their

66 See, e.g., Heers 1977:129–68, on medieval Europe. The clustering of small houses around larger houses at Ugarit is discussed below in chapter 13.2.
neighboring retainers and to purchase goods and services unavailable within their own house-group.

It is striking, therefore, that the owners of the larger houses at Amarna were royal officials and craftsmen, whose names and titles are actually known in some cases. We might have expected these royal personnel and their families to have depended on rations provided by the pharaoh, following the common view of the ancient Egyptian economy as a huge redistributive apparatus. But Kemp points out that the ideal for an Egyptian official was not bureaucratic promotion and public compensation, in the modern sense, but a life of comfort on his own prosperous and self-sufficient farm. Whatever his other duties, a wealthy official at Amarna was first of all a farmer who managed his own estate, which was his principal source of support, although his kinsmen and clients no doubt performed much of the actual labor.

Many manufactured items, especially pottery and textiles, were also produced within private households, which were therefore self-sufficient in most goods. Some households, of course, engaged in specialized craftwork, which presumably provided income in addition to what was received from agriculture (Kemp 1981:84ff.). Evidence of this is provided, for example, in the debris found in the houses of sculptors, in one of which excavators found the famous painted bust of Queen Nefertiti. Regardless of administrative or craft specialties, however, the archaeological data from Amarna indicate that there was no strict dichotomy between “urban” and “rural” modes of life in New Kingdom Egypt.

The patrimonial picture of Egyptian society provided by Amarna accords with Janssen’s (1975) thesis that the economy of Egypt during the New Kingdom was characterized by what Polanyi called “redistribution” and “reciprocity” rather than by market exchange. In other words, ancient Egyptian economic transactions were normally embedded within personalized social relationships patterned on the household model. Moreover, the redistribution of goods was done not just at the highest level of the social hierarchy. The essentially agrarian character of the households at Amarna, even those of royal officials and occupational specialists, suggests that the redistribution of basic commodities was not just a feature of the great households belonging to the pharaoh and the gods, in which rations were given to their dependents. This kind of redistribution was replicated in similarly structured but much smaller households at various levels in the social hierarchy.

The implication is that, in this period at least, Egypt did not have a simple two-sector economy consisting of a “state sector,” characterized by the distribution of rations to royal dependents, over against a semi-autonomous “village sector.” Rather, as Kemp (1989:316) argues, “at Amarna the more or less continuous gradient of house size and other status indicators . . . as well as the interlocking horizontal pattern of houses of different sizes, suggests a pattern of economic relationships of some complexity. The residential parts of the city look for all the world like an aggregate of villages and neighborhoods, each with its many layers of dependence, economic and otherwise.”

Personalized patrimonialism as opposed to rationalized bureaucracy is also evident in New Kingdom Egypt’s administration of its Asiatic empire. In his study of the relations between Egypt and Canaan, Donald Redford (1992:199f.) concludes that “the administration of the new empire was created on an ad hoc basis [in the 18th and 19th Dynasties], with little imagination beyond a simple response to a practical need” (cf. Hachmann 1982). Redford points out that the Amarna letters give no indication of clearly demarcated imperial “provinces” in Canaan of the later Roman type, each with its Egyptian governor. Instead, a local ruler, called a ḥazanmu in Akkadian, was installed in every city, and a roving Egyptian inspector or courier, called a MAŠKIM (Akk. rābiṣu, Canaanite sōkinu), made his rounds and reported back to the pharaoh.

As Redford himself puts it:

The spheres of operation of these [Egyptian] officers were constantly shifting on an ad hoc basis. . . . Because of his familiarity with one part of the country an officer’s activity might be restricted to one region only, but the reason was a private one and had nothing to do with any prior division of the territory. When Egyptians alluded to the northern empire, they still spoke of “the land of Canaan (or Kharu, Djahy, etc.)” and the cities therein each with its own “territory,” never of “the province of so-and-so.” The officer is dispatched by the king on assignment and is allotted a certain number of towns in Canaan on a circuit. These he visits on his rounds, and while there exercises wide authority. He bears letters from Pharaoh, can arrest locals and convey them to Egypt, and can requisition dues and taxes; he decides cases at law and can even settle border disputes between one town and another. Sooner or later he always returns to Egypt for debriefing and consultation with the king or his officials. . . . From the voluminous records of the 19th and 20th Dynasties, it is quite clear that the provincial administrator of the time was primarily a courier (“king’s messenger to foreign lands”) who carried the king’s messages and reported back to him. [Redford 1992:201f.]

A few strategic cities were garrisoned with Egyptian troops, but the administration of the pharaoh’s
Models and Evidence

Domain was left largely in the hands of a native elite, with the stipulation that the ḥazannu of each city was responsible for the provision of the goods and services that the Egyptians required. This simple system is similar to the fiscal and administrative organization within smaller kingdoms, such as Ugarit, where ḥazannu was the term for a village headman or mayor and the MAŠKIM (Akk. ṛabisu, but probably read as sākinu at Ugarit) was a royal official who monitored the king’s domain. Again, there is a “fractal” similarity of structure across scales of measurement, which is produced by the application of the same basic political and economic relationships in diverse contexts.

This survey of patrimonial society in the Bronze Age Near East has necessarily been quite selective. Nonetheless, the cumulative effect of the diverse cases assembled in this chapter is to call into question the structural dichotomies between public and private, urban and rural, slave and free, palace and village, that underlie many sociohistorical reconstructions, including the two-sector model which has been so widely applied to Bronze Age societies in recent decades. I have tried to defend a more unitary model based on the ubiquitous native conception of the social order as a collection of households held together by the great household of the ruler, and ultimately of his god. This reflects my theoretically based methodological choice to give precedence to the native understanding of political and economic relationships rather than analyzing these from an external point of view on the basis of functionalist or materialist assumptions. The key requirement for any durable political entity is the effective and reproducible legitimation of political authority among those who must obey. This was accomplished throughout the Bronze Age Near East—in a strikingly similar manner in many different places—by the use of the symbol of the house of the father and the related metaphors of sonship and servanthood. The flexible “root metaphor” of the patriarchal household served to integrate the traditional practices of everyday life in agrarian communities with a wider political, indeed cosmic, realm that transcended local face-to-face relationships.

Of course, there was considerable variation in different times and places in the detailed outworking of this vision of the social order. Some patrimonial regimes were governed in a more centralized fashion than others because of predictable functional factors (e.g., the effect of geography on the maintenance of military control) or unpredictable personal factors (e.g., the political and military skill of the ruler). But these variations should not be allowed to obscure the remarkable power of the simple household metaphor to provide a common framework for Bronze Age civilization.
Chapter 13. House Plans and Neighborhood Organization

There are two implications of the patrimonial household model (PHM) that can be tested against the archaeological evidence available from the site of Ras Shamra, ancient Ugarit. The first implication is that there existed in the kingdom of Ugarit the kind of structural homology between “center” and “periphery” that is characteristic of patrimonial society; in other words, the lack of a strict dichotomy in socioeconomic organization between the “urban” and “rural” components of society.1 This structural homology ought to be reflected in the subsistence strategies and household organization found among city-dwellers in Ugarit, for whom agriculture should have been a principal means of livelihood and for whom relatively large joint-family households and other forms of kin-based co-residence should have been the ideal. The archaeological evidence that supports these predictions is discussed in this chapter, where the material from Ugarit is interpreted in light of comparative historical and ethnographic data.

The second implication of the patrimonial model, which is related to the first, is that center and periphery in the kingdom of Ugarit were integrated by means of manifold personal ties of patronage and dependency, patterned on the unequal household relationships between fathers and sons or masters and servants, rather than constituting two opposing social groups that were related only through the impersonal juxtaposition of a unitary “palace sector” on top of a semiautonomous “village sector.” As we have seen in chapters 10 and 11, the legal and economic documents from the kingdom of Ugarit contain some evidence of a unitary hierarchy of households in which political authority and economic exchange were realized through dyadic personal relationships between social superiors and their dependents at each level rather than being focused in the single node of the palace. The royal household stood at the apex of this hierarchy, of course; thus every householder in the kingdom was ultimately a “palace dependent” who took his place within a single pyramidal structure. But the overall socioeconomic organization of Ugarit (and many other similar polities), although structurally and conceptually encapsulated within the household of the king, was probably more decentralized than first appears. This is so because the ruler would have found it more feasible to exercise his administrative control—impressive as it was in many respects—through a limited number of leading householders rather than through the individual supervision of thousands of persons at every level. In Eisenstadt’s terms, the elite was “embedded” in traditional social relationships of domination and subordination, rather than being an “autonomous” social entity. As a result, the structure of authority within the capital city and the royal palace (the social center) mirrored the traditional authority structure found in towns and villages throughout the kingdom (the periphery).

There are archaeological indications of this hierarchy of households on the local neighborhood level. Clusters of houses share facilities (including tombs) and vary in size in a way that suggests urban “clans” or patronymic associations organized around leading householders (see chapter 6.2 and 6.3). Such clans function as extended, composite households whose members understand their physical proximity and economic cooperation in terms of common ancestry. The component parts of a composite household need not occupy separate dwellings, however. The largest residences at Ugarit, located near the vast royal palace, were produced by the amalgamation of smaller houses and measure hundreds of square meters in area. These were probably inhabited by the highest ranking householders, together with their families, their retainers, and their retainers’ families. This sort of arrangement is not surprising because large composite households occupying a single large dwelling are known among the elite in many Mediterranean societies.

These predictions of the PHM contradict those of the two-sector or “Asiatic mode of production” (AMP) model, discussed above in chapters 9 and 11,

1 “Center” and “periphery” are used here in the sense in which they have been employed by Shils (1975) and Eisenstadt (1993); see chapter 3.1 above. For Shils, the social “center” is “the center of the order of symbols, of values and beliefs, which govern the society” (Shils 1975:3); thus the terms “center” and “periphery” do not necessarily imply spatial separation. For Ugarit, however, it can be argued that the social center was focused in the physical center of the kingdom at Ras Shamra, which appears to have been the main locus of administration, of ritual, and of literary activity. In this case, then, the distinction “urban-rural” is more-or-less synonymous with “center-periphery.”
which assumes that urban and rural modes of livelihood and social organization were fundamentally different, and that individual villagers experienced higher authority only in the form of an impersonal bureaucracy whose agents collected taxes and supervised corvée labor.\(^2\) Mario Liverani, for example, argues that the Levantine “urban revolution” of the third millennium B.C. resulted in the emergence of a specialized population resident in the new cities which grafted itself on top of the unspecialized rural population and extracted its surplus to support itself (Liverani 1975:156–64; 1979c:1342–48; 1984).\(^3\) In his view, the innovative urban population was characterized from the beginning by nuclear families and an individualistic ethos, unlike the rural population, which retained its traditional kin-based social structure and its relatively low level of social and economic differentiation, being solely concerned with basic production and reproduction. According to the two-sector model, the fundamental difference between the two groups lay in the lack of ownership of the means of production on the part of the urban specialists, who were royal dependents and subsisted mainly on rations. The rural population, on the other hand, owned the means of production (i.e., land and animals) and therefore constituted a highly traditional and self-sufficient “free” sector that was exploited only indirectly and impersonally by the city-based ruling elite.

Liverani suggests that the two-sector system had begun to break down by the end of the Late Bronze Age as rural kinship gradually withered and nuclear families and individual property became common even in the villages, which had originally been characterized by inalienable and indivisible communal property. In a sense, therefore, center and periphery were becoming homologous, from Liverani’s point of view, as the rural population absorbed the newer urban ethos. But this kind of homology is antithetical to that pictured in the PHM, in which the city-dwellers themselves never departed from the original “rural” kin-based pattern. For Liverani, however, nonagricultural nuclear-family households predominated in the city of Ugarit from its inception until its demise.\(^4\) As he puts it:

La famille est partout nucléaire. Elle donne peu d’importance à des liens plus étendus. . . . La famille paysanne vit sur un moyen de production—la terre—qui est partageable entre les différents héritiers et ne demande pas de compétences individuelles. Le dépendant du palais, spécialiste d’une technique particulière, vit au contraire lié à un “poste” dans l’administration, qui lui est assuré par ses capacités personnelles et l’instruction reçue, par la bienveillance royale et par son comportement social. [Liverani 1979c:1344; see also cols. 1318–21]

The evidence does not support this interpretation, however, as we shall see.

1. Household Size and the Use of Space

Since 1929 French archaeologists have conducted more than fifty campaigns of excavation at Ras Shamra, and archaeological exploration there continues unabated.\(^5\) More than 6 hectares of the 22-hectare tell have been uncovered, providing an excellent picture of the layout of the city of Ugarit just before its destruction at the end of the Late Bronze Age, early in the twelfth century B.C. (see figure 18 in chapter 10.2). The remains of ancient Ugarit are particularly revealing because the city was destroyed suddenly and was never extensively resettled; moreover, later quarrying for building materials was not as widespread as it might have been, so the stone wall foundations are quite well preserved and artifacts are often found where they were abandoned.

\(^2\) The hypothesis of an urban-rural dichotomy is not peculiar to the two-sector (AMP) model, of course, but is common to all theories of ancient Near Eastern urbanism that regard even the earliest cities as centers of a qualitatively different form of social life rooted in functional specialization and bureaucratic administration. This functionalist view of ancient Near Eastern urbanism is criticized in chapter 9.3 above.

\(^3\) See chapter 11.2 above. Michael Heltzer is less explicit than Liverani on this score, but it is clear that the same view of Bronze Age urbanism underlies his work on Ugarit (e.g., Heltzer 1976; 1982), discussed in chapter 11.1.

\(^4\) Liverani (1975:148) suggests, in passing, that “free” peasants also lived in the capital city, which in some respects was therefore also a “village.” But elsewhere he states explicitly that: “Les maisons privées d’Ugarit appartiennent pour la plupart à des fonctionnaires palatins et autres spécialistes . . . .” (Liverani 1979c:1318). Moreover, his calculations of the population of the city of Ugarit, in which he compares an estimate obtained from administrative lists of specialists with an estimate based on the total residential area, assume that most urban residents were nonagricultural palace dependents.

Unfortunately, however, the finds have not been fully published (see the comments in Dornemann 1981). No complete inventory of pottery and other small artifacts, with their findspots, is available for most of the buildings that have been uncovered. Many of the plans published before the 1980s are excessively schematic, and for some buildings no plans are available at all. Under the supervision of Claude Schaeffer, who directed the Ras Shamra excavations until 1971, the archaeological team’s focus apparently was on the most impressive artifacts—the finest pottery, the ivories, metal vessels, iconographic finds, and, of course, the tablets and other inscribed objects—rather than on collecting and publishing all of the material remains, or even a representative selection of them. Faunal and botanical evidence has rarely been published, for example, and perhaps was not collected systematically before the 1980s. Furthermore, only a limited sample of the locally produced pottery (as opposed to imports) has appeared in the reports of the excavations at Ras Shamra.

In general, archaeological excavation and publication at Ras Shamra has been architecturally oriented rather than being concerned with a detailed analysis of debris layers and their contents. The emphasis on the large-scale mapping of buildings is perhaps not surprising in view of the excellent state of preservation of the well-built stone wall foundations of ancient Ugarit and the temptation to uncover as many of them as possible. Indeed, the rapid clearance of large areas was common practice at such sites in years past, and few sites contain as many spectacular architectural and artifactual finds as have been retrieved at Ras Shamra. But after seven decades of excavation we know much less than we might about even the magnificent palace and temples, which were the center of attention during the early years of archaeological work at the site. As for the private dwellings, they have been relatively neglected until recently. Many details have not been published and much that we would like to know was apparently not recorded in the first place. Ordinary residential areas, in particular, were neglected, and the consequent dearth of detailed information concerning the more mundane aspects of daily life in ancient Ugarit hinders attempts at reconstructing room functions and patterns of economic activity in general.

Nonetheless, a substantial amount of archaeological information is available from Ras Shamra, especially with respect to the architectural plan of the city, almost a third of which has been exposed. Furthermore, the quality of the fieldwork and the publications has improved since the 1970s, especially after Marguerite Yon took charge of archaeological work at the site in 1978. Under her direction, attention was focused on domestic architecture, both in previously excavated areas and in newly opened residential areas near the center of the tell (the “Centre de la Ville”) and near its south-central edge (the “Sud-centre”), and the results of this work have begun to appear in print (see Yon 1992b for a summary of the aims and results of the Ras Shamra excavations since 1978). In addition to regular preliminary reports in the journal *Syria*, a few longer treatments of the private dwellings of Ugarit have appeared in recent years. In 1983 Olivier Callot published a detailed study of house design and building techniques in Late Bronze Age Ugarit based on the remains of one particularly well-preserved house in an area called “Ville Sud,” which had been excavated in 1959–60 (Callot 1983 = RSO 1). This monograph was part of Callot’s comprehensive reanalysis of the entire Ville Sud, the results of which were published in 1994 (Callot 1994 = RSO 10). Meanwhile, Yon and her colleagues published a volume in 1987 describing several houses that they had uncovered since 1978 in the Centre de la Ville (Yon et al. 1987a = RSO 3). The latter publication is significant because, in addition to architectural plans, ordinary small finds were published for the first time in reasonable detail, facilitating the reconstruction of room function and the use of space. In 1991 another volume appeared that contains descriptions of the stone tools and other stone objects found at Ras Shamra since 1978 (Yon et al. 1991 = RSO 6). In this chapter, I will focus on the residential areas of Ugarit that have been excavated and studied most recently, in the Ville Sud and Centre de la Ville, because the houses there are described in much more detail in the published reports than are the previously excavated houses elsewhere on the tell.

Unfortunately, the botanical and faunal remains from the recently excavated areas of the site have not yet been published. Furthermore, the pottery and other small finds are listed simply as “matériel représentatif” for each room or alley and are not pinpointed on the plans, raising questions about the precise distribution of the finds and the completeness of the published assemblages—and no doubt reflecting the still-preliminary nature of the extant reports. But enough has been published from the recent excavations to permit interpretations concerning economic activity and the use of space in the private dwellings of Late Bronze Age Ugarit. Moreover, although only a small number of houses have so far been excavated carefully and published adequately, the conclusions drawn from this limited sample are valid for the city as a whole (as the current excavators of the site have suggested) because the many other more sketchily
published houses uncovered throughout the site in earlier years clearly share the same characteristics.

In previous chapters I have discussed other Near Eastern and Mediterranean examples of urban households and neighborhoods in ancient Greece, Italy, Palestine, Mesopotamia, and Egypt, and in the pre-modern Islamic world. These diverse examples exhibit a basic similarity that renders them mutually illuminating, for there is a common lack of a fundamental socioeconomic dichotomy between urban and rural modes of life and a corresponding preference for some form of kin-based coresidence—typically the agricultural joint-family household, and often also patronymic associations of neighboring households—both in walled cities and in smaller villages. The Iron Age Israelite settlements discussed in some detail in chapter 8 are particularly useful for comparative purposes because of their relative geographical and cultural proximity to Ugarit. The city of Ugarit was larger than these Israelite walled towns, but it exhibits a similar urban environment, including evidence of the predominantly agricultural interests of its inhabitants and a range of house sizes that indicates a preference for joint-family households.

The Houses of the “Ville Sud”

In 1983 Olivier Callot produced a detailed study of a large, well-preserved house in the Ville Sud, an area that was excavated in 1959 and 1960 (see figure 20 below). Callot’s subsequent analysis of the entire Ville Sud has greatly increased the quantity of published archaeological data on the domestic architecture of the site (Callot 1994). This excavation area is 190 m long and 30 m wide (5,700 m²). It runs through the middle of the city from north to south, spanning half the width of the tell, and it contains all or part of 37 houses that range widely in size and quality of construction. The Ville Sud is thus a representative sample of the domestic architecture of the site.

It is unlikely that future excavations or publications will substantially alter the conclusions that can be drawn from the Ville Sud, and from the data published on the smaller exposures in the Centre de la Ville and Sud-centre that have been excavated since 1978. Excavation in the Ville Sud revealed a network of narrow winding streets and blind alleys running between irregularly shaped blocks of houses or insulae, which are numbered 1 to 14 on the plan shown below in figure 20. Callot’s 1983 study focused on House A in Insula 6, a block that contained three other smaller houses as well (B, C, and D), as shown in figure 21. House A is roughly rectangular, with internal dimensions of ca. 9 × 22 m and an overall interior ground-floor area of ca. 200 m². It is larger than average and its builders made extensive use of labor-intensive ashlar masonry, but in many ways it is quite typical of domestic architecture at Ugarit, as Callot has noted. It has a small inner courtyard in which was a well (locus 10), and a subterranean vaulted tomb (locus 13) that was entered by way of a dromos whose opening is in a neighboring room.

Callot has reconstructed the original appearance of this house, as shown in his excellent drawings (see Callot 1983:22–26, figs. 18–26). He believes that it had at least two stories because of the thickness of the walls and the presence of a partially preserved stone staircase near the main entrance. Callot postulates stone-and-timber construction (of which there is clear evidence elsewhere on the site) and a rooftop terrace. He also points out that the water-supply system of House A was quite elaborate. Rainwater was channeled from the roof into the well through conduits cut into the stone border along the sides of the courtyard, and there was a second well near the main entrance. An important function of the courtyard was to provide daylight to the rest of the house, as Callot shows in a schematic plan (p. 48, fig. 27).

Unfortunately, few artifacts and none of the faunal or botanical remains from House A and other houses in the Ville Sud have been published, so we have to guess at room function based on ethnographic analogies from traditional Mediterranean urban settings.6 Sleeping quarters were probably on the second floor, and the roof provided additional well-lit space for work and other activities. The ground floor and courtyard were probably used for cooking and craftwork or other productive activities. Callot plausibly suggests that the most dimly lit ground-floor rooms, farthest from the courtyard, were used for storage. As for stables, there is not enough published evidence to make a judgment about their presence or absence in Insula 6, although there is evidence that they existed in other houses in the city, as I will show below.

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6 See Callot 1994:219ff. for a catalogue of small finds listed by insula and room. As the author notes, this list is fragmentary and lacks most of the ceramic and lithic finds. On the inadequacies of the collection and recording methods of the earlier excavators of the site, see Callot 1994: 185ff.
Figure 20. Plan of the “Ville Sud” at Ugarit (after Callot 1994:371, fig. 270)
Figure 21. Insula 6 of Ville Sud (after Callot 1983:10, fig. 3)
In his discussion of the activities carried out in the Ville Sud, Callot (1994:186f.) questions the earlier interpretation of this area as a specialized artisans’ quarter inhabited by metalsmiths, noting the lack of crucibles and the scarcity of slag. The original excavators had pointed to the discovery of 19 whole or fragmentary molds for making metal implements, but as Callot points out, these were not concentrated in any one location but were distributed among several different houses. Rather than supposing that there was a metalsmith’s workshop in each of these houses we can assume that ordinary householders had their own molds which they periodically took to metalsmiths elsewhere (probably outside the city, in view of the unpleasant pollution associated with metallurgy) so that new tools could be fashioned for them—tools meant for their own use rather than for sale (p. 187). On similar grounds Callot also dismisses the original excavators’ identification of two other workshops in the Ville Sud, one of a “graveur de cylindres” and the other of a “sculpteur de stèles,” because the artifacts in question were not found in large numbers in any one location.

In general, although there may have been specialized artisans at work in these houses, there is little evidence of this. Here is a situation in which, as Callot puts it, “les fouilleurs, entraînés par une idée préconçue, ont opéré des regroupements abusifs d’objets dans le but de démontrer à tout prix que ce quartier, véritable ‘souk’ de la ville, était entièrement voué à l’artisanat et au commerce” (ibid., p. 189). This preconception, stemming from the anachronistic notion of an urban-rural dichotomy, is overturned not just by the lack of direct evidence for specialized occupations and commerce, but by the abundant evidence of work related to agriculture in the Ville Sud and other domestic areas excavated at Ras Shamra, especially the production of olive oil (ibid., pp. 190ff.). Both textual and archaeological evidence indicates that specialized craft production must have occurred here and there in the city, but agrarian activities seem to have predominated in most households.

Household Size and Organization

How many people lived in the houses of the Ville Sud? Assuming a generous 10 m² of roofed living space per person, there was room for at least 17 inhabitants in House A of Insula 6, if we consider only the main living and sleeping quarters on the second floor. Tables 16 and 17 below list the roofed living space and potential population of the other houses in the Ville Sud and in the Centre de la Ville. The actual number must have varied over the lifecycle of each household, but the theoretical capacity of these houses has implications for the question of household organization. Callot (1983:73) suggests that House A was occupied by a single wealthy family (perhaps a polygamous one) that also had a number of servants. But this disregards the possibility that the larger houses at Ugarit were inhabited by joint families of the traditional Mediterranean type, together with their kin-related retainers and servants.  

Callot is influenced here by Mario Liverani, whose work he cites. Liverani has argued that nuclear families were the norm in Ugarit (e.g., Liverani 1979c: 1344; see also cols. 1318–21). I have previously discussed this hypothesis in the context of its central role in Liverani’s two-sector model of Ugaritian society. His assertion that the urban “palace sector” was characterized by nuclear families is a deduction from that model which is not supported by the archaeological or textual evidence. To be sure, Liverani does not ignore this evidence; indeed, he argues that it confirms his hypothesis. Based on the average size of the houses excavated at Ras Shamra (80–100 m²), he proposes a mean family size of 6–8, presumably because he assumes that there were 10 m² of roofed living space per person (Liverani 1979c:1319ff.). But a mean family size of this magnitude indicates a preference for joint families, not nuclear families (see chapter 7.2 above). Liverani accounts for this by suggesting that polygamy was frequently practiced, and he cites a Ugaritic text (RS 11.857, reproduced on the next page) which enumerates the members of households that apparently belonged to men from Alašiya (Cyprus), possibly expatriate merchants. According to Liverani, this text demonstrates the high incidence of polygamy within nuclear families resident in the capital city.

An examination of this text shows, however, that multiple wives are listed in only 5 out of 27 readable entries (lines 7, 11, 16, 18, 20), which is a rather low incidence of polygamy considering that these men, if they were foreign merchants, were probably much wealthier than average. And if the men whose families are described here were from Alašiya, their

7 The courtyard of House A occupies ca. 5×5 = 25 m²; thus the roofed living area was ca. 200 – 25 = 175 m² (see table 16 below). This is a minimal estimate, for an average closer to 8 m² per person (or even less) is more likely in a densely populated urban environment (see the discussion of this issue in chapter 8.6 above).

8 Although he does not fully explain his calculations, Liverani apparently assumes an average of 60–80 m² of upper-floor roofed living space, excluding courtyards averaging ca. 20 m² in area.
houses and joint-family households among the Alashiys themselves. The terms bn, “(adult) son,” and bt, “(adult) daughter,” are distinguished from gčr, “youth,” and pgť, “maiden” (WUS no. 2138 and no. 2246), which likely refer to unmarried adolescent children. Furthermore, in one entry (line 8) two nērmin are mentioned, while another entry (lines 16–17) mentions five nērmin (in the feminine plural); a nērmi was obviously the female equivalent of a nēr. Other Ugaritic texts show that the terms nēr and nērt denote male and female household retainers, respectively, rather than children. So what we seem to have here

9 Cutler and Macdonald (1976) have argued convincingly that nēr, in both Ug. and Heb., denotes a household retainer of relatively high status (a “squire” or “knight”), who was often a youth but not necessarily so (see also Stähli 1978). The nērmi appear as a military corps in palace administrative texts from Ugarit, and as retainers or subordinates of the maryanna-charioteers (cf. Eg. nērm; Rainey 1965c:21). In
is a distinction between adult children, adolescent unmarried children, and unrelated young men and women (servants or apprentices?) living in the same household.

The terms ǧṣr and ṣḏṯ might be thought to refer to the householder’s servants, like nʾr and nʾrt, as opposed to his children, were it not for evidence from other administrative census lists (discussed below) that the terms bn and bt specifically denote married offspring resident in their father’s house, thus creating a contrast with ǧṣr and ṣḏṯ as unmarried children. Moreover, ǧṣr and ṣḏṯ elsewhere connote youthfulness and virginity rather than servile status. In the story of Aqhatu, Damišlu’s beloved son is called aqht il ǧṣr, “Aqhatu the youth,” and his daughter is named Puṣātu (ḥt). Both are evidently unmarried, yet sexually mature. In the Baʿlu myth, the god Mūtu’s epithet is yld il ǧṣr, “Ilú’s beloved youth.” In literary texts from Ugarit, therefore, the social category indicated by the term ǧṣr is that of an unmarried youth who is the son, not the servant, of the household head. Being unmarried, the ǧṣr was well suited for warfare and adventure; hence the extended meaning “hero” or “soldier.”

The main distinction between a ǧṣr and a bn is that the former is an unmarried son whereas the latter is an older married son who thus favors a more settled life.

The use of ǧṣr and ṣḏṯ in the same text with bn and bt indicates that age-group categories, reflecting marital status, were used in this census. Martha Roth (1987:738–46) has discussed comparable age-group categories that are employed in Neo-Babylonian marriage agreements and in the Neo-Assyrian census of rural households called the “Assyrian Doomsday Book.” It is worth noting that a word written LŪ. NAR—probably to be read as Northwest Semitic *nāʾrū, “youth,” in this context, not Akkadian nāru, “singer,” according to Roth—is used in the Assyrian Doomsday Book to denote a household member who is not a member of the householder’s family. As she says, “it is highly improbable . . . that this unrelated person was a singer; he is more likely to have been a farmer hand, perhaps a youth from a neighboring farm that had manpower sufficient that his labor was not needed at home” (ibid., p. 746).

If we adopt this interpretation of bn and bt versus ǧṣr and ṣḏṯ in RS 11.857, it is significant that married daughters are mentioned for only three of the households (see table 15 below), which is to be expected because they normally lived with their husbands’ families. Moreover, households with married daughters (bt) did not have married sons (bn), and vice versa, suggesting that a son-in-law was brought into the house for lack of a son. Three households had married sons, giving a total of 6 out of 26 households (23%) that had married offspring in the second generation. Only one household had both adult and adolescent children (line 21); in this case, a married son (bn) and an unmarried daughter (ḥt). In other words, most householders had either adult or adolescent children, but not both, which is to be expected under premodern conditions of limited parental life expectancy and duration of fertility. If bn and bt alone refer to the children of the householder and ǧṣr and ṣḏṯ merely denote servants, as some scholars have thought, this aspect of the census would be difficult to explain, because one would expect a combination of both children (bn/ḥt) and servants (ǧṣr/ḥt) in most households.

It must be remembered that in RS 11.857, as in most of the other Ugaritic household census lists discussed below, the wives and children of married sons are not listed. Indeed, prepubescent children are not recorded at all, perhaps because they could not perform corvée or military service and so were not of interest to the palace. Only the patriarch’s wives and his adult or adolescent offspring are enumerated. Thus the households for which no children are listed may have included infants or young children of the household head.

For this reason, the mean number of persons per household computed from this text (3.6) is misleading. It is more useful to cite the proportion of households that had married children (bn/ḥt)—that is, joint-family households—which is approximately one-fourth. According to my interpretation, younger families, in the “nuclear” phase of the household lifecycle, inhabited the remaining three-quarters of the households. A quarter (6 out of 26) consisted of a single

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The Hebrew Bible, nāʾar can also refer to a young boy, but the usual term for a little child is yeled (fem. yaldā; cf. Ug. yld). Moreover, in Ugaritic texts nʾrm are men of military age. It is unlikely, therefore, that nʾrm and nʾrt in RS 11.857 refer to the householder’s youngest children, in contrast to the adolescent ǧṣrm and ṣḏṯ, especially since the former terms appear in only 2 out of 27 entries.

Miller (1970) argues that the primary meaning of ǧṣr is “hero” or “mighty” rather than “youth,” but this translation is awkward in the household census in RS 11.857. It is true that the Hebrew cognate ʿṣr (II) has a military connotation, but the earlier Ugaritic usage seems to emphasize the relationship between the ǧṣr and his father or household head.

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11 There are 29 households in the list but lines 14 and 15 are illegible and the entry in line 29 is excluded because it simply mentions 20 persons (ḥt) without any subcategories. A household of 20 persons probably had a joint-family structure, however, so if we include it we obtain roughly the same proportion (7 out of 27 = 26%).
conjugal couple who had no children (neither \(bn/\) \(bt\) nor \(g\)\(zrlp\)\(gt\)), or who had very young children who were not counted. Half (13 of 26) had unmarried adolescent children (\(g\)\(zrlp\)\(gt\)) and perhaps also additional prepubescent children. These proportions indicate age ranges and a mixture of nuclear- and joint-family households that accords with the Mediterranean demographic studies cited above in chapter 7.

Table 15. Household Members Listed in the Ala\(\text{s}\)\(iy\)an Census (RS 11.857)

<table>
<thead>
<tr>
<th>Household (n = 27)</th>
<th>Wives</th>
<th>Sons (adult or married)</th>
<th>Daughters (adolescent or unmarried)</th>
<th>Youths</th>
<th>Maidens</th>
<th>Retainers</th>
<th>Total (incl. head)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 (bt) krzn</td>
<td>1</td>
<td></td>
<td>1</td>
<td>3</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2 (bt) gg</td>
<td>1</td>
<td></td>
<td>1</td>
<td>3</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3 (bt) nwrd</td>
<td></td>
<td>1</td>
<td>2</td>
<td>4</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4 (bt) ydrm</td>
<td>2</td>
<td></td>
<td>1</td>
<td>3</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>5 (bt) [?]</td>
<td>2</td>
<td></td>
<td>1</td>
<td>4</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>6 (bt) ilsk</td>
<td>1</td>
<td></td>
<td>2</td>
<td>4</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>7 (bt) arzw[l]</td>
<td>1</td>
<td></td>
<td>1</td>
<td>2</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>8 (bt) iwrrzn</td>
<td>1</td>
<td></td>
<td>1</td>
<td>2</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>9 (bt) [?]</td>
<td>2</td>
<td></td>
<td>1</td>
<td>4</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>10 (bt) aup[]</td>
<td>1</td>
<td></td>
<td>2</td>
<td>2</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>11 (bt) tph[]</td>
<td>1</td>
<td></td>
<td>2</td>
<td>2</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>12 (bt) sk[n]</td>
<td>3</td>
<td></td>
<td>3</td>
<td>5</td>
<td>12</td>
<td></td>
<td></td>
</tr>
<tr>
<td>13 (bt) [?]</td>
<td>2</td>
<td></td>
<td>[1]</td>
<td>1</td>
<td>5</td>
<td></td>
<td></td>
</tr>
<tr>
<td>14 (bt) [?]</td>
<td>1</td>
<td></td>
<td>1</td>
<td>2</td>
<td>5</td>
<td></td>
<td></td>
</tr>
<tr>
<td>15 (bt) [?]</td>
<td>2</td>
<td></td>
<td>1</td>
<td>1</td>
<td>5</td>
<td></td>
<td></td>
</tr>
<tr>
<td>16 (bt) m[?]</td>
<td>1</td>
<td></td>
<td>1</td>
<td>1</td>
<td>4</td>
<td></td>
<td></td>
</tr>
<tr>
<td>17 (bt) hdmrd</td>
<td>1</td>
<td></td>
<td>2</td>
<td>4</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>18 (bt) sdq[]lm</td>
<td>1</td>
<td></td>
<td>2</td>
<td>4</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>19 (bt) rpi[]</td>
<td>1</td>
<td></td>
<td>2</td>
<td>4</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>20 (bt) alhn</td>
<td>[1]</td>
<td></td>
<td>1</td>
<td>3</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>21 (bt) tt</td>
<td>[1]</td>
<td></td>
<td>1</td>
<td>3</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>22 (bt) tr[]dgs</td>
<td>[1]</td>
<td></td>
<td>1</td>
<td>3</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>23 (bt) [?]</td>
<td>1</td>
<td></td>
<td>1</td>
<td>3</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>24 (bt) t[]fa[]a</td>
<td></td>
<td></td>
<td>20?</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>25 (bt) [?]</td>
<td>[?]</td>
<td></td>
<td>1</td>
<td>3?</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Total: 30 4 4 9 12 2 5 93

a Line 29 mentions only \(\text{g}\)\(zrlp\)\(gt\) \(bt\) \(t\), without giving individual counts of the different types of household members. A total of 20 persons is much larger than the total number listed for the other households, suggesting that this figure includes the families of adult sons or daughters; i.e., a sizable joint family. The total for this household has been excluded from the calculations of the grand total (93) because it is not comparable to the other totals given in the far right column.

Another Ugaritic text, RS 18.50 (reproduced below), also demonstrates the joint-family structure of Ugaritian households. In two entries (lines 4–5 and 10–12) the phrase \(\text{tn bnh } b\text{\(l\)}m\) appears, which should be translated “his two married sons” or “his two sons (who are) married men” (Gordon, UT Glossary, no. 493, p. 374). In three other entries (lines 1–3, 6, 7–9), a certain number of \(b\text{\(l\)}m\) are also recorded. A \(b\text{\(l\)}m\) here is a “husband,” a married man with a family of his own.

The joint-family household structure evident in this text is made especially clear in lines 1–3, where it is recorded that the household of a man called \(bn\) \(b\text{\(l\)}m\) consists of three married men (\(b\text{\(l\)}m\)) and their father-in-law together with his four daughters, i.e., their wives. The three \(b\text{\(l\)}m\) are not called the “sons” of the householder in this entry, as is usual in the subsequent entries. Rather, the householder is called \(ad\text{\(h\)}m\), “their patriarch,” and his four daughters are mentioned because they remained in his household after marriage (matrilocal residence). Presumably, the patriarch had no surviving sons of his own. The discrepancy between the number of husbands and the number of daughters can be explained by supposing either that one of the husbands had died or that one of them had married two of the daughters.
Each of these who belonged to a household would probably have had one or more wives and their “bull” patriarch and his four daughters. Thus the six husbands mentioned in each case apparently include the patriarch, or else they reflect a frère che of coresident brothers whose father had died. Each of these brlm would have had one or more wives and perhaps also some young children, although these are not recorded. Note that only four surviving wives are listed for the six brlm in lines 7, 8, and 9.

Heltzer (1976a:89f.) adopts a quite different interpretation of RS 18.50, however. In the first place, he assumes that rural, rather than urban, households are enumerated here. But only two of the six entries in this text identify a town other than Ugarit, so it is incorrect to assume that all of the persons listed were villagers. Furthermore, Heltzer translates brlm as “workmen” rather than “husbands.” This is unlikely: the noun brl is well attested elsewhere with the meaning “husband” or “family head,” but brl appears as a verb meaning “to work” (cf. Heb. pācal) in only a few, mostly mythological, texts, and never as a noun or participle with the meaning “worker” (see WUS nos. 544 and 546, pp. 54ff.). Given the context in RS 18.50, where household and family terms abound (e.g., “son,” “daughter,” “sister”), it is more likely that brlm here means “husbands.”12 Any workmen who belonged to a household would probably have been called bdm (“servants”), not brplm.

This interpretation is supported by three other Ugaritic texts (RS 11.778 = KTU 4.80:17, 13.298 = KTU 4.295, and 18.258 = KTU 4.417) which have been used by Heltzer and other scholars to determine the size and composition of Ugaritian households (Heltzer 1976a: 84–88; cf. Garr 1987:32–34). Heltzer notes that many of the same families are mentioned in these three documents, which have entries of the following form:

PN GN-y w X bnh w ḫṭḥ (w bnḥ) w Y alp w Z ṣin i.e., “PN of GN and his X sons and his wife (and his daughter) and Y oxen and Z sheep/goats.”

In two cases, a daughter-in-law (klt, lit. “bride”) is also mentioned, and in one instance three daughters-in-law are recorded (RS 11.778 = KTU 4.80:4, 13, 19). This indicates that the sons (bnm) enumerated here were married adults, as Heltzer acknowledges. A son-in-law (ḥtn) also appears in one entry (RS 11.778 = KTU 4.80:16–17); in this case, the patriarch probably did not have a son of his own, as Heltzer has suggested (unfortunately, the tablet is broken here, so the existence of a son cannot be determined). In one case, the brother of the householder is mentioned (RS 11.778 = KTU 4.80:10). Immature children and servants are not listed, however.

Although these three texts clearly deal with villagers, the household size and structure revealed in them is much the same as that of the urban households enumerated in RS 11.857 and RS 18.50. There was no fundamental structural difference between urban and rural households in the kingdom of Ugarit. Both were characterized by a joint-family structure in

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12 Dennis Pardee (pers. comm.) has suggested to me that the term brl in household census texts may be an abbreviation of brl ḫḥ, “owner of a woman,” i.e., “husband.”
which married sons continued to live with their parents or, lacking a son, a married daughter brought her husband into the household. The effects of mortality were such that on average only one or two sons would have survived to marry and start families of their own (see chapter 7.2 above), although this average conceals a wide variation, for households with as many as six conjugal couples are recorded in RS 18.50. A full joint-family household with two married sons probably consisted of 8–10 persons, excluding servants; that is, three conjugal couples (including the patriarch and his wife) together with one or two young children in each of the two sons’ families.

This configuration was normally short-lived, however, because in most cases the death of the patriarch would occur not many years after the marriage of his sons; thus at any given time at least two-thirds of all households would be in the nuclear phase of the household lifecycle, consisting of a single conjugal couple and their children, depending on the prevalence of households with coresident brothers whose father had died (frères). What is at issue here is not the number of persons per household, averaged over all households, for this average obscures the mixture of nuclear- and joint-family households. What is more significant for the structure of society is whether the proportions of households of different sizes indicate that postmarriage patrilocal residence (the joint family) was the ideal in Ugarit as in many other premodern Mediterranean urban settings. In my view, the textual and archaeological evidence clearly demonstrates a preference for patrilocal residence, reflected in the presence of three-generation joint families in a substantial proportion of Ugaritian households.

Figure 22. Plan of the “Centre de la Ville” at Ugarit (northern insula; after Yon et al. 1987a:6 and 12, fig. 1)
2. House Clusters and Shared Facilities

The recent excavations in the area known as Centre de la Ville provide further information concerning domestic architecture at Ugarit (see Yon et al. 1987a = RSO 3; Yon et al. 1987b; 1990a; 1991 = RSO 6; Gachet 1996). Several houses have been uncovered here, permitting an analysis not simply of individual house design but also of the organization of residential neighborhoods. The architectural remains have been assigned to various houses and streets by the excavators (note that a “house” here is defined simply as a set of interconnecting rooms rather than a free-standing structure); but only three houses (A, B, and E) have been published in detail. The excavators have identified all or part of seven houses (A–G), however, based on the positioning of doorways. Doorways are not always easy to recognize, especially in places where only the foundation courses of walls are preserved, but the detailed stone-by-stone plan included in RSO 3 (plate 6) bears out the interpretation shown above in figure 22. A street (locus 1038) borders houses A and B to the north. Another street (locus 1288) marks the boundary of houses B and C on the east. A blind alley (loci 1228 and 1208) leads north from the main street (locus 35), which runs across the middle of the excavated area.

The excavators comment on the architecture of this area in terms strikingly reminiscent of the later Islamic city (see chapter 6.3 above), noting the care with which domestic privacy is protected by the winding streets, the narrowness of the exterior doorways, and the absence of exterior windows (Yon et al. 1987c:118; see also Yon 1992b:27 on the lack of a master plan for the city). As in House A of the Ville Sud, the houses here probably had two stories and open courtyards. But the placement of the courtyards is unusually irregular in this insula; moreover, the houses vary widely in size. House A, for example, occupies 80 m²; house B, 120 m²; and house C, only 65 m². These sizes were not constant, however. The plan shown in figure 22 reflects only the final state of the buildings, at the time of the destruction of the city. The excavators also found evidence of considerable previous rearrangement of living space, achieved by blocking and unblocking doorways. It is clear that individual houses could expand or contract, depending on the needs of their inhabitants. In fact, the excavators suggest that there was originally a larger house here that was later subdivided. They point out that this kind of rearrangement was possible because the fundamental architectural unit was not the “house” as such but the insula, or block of houses, bounded by streets and alleys.

The “ideal” house at Ugarit possessed a well, a tomb, and an inner courtyard. In my opinion, the architectural remains in the northern insula of the Centre de la Ville do indeed point to the subdivision of a larger house (encompassing houses B and C, and perhaps A as well) that had its own well, tomb, and courtyard (locus 1064). The well and the tomb are now found in the portion of the insula that the excavators have called house B. But why was this house subdivided? The excavators suggest that an increase in population led to smaller dwellings and a decrease in per capita dwelling space. More specifically, Marguerite Yon has advanced the hypothesis that there was an influx of villagers into the city of Ugarit at the end of the thirteenth century B.C., caused by the social crisis that, according to Liverani, led to the collapse of the kingdom. Following Liverani, Yon argues that:

The palace distributed land, property, and revenues, and the king seems to have been more interested in the king’s men than in the freemen. At the end of the Bronze Age, Ugarit experienced a veritable social crisis: the king became increasingly associated with a single social group—the maryanni and other leading citizens . . . and could no longer be counted on to maintain social equilibrium. Many villagers flocked to the city in an attempt to benefit from royal favors, while villages that managed to resist the increase in taxes and tithes tended to organize their own affairs, detaching themselves as much as they could from royal authority. . . . This situation is visible in the archaeological record as a series of modifications in the distribution of space. [Yon 1992a:114]

The textual evidence does not support Liverani’s theory, however, as I have argued at some length in chapter 11. That theory rests on the two-sector model of Bronze Age Levantine society and on Liverani’s ideas concerning the breakdown of the two-sector system (i.e., the Asiatic mode of production) at the end of the Late Bronze Age. Furthermore, Yon’s hypothesis of an influx of people from the villages is not needed to account for the subdivision of the house in the northernmost part of the Centre de la Ville. What we have here is probably a division of family property that resulted in two or three separate households, all descended from a common ancestor. The members of these households continued to live as neighbors and continued to share the original family well and the family tomb—not to mention the cesspit located in the original courtyard (locus 1064). This courtyard, now part of house C, continued to provide light to house B, according to the excavators (Yon et al. 1987c:78; Yon 1992a:115). The largest dwelling produced by the division (house B), which
also possessed the well and the tomb, was probably inhabited by the senior member of the lineage. Moreover, his house was still large enough (up to 100 m$^2$ of upper-floor roofed area) to have accommodated a joint family of 10 persons, allowing a generous 10 m$^2$ of living space per person (see table 16).

The question also arises as to how villagers migrating into the city from the countryside would have acquired houses in Ugarit, because it is unlikely that there was a real estate market in the modern sense. It is hard to prove this one way or another from the textual evidence found at Ugarit, but second-millennium Mesopotamian evidence indicates that house ownership was bound up with existing kinship relationships, even in cities. Thus Marc Van De Mieroop (1999:274) expresses doubt, having surveyed Old Babylonian urban real estate transactions, “that there was any real estate market in the modern sense of the word in ancient Babylonia. Indeed, people owned their houses and could sell parts thereof, or perhaps even entire dwellings. But the sales were usually to relatives, mostly male siblings, or to neighbors [usually also kinsmen of some kind?] who wanted to expand their own property. It seems rare that someone moved into a neighborhood and bought himself a house, as we do today.”

The architectural modifications observable in the Centre de la Ville bring to mind ethnographic data concerning the fluid character of Near Eastern urban households and neighborhoods, which are defined by social relationships rather than by fixed architectural features. In traditional Islamic cities disparities in wealth and house size are often observed among neighboring households. The poorer households depend in various ways on the leading household of the neighborhood, which controls access to certain shared facilities. It is worth noting, therefore, that there was only one well (1071) and one tomb in the entire “northern insula” (houses A–G) of the Centre de la Ville (see figure 22 above). Both of these are in house B, suggesting that it was not only the inhabitants of house C, but other neighboring households as well, who had close social ties to the occupants of house B. The inhabitants of houses B and C were clearly interdependent because they shared a well (in B) and a courtyard and cesspit (in C) that had previously belonged to a single house, an arrangement that must have resulted in constant social interaction. But if the other households in the insula also had close ties to house B, then the existence of only one tomb for this cluster of houses is explicable, because the entire insula may have been inhabited by an urban clan or patronymic association that buried its dead together. Similarly, the entire insula apparently shared the same well. The excavators themselves point out that it is unlikely that the households without wells brought water in from streams outside the city when there was a well close by in house B (Calvet and Geyer 1987:134).

To sum up: the smaller sizes of the neighboring houses and the presence in house B of important facilities that were probably shared by its neighbors indicate patterns of social interaction in this insula that are well known in later Mediterranean and Near Eastern cities. And similar house-clusters are found also in the Ville Sud, particularly in Insulas 6 and 13 (see figure 20 above). Insula 6 of the Ville Sud had four houses with a wide range of sizes (65, 130, 165, and 200 m$^2$) that shared one oil press and two tombs. Insula 13 had at least seven houses of various sizes arranged around a blind alley that penetrated deep into the insula.

![Figure 23. Plan of the “Quartier Résidentiel” at Ugarit (after Yon 1997:75, fig. 36)](image-url)
In addition to the recently excavated dwellings in the Centre de la Ville and the houses of the Ville Sud that Callot has described, several dozen Late Bronze Age houses were uncovered during earlier campaigns of excavation at Ras Shamra, in the “Quartier Résidentiel” (formerly called the “Quartier Égéen”) east of the royal palace (see figure 23), and in the “Acropole” and “Sud Acropole” areas south of the temples. Unfortunately, little is known about these houses. Large-scale plans of individual houses have been published in only a few cases, and even these plans are quite schematic and are not accompanied by detailed descriptions or lists of small finds (see Courtois 1979a for a selective survey of Late Bronze Age domestic architecture at Ugarit). It is clear, however, that the basic elements mentioned above (a courtyard, a well, and a tomb) are found in the “standard” or ideal house, although such houses were often subdivided or amalgamated later to form smaller or larger dwellings. The house areas calculated below in table 16 represent the final state before the abandonment of the city of the most fully described of these dwellings, revealing a range of variation in house sizes that indicates a mixture of nuclear- and joint-family households which conforms to what was described in chapter 7.2 as an enduring Mediterranean pattern, and is also evident in the Ugaritic administrative texts discussed above.

Assuming 10 m² of upper-floor roofed living space per person, household sizes in the Centre de la Ville and the Ville Sud ranged from 3–18 persons with a mean of 8.6 per house (see table 17). A large proportion of the dwellings were big enough to have housed three-generation joint families of 7 persons or more. If population density was higher and per capita living space in Ugarit was only 8 m², individual households in this sample consisted of 4–22 persons, with a mean of 10.7 per household. This higher density of 8 m² per person is likely only if there were a few servants or more distant kin in each household in addition to immediate family members, which is quite possible. Otherwise, a lower density of 10 m² conforms quite well to demographically predicted family sizes.

These schematic figures are not meant to be exact but to indicate a plausible range of household sizes and proportions of household types in light of the architectural evidence. The actual situation in Late Bronze Age Ugarit must have been much more complex and would have changed constantly. There was also variation from one neighborhood to another. For example, estimates of household size calculated on the same basis would be substantially greater for the Quartier Résidentiel, where individual houses were much larger on average. It may be that the wealthiest households in Ugarit enjoyed considerably more roofed living space than 10 m² per person, but they probably also had more servants and quasi-kin domestic retainers, in addition to larger, possibly polygamous, joint families. It is therefore reasonable to suppose that the total number of occupants was proportionate to the roofed area of each house, although the actual density is difficult to determine in this rather unusual domestic district. For most ordinary private houses in Ugarit, however, plausible estimates of household size can be obtained by assuming that roofed dwelling space averaged 8–10 m² per person, a figure that is supported by Near Eastern historical data pertaining to urban settlements and by cross-cultural ethnographic research (see chapter 8.2). This assumption also yields estimates of the number of persons per residential hectare which are in keeping with what is found in other premodern walled towns of the Levant, such as Ottoman Aleppo (see chapter 7.3).

It is worth noting that the largest houses in the Quartier Résidentiel (QR-16, QR-17, and QR-A in table 16) were not planned as mansions but consisted of many interconnecting rooms and courtyards of various sizes, with roofed living areas totaling ca. 435, 595, and 600 m², respectively. The excavators have suggested that the “Maison de Rapanou” (QR-A), named after a high official whose archive was found in this house, was originally two separate houses (Courtois 1979b:1254). In fact, there may have been three houses here originally, because there are three tombs, three staircases, and at least three courtyards. Similarly, the irregularly shaped “Maison aux albâtres” (QR-17), which takes up most of an insula, may represent the amalgamation of several ordinary houses. Some of the largest private houses in Ugarit therefore do not appear to have been planned as palatial residences featuring spacious gardens and huge audience chambers. On the contrary, the large number of average-sized rooms in these houses may indicate that the owner and his family did not have abnormally large living quarters, but shared their home with numerous domestic retainers, who perhaps had families of their own.

13 One private residence that does seem palatial is the “Petit palais” or “Palais Sud,” just south of the royal palace, which Courtois (1990) has convincingly identified as the house of an important official named Yabnina. The walls of this house are thicker than average, its rooms are unusually large, and it stands apart from other dwellings in the vicinity as a single, planned structure rather than the product of the accretion or amalgamation of ordinary rooms.
Table 16. House Areas in Late Bronze Age Ugarit

<table>
<thead>
<tr>
<th>House</th>
<th>Overall (m²)</th>
<th>Courtyard (m²)</th>
<th>Less court (m²)</th>
<th>House</th>
<th>Overall (m²)</th>
<th>Courtyard (m²)</th>
<th>Less court (m²)</th>
</tr>
</thead>
<tbody>
<tr>
<td>CV-A</td>
<td>80</td>
<td>20</td>
<td>60</td>
<td>Mi-2</td>
<td>100</td>
<td>15</td>
<td>85</td>
</tr>
<tr>
<td>CV-B</td>
<td>100</td>
<td>(40?)</td>
<td>0</td>
<td>Mi-4</td>
<td>155</td>
<td>10</td>
<td>145</td>
</tr>
<tr>
<td>CV-C</td>
<td>65</td>
<td>10</td>
<td>55</td>
<td>Mi-5</td>
<td>45</td>
<td>0</td>
<td>45</td>
</tr>
<tr>
<td>CV-D</td>
<td>130</td>
<td>25</td>
<td>105</td>
<td>Mi-6</td>
<td>120</td>
<td>15</td>
<td>105</td>
</tr>
<tr>
<td>CV-E</td>
<td>55?</td>
<td>15?</td>
<td>40</td>
<td>Mi-7</td>
<td>100</td>
<td>25</td>
<td>75</td>
</tr>
<tr>
<td>VS-1</td>
<td>175</td>
<td>10</td>
<td>165</td>
<td>Mi-8</td>
<td>200</td>
<td>55</td>
<td>145</td>
</tr>
<tr>
<td>VS-2B</td>
<td>70</td>
<td>15</td>
<td>55</td>
<td>Mi-9</td>
<td>110</td>
<td>25</td>
<td>85</td>
</tr>
<tr>
<td>VS-4Bn</td>
<td>40</td>
<td>5</td>
<td>35</td>
<td>Mi-10a</td>
<td>250</td>
<td>40</td>
<td>210</td>
</tr>
<tr>
<td>VS-4Bk</td>
<td>120?</td>
<td>0</td>
<td>120</td>
<td>Mi-11</td>
<td>350</td>
<td>65</td>
<td>285</td>
</tr>
<tr>
<td>VS-5A</td>
<td>95</td>
<td>10</td>
<td>85</td>
<td>Mi-12</td>
<td>200?</td>
<td>25</td>
<td>175</td>
</tr>
<tr>
<td>VS-6A</td>
<td>200</td>
<td>25</td>
<td>175</td>
<td>QR-13</td>
<td>70</td>
<td>15</td>
<td>55</td>
</tr>
<tr>
<td>VS-6B</td>
<td>65</td>
<td>10</td>
<td>55</td>
<td>QR-14</td>
<td>250</td>
<td>50</td>
<td>200</td>
</tr>
<tr>
<td>VS-6C</td>
<td>150</td>
<td>30</td>
<td>120</td>
<td>QR-15</td>
<td>60</td>
<td>0</td>
<td>60</td>
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<tr>
<td>VS-6D</td>
<td>150</td>
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<td>100</td>
<td>QR-16</td>
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<td>435</td>
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<td>50</td>
<td>QR-17</td>
<td>670</td>
<td>75</td>
<td>595</td>
</tr>
<tr>
<td>VS-10B</td>
<td>210</td>
<td>40</td>
<td>170</td>
<td>QR-A</td>
<td>675</td>
<td>75</td>
<td>600</td>
</tr>
<tr>
<td>VS-10C</td>
<td>90?</td>
<td>20</td>
<td>70</td>
<td>QR-B</td>
<td>160</td>
<td>20</td>
<td>140</td>
</tr>
<tr>
<td>VS-13A</td>
<td>75</td>
<td>0</td>
<td>75</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>VS-13B</td>
<td>120?</td>
<td>15?</td>
<td>105</td>
<td></td>
<td></td>
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<tr>
<td>VS-13C</td>
<td>70</td>
<td>0</td>
<td>70</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>VS-13D</td>
<td>55</td>
<td>10</td>
<td>45</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>VS-13E</td>
<td>75?</td>
<td>0</td>
<td>75</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>VS-13F</td>
<td>130?</td>
<td>30</td>
<td>100</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>VS-14B</td>
<td>45</td>
<td>0</td>
<td>45</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>VS-14C</td>
<td>100</td>
<td>15</td>
<td>85</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>VS-14D</td>
<td>110</td>
<td>0</td>
<td>110</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>VS-14F</td>
<td>40</td>
<td>0</td>
<td>40</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total</td>
<td>2690</td>
<td>380</td>
<td>2310</td>
<td>Total</td>
<td>4000</td>
<td>560</td>
<td>3440</td>
</tr>
<tr>
<td>Mean</td>
<td>99.6</td>
<td>14.1</td>
<td>85.6</td>
<td>Mean</td>
<td>235.3</td>
<td>32.9</td>
<td>202.4</td>
</tr>
<tr>
<td>S.d.</td>
<td>46.6</td>
<td>13.4</td>
<td>39.8</td>
<td>S.d.</td>
<td>198.9</td>
<td>24.6</td>
<td>177.6</td>
</tr>
</tbody>
</table>

Note: The data for Centre de la Ville are taken from the plans published in Yon et al. 1987c; 1990a; for Ville Sud from Callot 1983; 1994; and for the Quartier Résidentiel and miscellaneous houses from Courtois 1979a, figs. 2–17.

Partly excavated houses whose area cannot be determined are excluded. Overall house areas are measured inside the exterior walls, but courtyard areas include the courtyard walls. Estimates of roofed living area (overall less court) assume that courtyards were completely open and only the upper floor was used for living space. In cases where the identification of the courtyard(s) is uncertain, the courtyard area is shown in italics.

a House B in Centre de la Ville probably did not have a courtyard; thus a roofed living area of 100 m² is assumed here. If it did have an unroofed courtyard, its roofed area was only 60 m². Locus 1265 in house B is tentatively identified as a courtyard by the excavators (Yon et al. 1987c:25; see figure 22 above), although they provide no evidence for this interpretation. If 1265 were an open courtyard, both it and the adjacent room 1282, which can be reached only from 1265, must be excluded (a combined area of 40 m²) when calculating the upper-floor roofed area, because no access to a floor above 1282 would have been possible from within house B. This implies an unusual waste of space,
so I think it more likely that loci 1265 and 1282 were covered and the upper floor area included the whole house (i.e., 100 m\(^2\)). Light and air would therefore have entered house B through the two doorways opening onto street 1038 and (as the excavators themselves suggest) through windows opening onto the neighboring courtyard (1064) in house C, the courtyard of the original house that was subdivided to form houses B and C.

b House E in Centre de la Ville was not completely excavated, so the figures here are reasonable estimates.

c House B in Insula 4 of the Ville Sud was probably two separate dwellings (VS-4B north and VS-4B south) flanking a large open courtyard (locus 7). VS-4B south was not completely excavated, but is ca. 120 m\(^2\).

d House numbers in this column (e.g., Mi-2, QR-13) correspond to the figure numbers in Courtois 1979a (figs. 2–17), except for QR-A (the “Maison de Rap\(\ddot{a}n\)u”) and QR-B (the “Maison du Lettré”), which are not described in Courtois 1979a. Measurements of QR-A and QR-B are taken from Schaeffer 1970, plan 1 (cf. TEO 1:275, fig. 33a).

e Identifications of courtyards in Courtois 1979a (which are quite tentative, in any case; see Yon 1992b:20) are ignored if it seems more likely that another room was the courtyard (e.g., a room in the center of the house), based on the courtyard’s presumed function as a source of light and air. Also, the last building discussed in Courtois 1979a (fig. 18) is not included here because it is only partially preserved, thanks to its location on the eroded northern edge of the tell (cf. Callot 1986).

<table>
<thead>
<tr>
<th>Table 17. Population Density and Household Size in Ugarit (Centre de la Ville and Ville Sud)(^a)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Roofed area per person:</td>
</tr>
<tr>
<td>10 m(^2)</td>
</tr>
<tr>
<td>Total second-story roofed area (m(^2))</td>
</tr>
<tr>
<td>Population in 27 houses = 0.67 ha</td>
</tr>
<tr>
<td>Persons per house</td>
</tr>
<tr>
<td>b Persons per residential hectare</td>
</tr>
</tbody>
</table>

\(^a\)Only the houses in the Centre de la Ville and the Ville Sud are considered in table 17 because they form contiguous residential blocks, unlike the miscellaneous scattered dwellings discussed in Courtois 1979a (figs. 2–12). The houses excavated before 1978 that are described in Courtois 1979a are also larger than average and so would bias the sample, because Courtois chose the most impressive examples. The Quartier Résidentiel, in particular, has three very large houses (QR-16, QR-17, QR-A). These are near the royal palace and were apparently inhabited by some of the most important royal officials, as the presence of archives (e.g., those of Rap\(\ddot{a}n\)u and Ra\(\ddot{s}\)ap\(\ddot{a}\)bu) indicates. The Centre de la Ville and Ville Sud provide a better picture of typical households in Ugarit.

\(^b\)The total areas excavated are ca. 1,000 m\(^2\) (Centre de la Ville, northern insula) and 5,700 m\(^2\) (Ville Sud). These figures do not include nonresidential districts, so the “persons per residential hectare” in this table applies only to densely occupied domestic zones, not to the city as a whole. I have not attempted to estimate the population of the entire city or the overall population density per hectare, because the ratio of residential to nonresidential space is unknown, as is the original area of the now-eroded tell—see Yon’s (1992b:20f.) critique of Randall Garr’s (1987) estimates.
Figure 24. Second-story roofed areas in Ugarit
(Overall area “less court” in the Centre de la Ville and the Ville Sud.)
In traditional Mediterranean cities, a householder’s wealth and status was reflected not so much in the amount of personal living space that he and his family enjoyed as in the size of his domestic retinue, creating what Philippe Ariès (1962) has called a “crowded social life” in a “big house.” If roofed living space averaged 10 m² per person, for example, even in such houses as QR-16, QR-17, and QR-A, then these dwellings were inhabited by big households that ranged in size from 43–60 persons.

3. Urban Farmers: Evidence of Agriculture in the City of Ugarit

The archaeological evidence from Ras Shamra indicates not only the prevalence of joint families, but also the essentially agrarian character of urban households in Late Bronze Age Ugarit. I have argued above in chapter 11 that it is unlikely that more than a small minority of the residents of the city of Ugarit subsisted entirely on royal rations. No doubt many householders were occupational specialists; but these men would have combined farming with professional work, or would have relied on their sons and clients to work their land. In premodern urban societies professional specialization does not preclude agriculture. Indeed, the land-grant system evident in legal texts from Ras Shamra presupposes that royal servicemen would support themselves primarily by their own agricultural efforts, a much more efficient arrangement than the distribution of royal rations on a massive scale. To be sure, the king gave rations of grain, wine, and oil to professional specialists (and to un-specialized corvée laborers) in return for their service, as the administrative texts reveal. But the archaeological evidence shows that farming was an important activity for many residents of the city. In general, some sort of “manorial” land-tenure system was characteristic of urbanized ancient Near Eastern polities, and even during the Hellenistic and Roman periods, urban households throughout the Mediterranean region grew much of their own food, as Moses Finley has pointed out (see chapter 6).

The textual and archaeological evidence from Ugarit and other ancient Mediterranean societies therefore does not support a rigid urban-rural dichotomy. Recent archaeological and historical work in the Near East has begun to recognize this (e.g., Schwartz and Falconer 1994), but it is still frequently assumed that the social and economic organization of ancient cities was quite different from that of villages. For this reason, the material remains of agricultural activity found within the city of Ugarit have often been overlooked or misinterpreted. Yet there is evidence for cereal harvesting, processing, and storage (stone tools and silos), olive-oil production (olive presses), animal husbandry (watering troughs and stables), and soil fertilization (compost pits). Unfortunately, no village sites near Ras Shamra have been excavated, so it is impossible to compare urban and rural residential patterns and productive facilities. Comparative historical and ethnographic data suggest, however, that the urban farmers of Ugarit lived and worked in much the same way as their rural counterparts.

Stone Tools and Silos

The excavators of Ras Shamra have published a collection of detailed studies of the stone objects found at the site, focusing especially on the more recently excavated Centre de la Ville (Yon et al. 1991 = RSO 6). The assemblages of stone tools, in particular, are characteristic of agricultural households (“urban” or “rural”) throughout the ancient Levant. The ground-stone implements found in Houses A–F of the Centre de la Ville include mortars, pestles, querns, rubbers, and pounders that were used in food preparation. More important as evidence of cereal production, however, as opposed to cereal processing, are the numerous flint sickle-blades that were recovered from these houses (see Coqueugniot 1991:146–59; 168–70). That these blades were used to harvest cereal crops is demonstrated by the silica sheen on them and the traces of bitumen used to attach the blades to a wooden haft in order to make a composite sickle (ibid., pp. 151 and 168). Furthermore, sickle-blades constitute 433 out of 562, or 77%, of the flint tools (excluding cores and débitage) found in the Centre de la Ville; and these blades represent 70–80 complete sickles (ibid., pp. 142, 170). The excavators do not comment on the significance of this evidence for the economic organization of the city, but the great quantity of sickle-blades retrieved from only six houses is, by itself, indisputable proof of the agricultural pursuits of their inhabitants.

In addition to stone tools, stone-built plastered silos (which presumably once had wooden coverings) were found in the Centre de la Ville. Silo 1069 in room 1047 of House A is elliptical in shape, with an interior diameter of 0.75–1.0 m and a depth of 1.1 m, for a volume of ca. 0.7 m³ (Yon et al. 1987c:45 and fig. 25). In room 1062 of House B there is a pair of silos (1270 and 1279) with volumes of ca. 0.7 m³ and 0.4 m³, respectively (ibid., p. 82; Calvet and Geyer

14 See Elliott 1991, esp. pp. 64–68 on artifact distribution. Note that “House” G, in which an oil-pressing installation was found (see the next section), had very few stone tools, as one might expect.
The presence of silos alone does not prove that cereal crops were cultivated by the inhabitants of these houses; but taken together with the discovery of numerous sickles and other remains of agricultural activity that are discussed below, it seems likely that substantial quantities of grain were produced and stored by the residents of the Centre de la Ville. In many households this grain was supplemented at regular intervals by royal rations, but that in no way contradicts the archaeological evidence for sizable, agricultural joint families in the city of Ugarit.

Olive Presses

The excavations in the Centre de la Ville uncovered a well-preserved oil-pressing installation in House G. Olivier Callot (1987) has discussed this press in some detail, and has reconstructed its appearance. The press was located in a rectangular walled yard (locus 1260) that is 77 m² in area and is bounded on three sides by streets and alleys. On the north side it is bounded by House D, which opens onto the unroofed pressing yard. In one corner of the yard there was a stone-paved area where the olives were crushed prior to pressing. This was separated by a stone partition from the area that held the beam press. The stone slab that served as a pressing platform is slightly hollowed out and has a channel at one end through which the oil flowed into a waiting receptacle. Standing beside it was a large pierced stone that was used to weigh down the wooden beam of the press.

Under the pressing yard was a small Middle Bronze Age tomb, and it was initially thought that this tomb was not in use at the same time as the press (Salles 1987:173). More recently, Joël Mallet (1990) has reported that the press is also Middle Bronze in date, and the excavators now suggest that the enclosure in which it was found was a walled garden or an open space during the latest phase of the Late Bronze Age (Yon et al. 1990a:6). The press apparently was in use during the first part of the Late Bronze period, however, and so was contemporary with an earlier phase of the houses in this insula. Whatever its date, the pressing installation in House G was built as part of a residential complex, like the similar oil presses in the Ville Sud and elsewhere at Ras Shamra that Callot (1987; 1994:191–96) has described, which were in use in domestic contexts as late as the final abandonment of the city.

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15 The figure of 785 kg/m² is used by, e.g., Kemp (1986:132), who also notes that 1 m³ of barley is lighter, weighing 705 kg. Wheat was probably the principal cereal crop in Ugarit, however, so the higher figure is used here.

16 Yet the household in House C also had two stone-lined plastered pits—note that no stone-built silos were found in Houses C–F, which must have had other means of storing grain.16 The households in the Centre de la Ville may also have relied in part on grain rations that were disbursed at regular intervals from the palace. The administrative texts recording rations support the latter interpretation, because they indicate that many households in the city did not grow all of the grain that they consumed. In any case, roofed area appears to be a better basis than silo capacity for estimating numbers of occupants.

17 See the most recent plan of the northern part of the Centre de la Ville in Yon et al. 1990a:2 (fig. 1), which shows that after further excavation the so-called Maison G turned out to be an unroofed (or at least partly open) enclosure.
The discovery of the oil press in the Centre de la Ville prompted Callot to search for evidence of other presses at Ugarit that had been uncovered earlier but were misidentified. He discovered that a number of pressing platforms, carved from stone slabs, had been found over the years in residential areas scattered across the tell. The original director of the Ras Shamra excavations, Claude Schaeffer, thought that they were libation-tables, but they resemble oil-pressing platforms found elsewhere in the ancient Levant (see Callot 1987:208 and the references there to Schaeffer’s interpretation). Similarly, many of the so-called stone anchors found throughout the site were incorrectly identified—they were almost certainly used as beam weights in oil presses, and not as anchors, as Callot (1987:209) points out (cf. Frost 1969; 1991). Some of them, particularly the large three-holed type, could only have been used as ships’ anchors because of their great weight; but the smaller, one-holed “anchors” are identical to the pierced weights typically found with olive-oil presses, and should be identified as such.

It is likely that many presses were uncovered throughout the site, but because they were misidentified and because the detailed plans remain unpublished we have a much smaller sample than we might otherwise have had from which to calculate the ratio of presses to dwellings. Nonetheless, it is worth noting that in the Centre de la Ville there is one press in an insula consisting of what were originally five houses, and this pattern is found also in the Ville Sud, in which most insulae had one pressing installation. Callot identified a total of seven presses distributed among the thirty-odd houses in the Ville Sud, suggesting that a ratio of presses to dwellings of approximately 1:5 was the norm (see Callot 1994:379, fig. 290). A similar ratio is found in eighth-century east Palestine in the Judahite walled town at Tell Beit Mirsim (see chapter 8.1).

No doubt these presses supplied oil for domestic use, but they may also have been used to produce surplus oil for export. Olive oil was a major export of the Levant as far back as the Early Bronze Age (see Stager 1985b), and its importance in Late Bronze Age Ugarit is reflected in the numerous legal and economic texts in which olive orchards and olive oil are mentioned. Pottery provides the best evidence for oil exports, however. In particular, the so-called Canaanite jar, which has been found in large quantities at Ras Shamra and in the nearby port of Ugarit at Minet el-Beida, was used to transport various commodities, including oil and wine. The Canaanite jar traveled all over the eastern Mediterranean during the Late Bronze Age. It is depicted in Egyptian reliefs that portray the unloading of cargo from Syrian ships (Grace 1956). The thirteenth-century examples are sturdy, heavy-shouldered containers ca. 50 cm tall, with tapering profiles and pointed toes that permitted them to be stacked in the holds of ships. This sharp-shouldered type, of which several dozen were found arranged in rows awaiting shipment at Minet el-Beida, the port of Ugarit (see Ug 1:32f. and plate 9), has been discovered in the tombs of the wealthy from Mycenae to Amarna. More than a hundred Canaanite jars of this type were found in the cargo of the Late Bronze Age ship that was wrecked off the Turkish coast near Ulu Burun, en route from the Levant to the Aegean (Bass 1986:277f.; Pulak 1988:10f.; Bass et al. 1989).

The question remains, however, whether the oil exports of Ugarit were the preserve of the king and a few of his high officials, who perhaps traded the surplus production of the royal estates known as gts (literally “press”), or involved humbler households as well. The textual data do not give a clear answer, although it is reasonable to suppose that in addition to its own production of oil and wine the palace collected oil as a tax payment from ordinary households for redistribution or for trade. But Callot believes that only the wealthiest households could have owned an oil press. As he puts it: “En effet la possession d’une huilerie peut plus difficilement être le fait de personnes modestes, car elle implique d’une part certains moyens financiers pour la réaliser, mais aussi d’autre part la propriété d’oliviers capables de l’alimenter, ou alors une clientèle suffisante de riches propriétaires fonciers” (Callot 1987:209f.). Accordingly, he argues that the numerous presses found in modest houses did not belong to their inhabitants but were either royal gts (contrary to the usual assumption that the gts mentioned in the administrative texts were all rural estates), or were owned by temples. Thus he suggests that the pressing yard in the Centre de la Ville did not belong to the neighboring houses to the north, with which it was contiguous, but belonged to what the excavators have interpreted as a local sanctuary—the so-called “Temple aux rhytons” situated across the street to the south (see Mallet 1987).

This interpretation is incorrect, in my opinion. Aside from the question of whether the “Temple aux rhytons” was indeed a sanctuary, there is no need to attribute oil presses only to royal, cultic, or otherwise elite establishments. There is a correlation between joint-family households and horticulture in the tradi-

18 Frost suggests that the smaller “anchors” were “fishing tackle” because they are not heavy enough to have anchored a boat. These are clearly press-weights, however.
tional Mediterranean economy. Large households serve to pool labor and capital, making possible investments in productive resources that a nuclear family could not afford. Furthermore, ethnographers in the Middle East have observed patronymic associations of households that cooperate in certain ways for their mutual benefit. The fact that there is one press in an insula of five or six houses in the Centre de la Ville, and a similar pattern in the Ville Sud, suggests to me that the press was used by all of the inhabitants of this small neighborhood, who probably had close social and economic ties with one another, and who also shared a well and a tomb, as I have argued above. Even if the press was used exclusively by the occupants of House D, which opens directly onto the pressing yard, there is no reason to doubt that they had the necessary manpower and resources to build and operate it, because House D has a roofed area of at least 105 m² and could therefore have accommodated a large joint-family household. Furthermore, the remains of numerous Canaanite jars in rooms 1201 and 1206 of House E may be evidence that other households in the insula also made use of the press (Yon et al. 1983:211 [photograph in fig. 10]; 1987c:97). In room 1201 at least six Canaanite jars were lined up along a wall. A Canaanite jar was also found in room 1221 of House F (Yon et al. 1983:213).

Watering Troughs, Mangers, and Stables

Many dwellings at Ras Shamra possess massive, rectangular basins cut from blocks of stone. Many of these were almost certainly watering troughs (or perhaps mangers, in some cases) for the animals stabled inside the houses. Jean Margueron uncovered one or more of these basins in each house that he excavated in 1975 and 1976 at the eastern edge of the Quartier Résidentiel (see Margueron 1977a:166ff.; cf. the stone basins in “la maison aux fours” noted by Courtois 1979a:107 and fig. 3). One of the basins was beside a well, but Margueron rejects the notion that it was used for watering animals, because the stone basins were found in the heart of the city and “le très grand nombre impliquerait une population animale excessive.” He suggests instead that some of the basins, at least, were “une sorte d’armoire ou coffre de rangement,” because more than 100 crude bowls were discovered in and around one of them—or else this basin with bowls was used for washing dishes, because there was a well in a neighboring room and the many vessels found near it indicate (according to Margueron) that the building in which it sat was destroyed just after a meal eaten by a large number of people. But the connection between animals and stone basins should not be minimized, in my view. In some cases, no doubt, such basins held water for human consumption; however, archaeological, historical, and ethnographic parallels suggest that substantial numbers of animals were stabled inside the city of Ugarit, in keeping with the agrarian character of most of the households. Thus many of the stone basins should be interpreted as either watering troughs or mangers.

Furthermore, the more detailed plans published in recent years show that at least some of these basins bordered areas paved with flagstones—in other words, stables. This is the case in House D of the Centre de la Ville, where a rectangular stone basin (locus 1302) measuring ca. 1.4 × 0.6 m is positioned along the northern edge of a paved area (Yon et al. 1990a:7 and fig. 1 [plan]).¹⁹ There are also remnants of flagstone pavement beside the rectangular basin (1.0 × 0.55 × 0.55 m) in room 1066 of House C, whose floor was badly disturbed, and in courtyard 1206 of House E (although no stone basin was found there).²⁰

It is generally agreed that the stone-paved side-rooms in Iron Age pillared houses in Palestine served as stables. Lawrence Stager (1985a:14) has summarized the evidence for stables on the ground floor of Iron Age dwellings, noting that the function of the flagstone pavement was to support the heavy animals and to permit their stalls to be mucked out more easily, while urine that was not absorbed by the straw or reed bedding could percolate between the stones. The combination of stone basins and flagstone pavement strongly suggests the presence of stables in at least two of the houses in the northern insula of the Centre de la Ville. There may also be a stable in the southern part of this excavation area, in the building south of the “Temple aux rhytons” (Yon et al. 1990a:12–18). No stone basin was found here, but there is a rectangular area measuring ca. 2.5 × 5.0 m (locus 184) that is open on one end and was paved with flagstones. It was separated from an adjoining, unpaved area (locus 185) by a low partition wall (183) that ended in a monolithic pillar. The excavators do not explain these features other than to suggest that this was a covered space with a utilitarian function, but the parallel with Iron Age pillared buildings indicates that it was a stable.

¹⁹ No measurements are reported by the excavators, so I have estimated the dimensions of this basin using the published plan.

²⁰ See Yon et al. 1983:206f. (photo in fig. 4); 1987b:180; 1987c:plate 6 (plan) and p. 97f. (photo in fig. 77).
Finally, near the southern edge of a recently opened area called the “Sud-centre,” which was first excavated in 1986, the excavators have uncovered three rectangular stone basins (see photograph 3 and figure 25 below). The smallest of these basins (locus 2015 in room 2016) measures ca. 0.8 × 0.6 m, and there appear to be remnants of flagstone paving nearby (Yon et al. 1987b:190; 1990a:24 [fig. 19]). (No measurements are given for this basin, so I have estimated its length and width based on the plan; the height and interior depth are not stated.) A much longer basin (locus 2085 in room 2076) has dimensions of 2.30 × 0.68 m, with an interior cavity 25 cm deep (Yon et al. 1990a:24–27). In a neighboring room, there is an unusual double basin (locus 2086 in room 2077), carved from a stone block measuring 1.90 × 0.60 m, with a height of 30 cm and two equal-sized depressions that are 18 cm deep.

Photograph 3. Houses in the “Sud-centre” at Ugarit with stone basins (view to the north) (Photograph by David Schloen.)

Figure 25. Plan of the “Sud-centre” at Ugarit (after Yon et al. 1990a:24, fig. 19; see photograph 3 above)
The excavators acknowledge that these stone basins may have been mangers or watering troughs; however, they leave open the possibility that they had some other function (ibid., p. 27). But the smallest basin in the Sud-centre is in a room that was apparently paved with flagstones; furthermore, near the other two basins are pillars made of large stone blocks, which would have supported an upper floor while leaving open one side of the rooms in which the basins sat, suggesting that those rooms were stables. Whether these three basins were watering troughs or mangers is difficult to determine, but the unusual double basin is shallower than the longer basin in the next room, and may have held fodder rather than water.

The two largest Sud-centre basins are both positioned at right angles to the neighboring walls and serve to close off access, except for a narrow gap, to the rest of the stable—a characteristic also of the stone basins in Houses C and D of the Centre de la Ville. The animals would have entered at the other end of the room, but there was enough space in each case for a stableboy to squeeze past the basin into the stable. In the Sud-centre, further excavation to the south of where the published plan ends may reveal the main entrance to the stable area, as well as evidence of flagstone pavement.

The excavators, however, do not identify the rooms with basins as stables; and they have suggested that some, at least, of the heavy stone basins were moved from their original position by squatters after the destruction of the city. With respect to House C of the Centre de la Ville, for example, they argue that because the basin in room 1066 prevents access to room 1089 to the south and blocks part of doorway 1091, it was placed there by shepherds camping in the ruins of the city sometime after the destruction of the city in ca. 1180 B.C. (Yon et al. 1983:73). More recently Callot (1994:201ff.) has noted the likelihood that a large number of animals were housed in the city, including not just transport animals such as horses and donkeys but also cattle, sheep, and goats. But it is not clear why he goes on to suggest that the animal population within the walls declined during the last centuries of Ugarit’s existence, which would betoken a major economic and demographic change for which there is no evidence.

21 In a previous article, however, Yon (1985:718) does briefly acknowledge the existence of domestic animals and agricultural activity within the city of Ugarit, as does Callot (1983:73). More recently Callot (1994:201ff.) has noted the likelihood that a large number of animals were housed in the city, including not just transport animals such as horses and donkeys but also cattle, sheep, and goats. But it is not clear why he goes on to suggest that the animal population within the walls declined during the last centuries of Ugarit’s existence, which would betoken a major economic and demographic change for which there is no evidence.

Compost Pits

With stables comes manure, and it is therefore not surprising that most of the houses in Ugarit were equipped with a stone-lined compost pit. The pits in Houses A and C of the Centre de la Ville have been described in some detail by Yves Calvet and Bernard Geyer (1987), who identify them as “puisards” (cesspools) for waste water. It is unlikely, however, that they were used primarily to dispose of waste water, as opposed to solid waste, because they contained potsherds, bones, and flints; moreover, the soil at the bottom of these pits was very fine and compact, and was marked by numerous traces of iron and manganese oxides, as were the potsherds found at this depth (ibid., p. 137). No chemical analyses of the soil have been published, but these pits clearly contained all manner of household garbage, including broken pottery, kitchen scraps, and—most importantly—human and animal excrement.

One of the largest “puisards” found in the Centre de la Ville is the rectangular pit 1080 in courtyard 1064 of House C, which is approximately 1.1 m long, 0.9 m wide, and 1.2 m deep. It is lined with fieldstones and was covered with a limestone slab measuring 1.30 × 1.10 × 1.15 m. Calvet and Geyer note that it contained organic material, as did the similarly constructed rectangular pit 1269 in House A, which measures 0.80 × 0.50 m (top) and 0.75 × 1.05 m (bottom), and is 1.90 m deep (ibid., pp. 137ff., 143). They suggest, moreover, that this came from latrines. But in the case of pit 1269, which contained many pot-
sherd, they argue, that although the pit had originally received “les eaux usées des latrines,” it was only after the latrines went out of use that the pit was filled with discarded pottery and other trash.

In my opinion, however, their assumption that the primary purpose of these pits was to dispose of liquid waste is incorrect. Ethnographic and historical parallels indicate that such pits were built to hold human and animal dung, to which was added liquid waste and anything else that needed to be disposed of (see the discussion below). It is true that the U-shaped pit (1096) located in the street on the north side of House A probably served as a cesspool only, because it is not inside a house, and a stone channel leading from room 1041 of House A runs under the exterior wall and empties into it; moreover, it was filled with beige sandy soil containing few potsherds and little evidence of organic waste (ibid., pp. 138ff.). But the fine, compact soil and traces of mineral oxidation in the bottom of pits 1080 and 1269, together with their large capacity, are clear indications that the primary purpose of the rectangular stone-lined “puisards” situated inside houses was to hold manure, although they also served as convenient receptacles for discarded pottery, stone tools, and other household trash.

This interpretation is supported by the fact that there was nothing at the bottom of the pits to filter the polluted water seeping out of them (assuming that they were cesspools) before it entered the water table, as Calvet and Geyer have noted (ibid., pp. 145ff.). They point out that some of the “puisards” are dangerously close to wells and silos, but they argue that the risk of contamination, although quite real, was nonetheless limited because of the slowness with which the water would have permeated the ground. But if the contents of the pits were primarily solid rather than liquid (which also meant that they were regularly emptied), the risk of water-borne pollution was greatly reduced and the placement of the so-called cesspools is much less anomalous.

Compost pits were found in Houses A and C of the Centre de la Ville, but House B does not have one. It is likely, however, that the residents of House B made use of pit 1080 in House C, which had originally formed a single dwelling with House B. In House E there is a rectangular, stone-lined compost pit (1253) in room 1209, near the entrance to the house, although a detailed description of its dimensions and contents has not yet been published (Yon et al. 1987c:95). The two remaining houses in the insula probably had compost pits as well, although this is not certain because House F has been only partially excavated, while in House D the excavators have identified “un puisard ou silo” (Yon 1990a:8) that had not been completely excavated when the most recent preliminary report was published. It is clear, however, that a compost pit was a standard feature of houses in the city of Ugarit, as is shown also by the frequency of “puisards” in the houses of the Ville Sud (Callot 1994:371, fig. 271).

The manure in such pits was mixed with vegetable matter and left to rot in order to produce fertilizer, and the compost was periodically removed and taken outside the city where it was applied to the fields—a practice known worldwide in agrarian societies (see Chisholm 1979). Tony Wilkinson (1982; 1989) has argued convincingly that the sherd scatters which can be observed around ancient Near Eastern settlements were created by this process. He notes that: “Because of the ineffectiveness of farm manures in semiarid environments it is likely that night soils and droppings were, from an early date, composted before application to the fields” (Wilkinson 1982:324). Wilkinson reviews the historical and ethnographic evidence for this kind of soil fertilization, which reveals that urban compost consisting of animal dung, human excrement, ashes, and household sweepings was a valuable commodity in traditional Mediterranean and Near Eastern societies.

Hypothesizing that broken pottery and other artifacts would have been mixed in with the compost, and that the density of surface sherds around an ancient site will therefore decrease with distance from it, Wilkinson has surveyed the area around a number of urban settlements (including the Early and Middle Bronze Age sites of Tell Sweeney and Tell al-Hawa in northern Mesopotamia) and has discovered that the thin surface scatter of pottery does indeed decline in density the farther one goes from a site. The sherd scatter disappears entirely at a distance of 3–6 km, that is, 30–60 minutes’ walk from the city. Sherd scatters therefore define zones of intensive cultivation around ancient Near Eastern urban settlements, zones whose size is comparable to the manuring zones observed around much more recent agricultural towns, because they are limited by the distance a farmer is willing to carry compost from his home in town to his fields.23 Indeed, Wilkinson (1994) has

22 Note that the sandy soil filling the upper portion of the other pits was probably washed in after the destruction of the city, and so need not indicate repeated filling with waste water.

23 Chisholm (1979:47–52) describes “agro-towns” inhabited by as many as 10,000 persons in southern Italy, Sicily, Sardinia, and Spain in the first part of the twentieth century.
argued that the reason that Upper Mesopotamian dry-farming settlements seldom exceeded 100 hectares (in contrast to the much larger irrigation-based settlements of southern Mesopotamia) was the high cost of transporting bulk commodities, which meant that most cities did not rely on imported food but sustained themselves mainly by intensive cultivation of the surrounding land. Moreover, the size of the main production zone around a city (and thus its population) was limited by the time and effort required to walk to and from the outermost fields. A study of settlement patterns and site catchments in the Jazira shows that, although some food was supplied by nearby villages, food produced farther away than 10–15 km made little contribution to a city’s needs.

The ground in the vicinity of Ras Shamra is too much obscured by vegetation to permit a similar study of the sherd-scatter zone around that site; but the presence of potsherds in the stone-lined pits of the Centre de la Ville, and the evidence from Bronze Age sites in northern Mesopotamia, suggests that a manuring zone of intensive cultivation did encircle ancient Ugarit. Furthermore, ancient Greek sites provide a close archaeological parallel to the stone-lined compost pits found at Ugarit. Fourth-century B.C. houses in the city of Halieis in the southern Argolid and also in Athens possess stone-lined pits with earthen bottoms that have been identified as koprones, or compost receptacles (Ault 1999; Owens 1983:47). These pits were square, rectangular, or trapezoidal in shape, ca. 2.25–5 m² in area and ca. 1–1.5 m deep. Many potsherds and other discarded material was found in them. They were thus strikingly similar to the “puisards” of Ugarit in shape, size, construction, and contents. E. J. Owens (1983) has summarized the Greek textual evidence pertaining to urban sanitary arrangements, which shows that the typical kopron was inside the house, usually near the main entrance. Owens argues that a kopron was not simply a latrine, because it was clearly a source of profit for its owner, as is indicated by several texts recording the sale or rental of the kopron separately from the rest of the house. Furthermore, persons known as koprologoi regularly emptied the koprones, and Owens suggests that these were not public slaves of low status, as has often been thought, but rather private entrepreneurs who purchased the composted dung for sale to farmers as fertilizer.

To sum up: there is substantial evidence that urban households in Ugarit engaged in agriculture. This is shown by the presence in the best-excavated and best-published residential area, the Centre de la Ville, of numerous sickles and other stone tools associated with cereal production and processing; of silos and large pithoi for the storage of grain; of oil presses and the distinctive type of “Canaanite” jar in which olive oil was shipped around the Mediterranean; of watering troughs, mangers, and flagstone pavement that indicate stables; and of stone-lined pits in which manure was composted to fertilize the fields. There are indications that similar evidence exists in every residential area so far exposed on the tell, although the data from other areas have not yet been published in adequate detail. Insofar as the Centre de la Ville is typical of domestic architecture at the site, as the excavators themselves have claimed, there is reason to believe that the members of joint-family households throughout the city of Ugarit subsisted primarily by means of their own agricultural efforts.

4. Intramural Family Tombs: The House of the Living as the House of the Dead

In many of the houses in Ugarit a corbel-vaulted tomb built of ashlar masonry is found beneath the floor and is entered from one of the rooms inside the house (on domestic tombs in Ugarit see Salles 1987; 1995). These tombs were intended for multiple interments, so they were equipped with an entrance ramp, or dromos, that could be unblocked for each new burial. Similar tombs have also been discovered at a few Middle and Late Bronze Age sites in Pales-
tine, although they are much less common there (see Gonen 1992a:98–123; 1992b). Three Middle Bronze II corbel-vaulted stone tombs were found beneath houses at Megiddo, and a single Late Bronze II tomb of this type has also been discovered at both Tel Dan and Tel Aphek. These tombs are directly comparable to those at Ugarit, although they are more poorly built. More distantly related are the eight large rectangular graves, lined with stones and covered with stone slabs, that were discovered in the Late Bronze Age cemetery at Tell el-‘Ajjul near Gaza. Those tombs were equipped with a stepped dromos and contained multiple burials, having been used repeatedly for three centuries or more.

Other parallels to the tombs of Ugarit exist in Mesopotamia; in particular, the corbel-vaulted mud-brick tombs of Old Babylonian Ur (Woolley and Mallowan 1976:33–39) and the even earlier corbelled stone tombs of the third millennium B.C. at Mari (see Jean-Marie 1990). In his discussion of the tombs at Ur, in which he notes the scantiness of the grave goods deposited with intramural as opposed to extramural burials, Leonard Woolley explains the practice of house-burial in an eloquent passage that is worth quoting at length:

The burying of the dead members of the household in the house which their descendants continued to inhabit certainly has for its motive the idea of preserving the continuity of the family; the generations were bound together, living and dead, by their sharing of the same dwelling, and while the living had to pay due respect to the dead they benefited, presumably, by their help and protection. . . . [It] was the privilege of the eldest son to be the “burner of incense to his father,” i.e., to carry on the ritual of ancestor-worship. It must be emphasized that the family vaults were made below the family chapels wherein the household gods were worshipped, and the cult of those and of the human forebears must have been intimately connected, and though the private person was not, after death, deified as was the king, yet the phrase “ancestor-worship” is perhaps not an undue exaggeration. In a patriarchal civilisation for which the family was the unit and the head of the family held powers of life and death he occupies in that restricted circle very much the position that the king occupies in regard to the country as a whole, and in Sumer with its localised gods very much the position that the patron god occupied in relation to the city.

The continuity of the family unit was of prime importance, and it could be in a measure assured by the retention within the walls of the family home of its departed members. This idea that the dead man continued to inhabit his old house might explain the poverty of the offerings in his tomb. So long as he was exiled after death to an outlying graveyard where he lay an alien and alone, it was necessary to supply him with all such things as might be needed for his enjoyment in another world; they had to be ready to his hand, or if anything to which he were accustomed he might return to his old home in the form of an angry ghost and so haunt the living to their hurt. But if he were still in his home everything there, everything in the use of the survivors was at his disposal as in the old days; there was no need to make special provision when he could take part in the family meals just as he took part in the sacrifices that were offered to the family gods, and the very absence of such provision emphasised the fact of his continued membership of the household. [Woolley and Mallowan 1976:38ff.; see also Woolley 1965:187ff.]

The God of the Father

Woolley’s interpretation has not been bettered, and it is equally valid for the tombs of Ugarit. The texts from Ugarit and from Emar shed further light on the relationship between the dead and the living in Late Bronze Age Syria, although there has been considerable debate about the nature of the ancestor cult in Ugarit and neighboring regions. It is generally agreed that deceased ancestors were regularly invoked by their living descendants and provided with food offerings. Some scholars have gone further and suggested that these ancestors were considered to be divine and were worshiped accordingly. The Ugaritic divine name ibia (written ilib) corresponds to DINGIR a-bi (literally, “the god of the father”) in an Akkadian god-list from Ugarit (RS 20.24:1 = Ug 5.18:1). In the “duties of the son” listed in the Ugaritic story of šAqhatu (KTU 1.17 1 24ff.) it is stated that a son is one who sets up a stela of his (i.e., the father’s) ilib in a holy sanctuary or temple (bdš). Is this ilib a divinized ancestor, hence a “divine father,” or is it the householder’s “paternal god” or “god of the father”?

In view of the Akkadian equivalent, DINGIR a-bi, the term ilib would seem to denote a clan deity—a “god of the father”—as in Mesopotamia, where we find similar references to “the god of my father(s)” (see the extensive literature cited in van der Toorn 1993:379ff. n.4). A number of scholars contend, however, that ilib instead denotes a deceased and deified

26 Compare the intramural burials, usually brick-lined shaft graves, of Early Dynastic Abu Salabikh in southern Iraq (Postgate 1985; Steele 1990). It is interesting to note in this regard Steele’s (1990) interpretation of the placement of the intramural burials at Abu Salabikh in terms of household competition and patterns of interhousehold patronage and dependency.

ancestor, “the father who has become a god,” citing the Hurrian equivalent _enni attanni_, “the god, the father” (written in _atn_ in _KTU_ 1.42:1). Karel van der Toorn (1993; 1996:154–68) has argued this in some detail. But there is no need to see in the Hurrian expression a reference to a deified human ancestor, as opposed to a clan god who is also, like the chief god ḪIIlu himself, a divine father.

Even if we accept that one’s _ilib_ was a god-like ancestor, this does not mean that all ancestors fell into this category, for the unnamed “god of the father/divine father” might well have been a distant anonymous ancestor regarded as the founder of the clan, as opposed to an identifiable individual of recent memory. In that case, an _ilib_ was not just any ancestor spirit but the protective “spirit of the clan,” the corporate representative of the members of the patrilineal clan, both living and dead.

This is supported by the ḪAqhatu passage in which the duties a son performs for his father (who is perhaps advanced in years but certainly not yet dead) include both mundane and cultic acts that ensure the physical maintenance of the house, the support and protection of his father’s person and reputation, and the veneration of his father’s clan deity. Note also that it presupposes a situation in which a grown son lives in his father’s house. The relevant passage is translated below, largely following the translation published recently by Dennis Pardee (1997a:344):

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<table>
<thead>
<tr>
<th>Line</th>
<th>Translation</th>
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<tbody>
<tr>
<td><em>l tbrkn l tř il aby</em></td>
<td>O Bull ḪIIlu, my father, please bless him; please pronounce a benediction upon him, O begetter of (all that is) begotten,</td>
</tr>
<tr>
<td><em>wykn bnh bbt</em></td>
<td>so that he may have a son in his house, a scion within his palace:</td>
</tr>
<tr>
<td><em>nš skn ilibh</em></td>
<td>someone to raise up the stela of his paternal god, in the sanctuary the votive emblem of his clan;^a</td>
</tr>
<tr>
<td><em>lars mšsu qtrh</em></td>
<td>to send up from the earth his incense, from the dust the song of his place;^b</td>
</tr>
<tr>
<td><em>ṭb qht nış</em></td>
<td>to shut up the jaws of his detractors, to drive out anyone who would act against him;</td>
</tr>
<tr>
<td><em>ahd ydh bškrn</em></td>
<td>to take his hand when (he is) drunk, to bear him up [when] (he is) full of wine;</td>
</tr>
<tr>
<td><em>spu ksmh bt bš</em></td>
<td>to eat his grain(-offering) in the House of Baświad, his portion in the House of ḪIIlu;</td>
</tr>
<tr>
<td><em>ṭh ggh bym ţtt</em></td>
<td>to resurface his roof when rain softens it up, to wash his outfit on a muddy day.</td>
</tr>
</tbody>
</table>

Notes:

^a Ug. ʾšm probably means “clan, father’s relatives” here rather than “kinsman, father’s brother” (cf. Heb. ʾšm).

^b The meaning of this couplet is uncertain. Theodore Lewis (1989:54, 60–65) makes a plausible case for the alternative translation “one who delivers his life from the Underworld / one who guards his footsteps from the Dust,” which means essentially that “the ideal son will keep his father from mortal danger while he is living” (p. 65). Note also van der Toorn’s (1996:165) view that _qtr_ here refers to the smoke of the domestic hearth as a symbol of the existence of the family. In that case the son is “one who sends forth his smoke from the earth, who protects his abode (qtr) from the dust”—in other words, the son is responsible to preserve his father’s family line within his ancestral home, symbolized by keeping the hearth lit and the house free from the dust and debris associated with abandonment (van der Toorn’s own translation is somewhat different).

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The dutiful son places in a temple, on behalf of his father, “the stela of his paternal god” _skn ilibh_ which is “the votive emblem of his clan” _ztr ʾšmh_. He also eats the food that is offered to the high gods in their temples on behalf of his father, and apparently also participates in the cult by burning incense. At home he keeps the house in good repair. In other words, a son is needed to take on the cultic and prac-
tical responsibilities of the aging head of the household, whom he supports and defends and will soon succeed as patriarch. In a way, the care and veneration of the ancestor begins already during his lifetime, but this is not to say that he is regarded as a “divine father” (ilib) himself.

In keeping with his view of the ilib at Ugarit, van der Toorn (1994; 1995) points to the household gods mentioned in texts from Emar and Nu zi (referred to in Akkadian as il and ilamu, respectively), arguing that these were actually ancestral spirits. At Nu zi, statuettes of these gods were passed down from father to son, and the heir who had possession of them was the head of the household. “The family gods represented the identity of the family. All family members had a right to participate in their cult; to be denied access to the gods was tantamount to being cut off from the family group” (van der Toorn 1994: 38). There was a similar domestic cult at Emar, and in the Emar inheritance texts the principal heir, by inheriting the main family house, is obliged to invoke, honor, and care for the ili u mitti, “the gods and the dead ones,” of his or her father (ibid., pp. 43–47—for ME-TE = “dead [pl.]” see Huehnergard 1983: 28). Van der Toorn interprets ili u mitti as an hendiadys; in other words, only the deified ancestors are intended here, which means that the inherited household gods of Emar (and of Nu zi) were ancestral spirits. Perhaps this is correct, but it is also possible that ili u mitti is not an hendiadys and that an explicit distinction was made between patron gods and deceased ancestors, both of whom required veneration. Indeed, van der Toorn (1995:47ff.) maintains that both ancestors and clan gods were worshiped at Emar.28

In Ugaritic god-lists ʾIluʾibi (ilib) is listed first, before the high gods ʾIlu, Dagan, and Baʾlu, which suggests that he was a primeval deity like the god Alalu in the Hurro-Hittite theogony. According to van der Toorn and others, ʾIluʾibi was an abstract figure who served as “the prototypical ancestor of the gods” themselves (van der Toorn 1993:385). Thus it is argued that the term ilib refers both to the “dead” ancestor of the “living” gods, whom they were supposed to honor, and, in parallel fashion, to the spirit of a deceased human patriarch who was to be honored by the living members of his household. But the undoubted parallelism between the ilib of the high gods of Ugarit and the ilib of a human family is operative whether or not the ilib was a deceased ancestor. What is significant is that ʾIluʾibi was the patron deity of the patrilineal clan of the gods themselves, who in this respect and many others reflect the most basic features of human society.

The Cult of the Dead

Regardless of whether deceased ancestors were called “gods” at Ugarit, it is very likely that there was a “cult of the dead” similar to the Mesopotamian kispu, characterized by regular offerings of food and drink for the dead (on kispu see Tsukimoto 1985). Many scholars have identified this with the Ugaritic term marzihu (or marzaḥu), which Marvin Pope (1981:176) has described as “a feast for and with the departed ancestors, corresponding to the Mesopotamian kispu.” Theodore Lewis (1989:80–98) has discussed in some detail the evidence and interpretations related to this term, which is cognate to Hebrew marzeh, a word that occurs twice in the Bible. He notes that there is little direct evidence for Pope’s interpretation, although the association of the rapāʿuma (who are clearly deceased ancestors; cf. Heb. ṭāʿāʾuma) with the marzihu in one Ugaritic text, and the mention of a bet marzēḥ in the context of mourning in Jeremiah 16:5, has led scholars to conclude that the marzihu was somehow related to the cult of the dead. Lewis’s own conclusion is that the marzihu at Ugarit was a socioreligious organization “known for its drinking festivals . . . which in some cases came to be associated with funeral feasts” (ibid., p. 94). Dennis Pardee (1996) has defended the even more “minimalist view” that the Ugaritic term marzihu has no relation to a funerary or mortuary cult in the texts currently known, although he does not deny that such a cult did exist.29

Whether or not there was any connection between the marzihu and the cult of the dead, it is generally agreed that deceased ancestors played an important social role in Late Bronze Age Ugarit. This is best shown in the funerary ritual RS 34.126 (KTU 1.161) for the burial of the penultimate king of Ugarit, Niqmaddu III. In this ritual, the ancestors of the

28 With regard to Emar, van der Toorn (1994:45) quite rightly notes the importance of the family dwelling as the locus of the cult of the family gods (whom he less plausibly equates with deceased ancestors). The images or stelae of these gods were inherited along with the house. As he says, the discovery of numerous clay house-models during the excavation of Emar (see also Margueron 1976) may indicate that as families grew and some had to leave the ancestral dwelling, they took with them a clay model of it so they could continue to practice the cults of family gods of which the house was the symbolic center.

29 Pardee also emphasizes (correctly, in my view) the lack of evidence for “deified ancestors” in Ugarit, interpreting ilib as “the god of the father”—what he calls “a generic designation of the special deity of a clan, tribe, or other ethnic group” (Pardee 1996:283f. n.17; 1997a:344 n.6).
Deceased king, who are called *rapātūma*, are summoned. Offerings are presented seven times\(^{30}\) and blessings are requested for the new king *Ammurapi*, the queen-mother Žarriyelli, and Ugarit itself (Bor-dreuil and Pardee 1991:151–63). It is true that this is a royal ritual which may have differed in many respects from ordinary funeral rites, but the physical similarity between the tombs in the royal palace and those found in ordinary houses suggests that this sort of ritual was not peculiar to the royal family, especially in light of the many textual references to the invocation of deceased ancestors by all types of people throughout the ancient Levant and Mesopotamia.

The original excavator of Ras Shamra, Claude Schaeffer, identified certain kinds of remains as evidence of the ancestor cults practiced above the tombs. In particular, he interpreted pits near the tombs, into which emptied stone gutters or clay pipes, as libation receptacles. Wayne Pitard (1994) has challenged Schaeffer’s interpretation, arguing quite convincingly that the so-called libation pits and the conduits leading into them had no relation to the tombs but were simply drainage devices. This does not mean, however, that no cult of the dead was practiced near the tombs. In most cases, it is true, the tomb was entered from a room that apparently had an ordinary domestic function. But the paucity of published evidence leaves open the possibility that some sort of shrine was normally located near the entrance to the tomb, as was the case in the houses of Old Babylonian Ur, where the mudbrick burial vault was typically under the floor of a room identified as the “private chapel” (Woolley and Mallowan 1976:29f.). The domestic chapels at Ur had a low shelf or altar for food offerings, ca. 35 cm high, running the length of the wall farthest from the door (see plate 43b, showing bowls that had contained food offerings stacked on a low brick shelf built against the wall). Behind or above the altar was a recessed hearth or niche for burning incense. In the corners of the chapel, at each end of the altar, were brick-built pedestals or tables, ca. 0.6 m square and 1 m high, upon which, presumably, were placed the images of the household gods, or of the dead ancestors themselves.

The presence of such a shrine at Ugarit is perhaps indicated by the two cavities in wall 1006 of room 1045 in House B of the Centre de la Ville, the room in which the *dromos* of tomb 1068 was located (Yon et al. 1987c:76ff.; Salles 1987:162). Wall 1006 is the wall farthest from the door, and the holes in it are only 25 cm above the floor; moreover, the excavators conclude, based on their size and shape, that these holes held short, horizontal wooden beams which supported a low wooden bench or shelf. More specifically, in my opinion, there was a wooden altar for food offerings here, comparable to the strikingly similar low brick altars at Ur, which were built along the wall opposite the door in the room above the tomb. Furthermore, the pottery found in room 1045 included fragments of bowls that may have held offerings for the dead.

Unfortunately, the contents of the tombs themselves have remained almost entirely unpublished.\(^{31}\) It is true that many of the Ugarit tombs were plundered in antiquity; but the skeletal remains left behind by the robbers, although disturbed, are a potentially valuable source of information concerning the number, sex, age, and biological interrelationship of the various persons buried within a given house—especially in view of the fact that it is now possible to determine sex and genetic affinity by means of DNA analysis. Future study of the bones found in tombs at Ugarit will be an important means of determining, in particular, the extent to which biologically unrelated persons were buried together, or, conversely, the closeness of the biological relationships among the deceased cohabitants of the tombs. This information might shed light on the size and composition of the community—the urban clan or patrilineage—that was served by a given tomb, and would be especially meaningful in situations where a single tomb appears to serve a cluster of several houses, as in the Centre de la Ville and the Ville Sud.

We currently know enough to say, however, that the use of intramural tombs at Ugarit, and the ancestor cult that went with it, reflects a strong belief in the importance of the temporal continuity of the household and its physical patrimony. The “living” household was also, in a sense, the household of the dead, because deceased ancestors continued to participate in the social life of their descendants. The corbel-vaulted stone tombs of Ugarit are therefore functionally equivalent to the extramural rock-cut family tombs of Iron Age Israel and Judah, in which multiple burials were made over long periods of time (see Bloch-Smith 1992).

\(^{30}\) Pardee (1996:274f.) makes the very plausible suggestion that the dead king was ritually lowered seven times into the deep shaft that is located between the two large tombs in the royal palace. In this way he was placed “under” his deceased ancestors in the tombs and brought into proper relationship with them.

\(^{31}\) Some of the LB human remains were described in Ugarita IV (see Charles 1962; Vallois and Ferembach 1962), but the skeletal material published there came from only a few tombs, and the methods employed are now obsolete.
In a study of Israelite attitudes toward the dead Herbert Brichto (1973) stresses the role of the tomb in marking family property that could not be alienated because, for one thing, the ancestors were buried there. He also suggests that the biblical injunction to honor one’s father and mother, and the promise of well-being if it were obeyed, originally referred to the afterlife. The dead were to be cared for and in return would provide protection and blessing; thus it is likely that rites, including food offerings and libations, were performed at tombs in ancient Israel as elsewhere, underscoring the ongoing participation of the dead in the life of the family on its ancestral property.

In Ugarit the connection between the dead and the preservation of the family’s patrimony was, if anything, even stronger, because of the presence of the ancestors within the family dwelling itself. For this reason alone it is improbable that nuclear-family dwellings were the norm in the city of Ugarit, as Liverani and others have claimed. If the eldest son did not live with his parents upon reaching adulthood but established his own household, are we to imagine that he moved back into the ancestral home after the death of his father, in order to preserve the connection between the living and the dead members of the family (cf. Salles 1987:160, especially note 5)? This is possible, of course, but a simpler hypothesis would be that he never left, and that the joint-family household was the norm. Moreover, this hypothesis is supported by the archaeological and textual evidence discussed above. That evidence also indicates the importance for the survival of the household of agriculture on hereditary landholdings outside the city. Those landholdings were no doubt an essential part of the family’s patrimony, together with the house itself and its tomb. The archaeologically observed combination of joint-family households, agricultural activity, and intramural tombs within the city of Ugarit therefore accords with textual evidence for the ongoing participation of deceased ancestors in the life of agrarian households.
Chapter 14. The House of ḫūl: Canaanite Gods and Human Society

Ugarit at the end of the Late Bronze Age was the heir of a long-lived West Semitic or “Amorite” cultural tradition that began in the third millennium B.C. Contrary to earlier scholarly conceptions, this was always an urbanized culture in which powerful kings ruled from strongly fortified cities. Transhumant pastoralists played a major role in certain Amorite kingdoms, as the Mari archives attest, especially in the steppe regions of inland Syria. But the “tribal” characteristics of Amorite society were not confined to this nonurban component. It is a false preconception of modern scholarship that traditional forms of kin-based social organization had withered and were merely vestigial among the inhabitants of Bronze Age Levantine cities.

This is not to say that the tribalism of settled communities (whether in walled cities or unwalled villages) was identical to that of widely ranging pastoral groups, who had a quite different mode of subsistence. Kinship relationships, actual or fictitious, seem to have spanned much larger regions among the latter, linking mobile clans of specialized herders into political and military units according to a complex genealogical charter of mutual rights and responsibilities whose scale extended far beyond what was typical among settled populations. Nevertheless, in sedentary communities the idiom of patrilineal kinship was demonstrably still the means by which political and economic cooperation was instituted, both in the organization of everyday agrarian production, distribution, and consumption at the household level, and in royal mobilization of manpower for military and construction purposes.

The doubt that kinship could have been an effective vehicle of social organization in undeniably “urban” centers such as the city of Ugarit is rooted in the urban-rural dichotomy that has been presupposed in much historical scholarship since the Enlightenment (see chapter 6.1). In the preceding chapters I have tried to show that this conceptual dichotomy is contradicted by administrative texts and archaeological evidence from Ugarit and its neighbors. It is contradicted also by the much-discussed Ugaritic literary texts, the myths and epics, in which patrilineal kinship relationships figure prominently. Such texts do not reflect an archaic, nonurban, “tribal” past, for which (as many scholars have assumed) Ugaritians of the Late Bronze Age were nostalgic but in which they did not find the pressing concerns of their own lives. On the contrary, extant Ugaritic literature is the product of a society that had been continuously urban for several centuries, and the stories we find in it were preserved and transmitted over the years precisely because they were meaningful to their speakers and hearers. Beneath all of their imaginative mythological elaborations, the plots of these stories reflect culturally shared “programs of action,” as Walter Burkert has called them, that revolve around the enmities and alliances generated within the extended patriarchal household. In the language of complexity theory, these programs are the “local rules” of social interaction which generate patterns of “global order”; in this case, the pattern we may call the “patrimonial state” (see chapter 3.2).

In chapter 2.3 I have discussed Burkert’s view of “structure and history in traditional narratives” in light of Paul Ricoeur’s distinction between metaphor and symbol. As Burkert (1979:17) has said, a traditional tale “was understood and retold because the members of the audience were potentially active [in the same plot] themselves.” In the same way, a powerful political symbol such as the “house of the father” endures because it is more than a mere metaphor. It is not a free invention of discourse but is “bound” to actual preverbal experience of the world—in this case, the shared experience of life in joint-family agrarian households of the type discernible in the architectural remains of Ras Shamra (see the essay “Metaphor and Symbol” in Ricoeur 1976).

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1 References to the “tent” (ahl) of the divine patriarch ḫūl, as opposed to the term “palace” (ahl), which is more frequently used to describe the dwellings of the gods, may evoke nomadic kings of the steppe in contrast to urban kings. But rulers of both types were contemporaneous; one another throughout the Bronze and Iron Ages, so there is no reason to regard this as an archaic feature without relevance to Late Bronze Age conditions. Many other aspects of the Ugaritic narratives reflect an urban setting in which kinship relationships were nonetheless important.

2 On the early Amorite antecedents of the story about the battle between the gods of storm and sea—a central episode of the Ugaritic Ba’ēl myth—see Durand 1993, and Bordes and Pardee 1993. As Thorkild Jacobsen (1968) proposed, it is likely that this story, known already in the eighteenth century B.C. at Aleppo, if not earlier, was transmitted from the Mediterranean coast to southern Mesopotamia, where it became a central element in the Enina eliš, the Babylonian account of creation (the debate over this hypothesis is reviewed in M. Smith 1994:108ff.).
My basic point, then, is that the programs of action evident in Ugaritic literary texts are best explained in terms of a patrimonial conception of the social order. The same programs are evident in a number of biblical narratives, to be discussed in a second volume on the patriarchal household as fact and symbol in the first millennium B.C. These programs or plots revolve around the problems and conflicts that arise in the course of the household’s lifecycle: (1) the lack of an heir and the quest for an heir (or a wife to provide an heir) in order to ensure the survival of one’s lineage and proper care for oneself and one’s ancestors in the afterlife; (2) the bitter rivalry among potential heirs of the younger generation, which is exacerbated by the favoritism and indecision shown by the aging patriarch and is characterized by factional alliances within his household; (3) the exile or even the murder of disinherited kinsmen; and (4) the senescence of the patriarch and the ascendance of the victorious heir to become the head of his own household—at which point the cycle begins again.

These programs of action are thus subplots within a larger plot formed by the typical lifecourse of the ideal (male) protagonist, whether in the household of an ordinary farmer, in the royal palace, or in the household of the gods—a protagonist whose enactment of this plot confirms the eternal order of the world. I do not mean to say that any one of the Ugaritic narratives contains all elements of this larger plot, but rather that their individual plot structures can be understood in terms of the overarching “natural” lifecourse that is experienced or witnessed in any patrimonial society and provides the symbolic basis for many kinds of social action.

This “patrimonial plot” assumes a society structured as a hierarchy of households, one within another, of which the household of the supreme god is the most inclusive. He is king over all because he is the father of all. Each ruler, at any level of the hierarchy, is the master and father of his subordinates, who owe him filial support and service on a highly personal basis. Kingship and kinship are not in opposition to one another; they are two sides of the same coin. This is possible because kinship is not static, rooted in the facts of biology, but dynamic, employing the genealogical metaphor to represent all manner of social and political relations of authority and dependence. Thus the symbolism of the patriarchal household can be both a motif of literary discourse and a template for practical action. In previous chapters, I have defended this patrimonial model of personalized authority in opposition to prevailing models of Bronze Age Near Eastern society which assume—anachronistically, in my opinion—a much greater degree of symbolic differentiation in this period than is actually attested, and thus wrongly imagine that centralized royal authority was necessarily deployed by means of rationally constructed and relatively depersonalized bureaucracies.

Of course, Ugarit was not necessarily a harmonious or idyllic society. The patrimonial symbolism of the father and his house no doubt served as an ideological distortion to buttress the despotic power of the ruling elite (and of male householders in general), legitimating the existing authority structure even as it provided a symbolic integration of society according to a simple, emotionally satisfying model. The Ugaritic myths and epics were therefore royal propaganda, in some sense. But the patrimonial problems and conflicts they portray, and the patrimonial resolution of these conflicts, stem from a root metaphor flexible enough that it could serve not only to legitimate existing power relations, but also to critique abuses of basic patrimonial values—values that include the duty of familial love and loyalty. The “house of the father” was a durable symbol not just because it was rooted in daily life, but also because it had the potential to be utopian as well as ideological.

Indeed, the capacity of this symbol to combine in one meaningful configuration both love and obedience, even on the broadest social scale, enabled it to fertilize and so perpetuate those patterns of practical action in everyday lived experience from which the symbol itself had grown.

1. ˘Ilù the Bull, the Father of Gods and Men

Aside from the fascinating philological issues and the many details of plot and character in the Ugaritic myths and legends, which have been much discussed over the past several decades, it is worth considering the basic structure of authority that is revealed in these stories. Mythological narratives are often murky refractions rather than direct reflections of mundane social realities, but their anthropomorphic characters and conflicts, however exaggerated and fantastic they may be, were meaningful to their original audience to the extent that they provided a mimèsis—an imaginative redescription—of human social experience. There is no simple one-to-one correspondence in Ugaritic narratives between the divine realm and the world of human beings, but these narratives nonetheless exhibit what Paul Ricoeur, drawing on the work of Mircea Eliade, has called the “logic of correspondences” that is characteristic of polytheistic religion. The “phenomenology of the sacred,” as it has been developed in the comparative history of religions, shows that the manifestation of

Houses and Households in Late Bronze Age Ugarit
numinous power in the everyday experience of the world is founded on “the capacity of the cosmos to signify something other than itself” (Ricoeur 1995: 54). This is apparent in the depictions of the gods and goddesses of Ugarit, who personify forces of nature—the awesome wind, clouds, thunder, rain, and lightning of the storm; the destructive power of the chaotic sea; all-consuming death, against which is deployed the regenerative power of plants, animals, and human beings—and who also personify basic social roles familiar to members of a patriarchal society.

Photograph 4. Seated figure of the Canaanite god 3Ilu found in Late Bronze Age Megiddo
OIM A18316, published in Megiddo 2 (OIP 62), plate 238:30
Courtesy of the Oriental Institute of the University of Chicago.

Photograph 5. Striding figure of the Canaanite god Ba3Ilu found in Late Bronze Age Megiddo
OIM A22467, published in Megiddo 2 (OIP 62), plate 235:22
Courtesy of the Oriental Institute of the University of Chicago.
Preeminent in the literature and cult of Ugarit was the god _Ilú, whose chief titles are “Bull” (gōru), “Father” (abu), and “King” (malku).¹ These are combined in the recurrent phrase tr  il abkh, “Bull  Ilú, your/his father,” and in the fuller expression (said of the god Ba'ilú) tr il abh l il mîlk d ykmnh, “Bull  Ilú his father, King  Ilú who established him” (KTU 1.3 v 35ft.).  Ilú is the begetter of all who are born, the “producer of progeny” (bn'y bnut), the “father of gods” (ab ilm) and the “father of humankind” (ab adm). In his capacity as progenitor he plays an important role in the two Ugaritic “epics” involving human protagonists, the stories of Dā'nîlû and Kirta.⁴ In both of these stories a concern for the preservation of the patrilineage is paramount in the face of the catastrophic problem of the lack of a male heir. In the story about Dā'nîlû (the “Epic of  Aqhatu”), Ba'ilú intercedes on his behalf with  Ilú his father, with the result that  Ilú helps Dā'nîlû to impregnate his wife. In the Kirta epic  Ilú instructs King Kirta (whose wives and brothers had all died) to march forth with his army in order to obtain a child-bearing wife.

In his study of the story about Dā'nîlû and his son  Aqhatu, Simon Parker (1989:104ff.) discusses a number of striking parallels to this theme of the lack of an heir which are found elsewhere in ancient Near Eastern and biblical literature. But in answer to Parker’s question: “why was this work elevating traditional, family responsibilities recorded by the scribe laureate of Ugarit [Ililmiku], and kept in the residence of the high priest of Ugarit? Is there not a conflict here between the institutions of the poem and those of the Ugaritic monarchy?” (ibid., p. 143), we can question his and other scholars’ assumption that patrilineal kinship could not have been important in the urban, monarchical society of Late Bronze Age Ugarit.⁵ The patrimonial interpretation of the architecture and administrative texts of Ugarit that I have defended at length in previous chapters (on independent grounds, without regard to the interpretation of Ugaritic literature) contradicts Parker’s assertion that “the contrast between the world of [Dā'nîlû] and that of the archaeology and archives of Late Bronze Ugarit remains—not to mention the world of [Kirta]” (ibid.; see also his concluding remarks in pp. 217–20). Parker greatly exaggerates the difference between the “literary world” of the Ugaritic stories and the “real” social world. It is true that in certain kinds of literature, especially in modern poetry, there is a purposeful suspension of direct reference in order to create an imagined world with its own values; but this is by no means true of all or even most traditional literature from the ancient Mediterranean. In the case of Ugarit, there is no need to posit this sort of radical break between literary and social worlds, in light of the substantial independent evidence I have cited that reveals a social world congruent with the literary world of Dā'nîlû and Kirta.⁶

In addition to being the father of all,  Ilú is also the supreme ruler. He heads the council of the gods and they defer to his wise judgment. But he is depicted as an elderly gray-bearded man who is capable of being manipulated, especially by his wife and daughters. He enjoys the pleasures of a ripe old age, including getting drunk and dallying with young women. He is carried home when he is drunk by the double deity Tukumuna-wa-Šûnama, who thereby fulfill one of the duties of the ideal son as presented in the story of Dā'nîlû and  Aqhatu (discussed above in chapter 13.4).⁷  Ilú is depicted as a man full of years and hence close to death; thus in the Ba'ilú myth, as I will argue below, the problem that drives the plot is the struggle among his sons to succeed him as patriarch. This is the social context in which the eruption of rivalries among these gods make sense. A similar problem emerges in the Kirta epic, when Kirta’s son Yaṣṣubu—apparently the heir whom Kirta had so desperately sought—is impatient for the death of his enfeebled father, whose kingship he wishes to inherit. The text as we have it ends tragically with a scene in which Kirta curses his son for his disloyalty (cf. the

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¹ On the epithets of  Ilú in Ugaritic and later NWS texts see Cross 1974.

⁴ These stories are translated with detailed notes and bibliography in Pardee 1997a and 1997d; see also Wyatt 1998.

⁵ Note that the scribe Ililmiku, and thus the extant copies of the stories which bear his name, should now probably be dated to the reign of Niqmaddu III (ca. 1210–1195), as opposed to the reign of Niqmaddu II in the fourteenth century B.C. (see Pardee 1997b:241 n.3). There is even less reason now than previously to think that such narratives were viewed at the end of the Late Bronze Age merely as archaic “classics” that belonged to an earlier era.

⁶ I also find doubtful Parker’s (1989:217) conclusion that “the two poems [about Dā'nîlû and Kirta] present us with quite different interests and values,” in addition to his conclusion “that the interests and values of each stand in a relation of contrast with what we know of Ugaritic culture.” This is not the place for a detailed analysis of the two stories, but it seems to me to be quite obvious that they reflect the same patrimonial interests and values, which are reflected in the problematic situation of the lack of an heir with which both stories commence.

⁷ See Pardee 1988:59ff.; also 1997a:344 n.9; 1997b:245 n.30 (cf. Wyatt 1998:46 n.39; 410 nn. 35, 37). With this role of Tukumuna-wa-Šûnama in mind, Pardee defends the view that  Ilú’s title ab šmm means “father of Šûnama” and not “father of years,” as many have thought.
bibilical story of King David’s rebellious son Absalom—see Parker 1989:199ff. and references there).

.Repositories: 1lu is the father of humankind as well as father of the gods. It seems, however, that the human king was regarded as the son of 1lu in a special sense. King Kirta is “the goodly lad of 1lu” (nēmn ilym l). He is described as a favored dependent of “the Bull, his father” (tr abh), using the same expression as is used with respect to the gods as sons of 1lu. There are many parallels to this in the ancient Near East, of course. In the prologue to the Old Babylonian laws of Hammurapi, that king is said to be “engendered” or “begotten” (Akk. banū) by the Mesopotamian moon god Sin and, in another passage, by the Amorite god Dagan. In a famous biblical psalm, Yahweh says of Ilu is the father of humankind as well as father of the gods. There has been no lack of commentary on these and other similar texts from many times and places in the ancient Near East. The point to emphasize here is simply that human political rule, at Ugarit and elsewhere, finds its justification within the framework of a cosmos structured as a patrimonial hierarchy of households.

2. Ba’lu and His Rivals: Authority and Autonomy in the Family of the Gods

Three levels or layers of correspondence between the gods and human experience are discernible in the longest composition known from Ugarit, the great Ba’lu myth (translated, with detailed notes and bibliography, in Pardee 1997b). The two basic episodes of this myth are the battle between the storm god Ba’lu (“Lord” or “Owner,” originally an epithet of the old Amorite weather deity Haddu; see Greenfield 1995) and his rival Yammu (“Sea”), and a similar struggle between Ba’lu as bringer of rain and fertility and his even more terrifying enemy Mōtu (“Death”). Ba’lu emerges victorious from both battles.

At one level, then, there is the personification of natural phenomena, giving narrative form to the perennially observed conflict between fierce but fructifying thunderstorms and the roiling Mediterranean Sea, and to the similar seasonal struggle between sterility and fertility in the agricultural cycle and in the birth and death of animals and people. On the same plane is the personification of collective social phenomena. Warfare, in particular, which unpredictably threatens human well-being, is represented by the treacherous and blood-thirsty goddess Anatu. It is for this reason that she violates all social norms for female behavior. In the Ba’lu myth her gruesome exploits are described as follows:

She smites the peoples dwelling on the seashore, wreaks destruction on the humans in the east.
Under her are heads like balls, above her are hands like locusts, heaps of fighters’ hands are like heaps of grasshoppers.
She attaches heads around her neck, ties hands at her waist.
Up to her knees she wades in the blood of soldiers, to her neck in the gore of fighters.
With (her) staff she drives out the (potential) captors, with her bowstring the opponents.

On a second level of correspondence, the Ba’lu myth depicts political relations among kings. Various gods are described as rulers of their own geographically distinct domains. They each have a palace or acquire a palace in a specific location. For example, Anatu’s palace is on Mount Ḫnibubu, and in Ba’lu’s case it is Mount Șapānu, the northern mountain which is the “Mount Olympus” of the Canaanite pantheon. In the Aqhatu epic we read that Prince Yariṣu, the moon god, rules the city of Șabilima (the “city of mourners”?; see Pardee 1997a:349 n.61). In the Ba’lu myth Prince Ba’lu (zb ʾbl b’l) fights against Prince Še’a/Ruler River (zb ʾml ʾp’lnhr) for the right to rule the gods. After that battle Ba’lu is established as king in his own palace, whereupon he holds a great feast for allied kings (his “brothers”) and takes possession of many cities. Later he sends his

8 This aspect has been emphasized in several studies of the myth. See M. Smith 1994:58–114 for a detailed review of scholarly interpretations.
9 It would be more accurate to say that Mount Olympus is the “Mount Șapānu” of the Greek world. Mount Șapānu is to be identified with the prominent 1,800-meter peak of Jebel el-Aqrāc (classical Mons Casius), near the Mediterranean coast ca. 40 km north of the city of Ugarit, at the northern boundary of the kingdom of Ugarit. See Burkert 1985:126 on the Mount Olympus of later Greek religion, the mountain in the north where storm clouds gather; this was the home of Zeus, god of storm and rain, “hurler of thunderbolts,” who has obvious similarities to Canaanite Ba’lu.
messenger to Môtu, the fearsome king of the city of the dead in the underworld, who did not attend Ba‘lu’s feast and does not recognize his suzerainty. Such ambassadors regularly go back and forth between the palaces of the gods in the Ba‘lu myth and other Ugaritic narratives. With ‘Anatu’s help Ba‘lu is eventually victorious even over Death, at which point he kills his remaining rivals among “the seventy sons of 2Atiratu,” chief wife of 3Ilu, and consolidates his now undisputed sovereignty.

On a third level of correspondence, however, the relationships among the gods resemble the typical kinship relationships and factional rivalries found in Mediterranean joint-family households. Each god conforms to a recognizable social type: ‘Ilu is the aging patriarch; his wife 2Atiratu is the matriarch of the family; Yammu and Môtu are sons of 3Ilu, and Ba‘lu falls into the same category, despite his title “son of Dagan,” because the “seventy sons of 2Atiratu” (and of ‘Ilu) are called his brothers (KTU 1.4 vi 44–46) and there is a reference to ‘Ilu as “his father” (KTU 1.3 v 35).

Ba‘lu’s title “son of Dagan” has puzzled scholars, for the god Dagan has no role in any of the main Ugaritic narratives. He is an old Amorite deity who was prominent in the middle Euphrates region, at Mari, Têrga, and Tuttul, and in inland Syria, as is shown by the documents from Ebla (see Healey 1995a). Moreover, in addition to being equated with the sons of ‘Ilu in the Ba‘lu myth, Ba‘lu addresses ‘Ilu as “Bull ‘Ilu, my father” in the 2Aqhatu epic (KTU 1.17 i 23), where he is also explicitly called “the son of ‘Ilu” (KTU 1.17 vi 29).

This discrepancy can be explained in one of two ways: (1) the reference to Ba‘lu as son of Dagan is a vestige of a different mythological tradition, perhaps originating in inland Syria—a tradition that is neglected in the extant version of the Ba‘lu myth, in which the god Dagan does not appear; or (2) the title “son of Dagan” reflects an awareness already in the Late Bronze Age of a longer family history of the Canaanite pantheon similar to what is recounted in the Ba‘lu myth. In the household of ‘Ilu, Dagan was patriarch of an extended family that included his half-brother and nephew Ba‘lu, the adopted son of Dagan, as well as sons of his own—Yammu and Môtu—who were Ba‘lu’s cousins and nephews.

The advantage of this explanation of Ba‘lu’s parentage as son of Dagan is that it accounts for the bitter rivalry between Ba‘lu and his agnatic relatives in the household of ‘Ilu. As a son of Heaven himself and younger brother of ‘Ilu, Ba‘lu could claim ‘Ilu’s patrimony as his own rightful inheritance. On the other hand, as full sons of the patriarch ‘Ilu, Yammu and Môtu could claim the same inheritance.

Whether or not these are the precise family relationships that the author of the Ba‘lu myth had in mind, hearers of the Ugaritic myth would have gained a vivid sense of a typical household struggle of the sort familiar in mundane social life. The goddess 5Anatu is depicted as a young woman (btlt), a virgin or at least an unmarried female, who is Ba‘lu’s loyal sister, aiding him in the course of intrahousehold factional rivalries. She intercedes on his behalf with ‘Ilu, and she persuades 2Atiratu to do the same. But 2Atiratu subtly supports her own sons against the claims of Ba‘lu, and this may explain ‘Ilu’s initial failure to defend what Ba‘lu regards as his rightful status. As Pardee says, “The depth of 2Atiratu’s real hostility to Ba‘lu and 5Anatu’s recognition of that hostility are both evident in the form of 5Anatu’s announcement of Ba‘lu’s death to ‘Ilu: ‘So now let

11 Compare the love shown to 2Aqhatu the “youth” (gzr), by his sister Pūgatu (pgt), whose very name refers to the age-category of adolescent females found in the household census text RS 11.857, discussed above in chapter 13.1. In some ways Pūgatu is similar to 5Anatu, for each avenges the death of her brother. On the term btlt, see Pardee 1997b:243 n.11 and references there.
2Aširatu and her sons rejoice, (let) the goddess (rejoice) and the host of her kin; for Mighty Ba’lu is dead, perished the Prince, master of the earth” (Pardee 1997b:256 n.121).

In iconography from Ugarit and elsewhere in Syria-Palestine, the god ʿIlu is portrayed as an old man dressed in flowing robes, seated on a chair with his hand raised in blessing. The god Ba’lu, on the other hand, is portrayed as a young warrior, stripped to the waist for battle, striding forward while brandishing a mace or a lightning-bolt spear (see photographs 4 and 5 above; for other examples see Negbi 1976). In the Ba’lu myth, Yammu and Môtu, Ba’lu’s main rivals, are also presented as young men of military age, in the same age group and social category as Ba’lu; thus they are called ǧer, “youth” or “hero.”

On the natural level of mythic correspondences, the battles between Ba’lu and his rivals represent the great clashes between perennial forces of nature, namely, storm and sea, fertility and death. On the political level they represent the struggles for supremacy among kings, with which Ugaritans of the Late Bronze Age were intimately acquainted, as they witnessed and participated in the imperial rivalries between Egypt and Hatti, and as their own kings jockeyed for power with their Syrian neighbors. And on the familial level, the sons of ʿIlu vie for preeminence in the patriarchal household, aided by their female relatives. They defer to the authority of their aging father, but they are impatient to succeed him as patriarch so that they can have a house of their own.

I have argued elsewhere (Schloen 1993) that the rivalry between junior kinsmen within a joint-family household can also be seen in two enigmatic myths: KTU 1.12, “The Devourers” (Wyatt 1998:162–68), and KTU 1.23, the story of “Dawn and Dusk” (translated in Pardee 1997c and Wyatt 1998:324–35). In both of these texts the patriarch ʿIlu impregnates human females who are in the role of low-status household servants, whereupon he sends them and their bastard children into exile in the wilderness (cf. the biblical story of Hagar in Genesis 21). In KTU 1.12, in particular, Ba’lu encounters ʿIlu’s voracious offspring in the wilderness (or seeks them out) and kills them, whereupon he apparently must make atonement because “he is clothed as in a garment in the blood of his brothers” (KTU 1.12 ii 46; see Wyatt 1998:166). The rivalry between the heir to the patrimony and his dispossessed kinsmen is characteristic of the social tensions endemic in a patrimonial society. The various Ugaritic myths capture very well the common human experience that brothers are not necessarily allies, as in the idealized Bronze Age political terminology of brotherhood discussed in chapter 12.1, but may also be one’s worst enemies and rivals. The only thing that can surpass the love between brothers is their hatred of one another as they vie for supremacy in the household.

Each of the three layers of meaning in the Ba’lu myth has its own specific “logic of correspondences,” forming a unified, internally consistent pattern. But these three layers coexist in the same narrative, and as a result they sometimes interfere with one another. The political level interferes with the familial most noticeably in that the gods as independent kings do not live together in one household, despite their familial ties, but have separate territorial domains. Most tellingly, even the matriarch 2Aširatu has a dwelling separate from that of her husband ʿIlu, although this feature of the myth is sometimes overlooked. Mark Smith (1994:xxiv), for example, states incorrectly that: “As the wife of El, Athirat is not accorded a separate abode.” In one scene it does appear at first glance that 2Aširatu lives with ʿIlu, for whom she prepares a warm drink (KTU 1.4 ii 3–11). But as Pardee says: “The picture is that of 2Aširatu expecting a visit from ʿIlu, whose domicile, as is clear from the continuation of the story, is at a distance from ʿAširatu’s” (1997b:257 n.135). As the story continues, 2Aširatu is surprised and frightened to see Ba’lu and ʿAnatu arriving instead. They give her presents and prevail on her to intercede with ʿIlu on Ba’lu’s behalf, whereupon she travels some distance by donkey to visit ʿIlu at his home.

The natural level also interferes with the familial. In a classic narrative topos, the stage for intrafamilial rivalry is set by ʿIlu’s favoritism, first toward Yammu and then toward his “beloved one” (ydd), as Môtu is called, stacking the odds against Ba’lu. But this is contradicted by ʿIlu’s provision of a palace for Ba’lu and by his great grief at Ba’lu’s death, which are explicitly motivated by the storm god’s essential role in the cosmos as the bringer of rain. When ʿIlu authorizes the construction of a palace for Ba’lu he is congratulated by ʿAširatu, of all people (here abandoning her role as supporter of her own sons in their rivalry with Ba’lu), with the words: “For now Ba’lu can send his rain in due season” (KTU 1.4 v 6f.). Similarly, after Môtu kills Ba’lu, ʿIlu appeals to the all-seeing sun-goddess Šapšu in the hope that Ba’lu has been revived, sending a message to her that says (probably echoing a traditional cultic liturgy):

Dried up are the furrows of the fields, O Šapšu,
Dried up are the furrows of ʿIlu’s fields,
Ba’lu is neglecting the furrows of the plowland.
Where is Mighty Ba’lu?
Where is the Prince, master of the earth?

[Translated in Pardee 1997b:271]
Likewise,  anatu is a beautiful young woman and faithful sister, but as the personification of the collective social phenomenon of warfare she constantly violates the norms of female behavior in a patriarchal society, delighting in slaughter and threatening to smash the head of her father  ilu and make his gray hair flow with blood. In the same way, the goddess  aṭṭartu (Astarte)—depicted in the nude with prominent genitalia in Canaanite iconography—personifies sexuality in a rather exaggerated fashion, although she perhaps conforms better to human social norms than does  anatu, if we understand her role in the myths to be that of a courtesan or prostitute.

Most perplexing is the logical contradiction on both the familial and political levels that is entailed in  ba’alu’s triumphant ascendance to preeminence among the gods, which implies the death or displacement of  ilu. On the familial level, we would expect  ba’alu to become head of his own household only after  ilu has died, just as on the political level his kingship over the gods is incompatible with  ilu’s kingship.  But the chief god  ilu, who is the personification of all creative and generative power, cannot die and depart the scene—at least not in Bronze Age Ugaritian religion, as opposed to later Canaanite, Israelite, and Greek religion, in which the figure of the divine patriarch merges with that of the young storm god.

What we have, then, is a “frozen” configuration of divine characters around which the narrative must flow.  ilu will always be the wise and aged patriarch, still recognized as supreme but about to depart the scene; and  ba’alu will always be the youthful son and heir, at the peak of his military strength and skill — always on the brink of his accession to authority in the household, and thus the symbol of joyous vigor and rejuvenation. Rather than trying to harmonize the ambiguous relationship between the two chief gods on a single level of correspondence, it is better to see in  ilu and  ba’alu two enduring social types. These two characters, who dominate the  ba’alu myth (and, it seems, the daily cult of Ugarit), capture fundamental experiences of fatherhood and sonship and their dynamic interrelation that constituted the social world of those who sacrificed to these gods, heard and recited the myth, and wrote it down. Perhaps that is why it was this particular stage in the family history of the gods that captured the attention of those who recorded and preserved the story of  ba’alu’s struggle for power.  

The contradictions produced by the interference of these distinct levels of meaningful correspondence between the gods and human experience are not random but predictable. Viewed from the correct perspective, the seemingly incoherent myth resolves itself into three coherent layers of meaning whose interaction with one another can be explained quite straightforwardly. The story is not an accidental accretion of narrative elements, yielding an illogical tale; nor are there any grounds for a forcible harmonization of conflicting narrative elements according to a single pattern of correspondences. Each level must be taken seriously on its own terms. But the three levels of correspondence in the myth also work together to produce a single foundational narrative that is (at least implicitly) cosmogonic, affirming the rightness of the entire natural and social world of human experience. The impersonal natural cycle of the seasons and powerful natural and social phenomena—fertility and death, storm and sea, warfare and sexuality—are humanized via narrative emplotment in terms of programs of action motivated by the perplexing aporias of human social existence, both on a local, familial scale, and on a wider political scale. The  ba’alu myth achieves a poetic mimēsis of reality by simultaneously redescribing both natural phenomena that elicit numinous awe and the inescapable problems of life and death in a patrimonial society.

Thus Ugaritic narratives reveal a patrimonial worldview that matches the patrimonial social organization detected in administrative texts and urban architecture. Lowell Handy (1994) has, however, defended the proposition that the Syro-Palestinian pantheon reflects the bureaucratic structure of mundane society. Handy cites Weber explicitly and he intentionally uses the term “bureaucracy” in the technical sense of a rationalized mode of social and political organization. In my view, this application of the concept is anachronistic and improbable. Indeed, until the latter part of the first millennium B.C., when Near Eastern stories of divine conflict in apocalyptic literature picture grand battles between multitudinous heavenly armies, we find no narrative echo of imper-
sonal bureaucracy as the basis for social order. In Ugaritic myths the battles between the gods are a matter of single combat, and their social context is the conflict generated among patrilateral kinsmen in an extended patriarchal family.

The *topos* of single combat is also found in the Middle Kingdom Egyptian “Story of Sinuhe” (trans. in Lichtheim 1973:222–35), set in the period of the early Twelfth Dynasty (ca. 1950 B.C.). In his adventures in a deurbanized Canaan (the description of which corresponds very well to what is known of the “Early Bronze IV” archaeological period), the exiled Egyptian official Sinuhe marries the eldest daughter of the ruler of “Upper Retenu” (in inland Palestine or southern Syria) and is given his own territory to rule. He is set at the head of the ruler’s children, presumably having been designated as heir to the kingdom; but as an outsider he arouses animosity, and eventually he is challenged by the “hero of Retenu,” whom he beats at his own game. Sinuhe defeats and kills the native challenger in single combat and takes possession of all his property—the fate that had been planned for him (see also Redford 1992:83–87). Although there is no question of direct literary dependence, the parallels between this episode in the story of Sinuhe and Baʿlu’s combat with his rivals in the Ugaritic Baʿlu myth are striking. Traditional plots of this sort reflect and validate a “tribal” authority structure based on familiar household relationships. The durability of this mode of legitimation in the Bronze Age Near East, in both rural and urban settings, is due to its invocation of a common political symbolism applicable at all levels of society, from the king’s house to the humblest home in the kingdom.
CONCLUSION

In this volume I have attempted to integrate a detailed discussion of archaeological, demographic, and economic aspects of the ancient patriarchal household (the “facts”) with a discussion of its symbolic significance, as seen from the vantage point both of political sociology and of the history of religions. This multidimensional and dialectical approach is required, in my view, by the philosophical and sociological considerations I have explored in the opening chapters of the book. On the empirical side, the Late Bronze Age kingdom of Ugarit in northwest Syria constitutes an excellent historical case for this kind of study because it has yielded sufficient archaeological and textual evidence to permit us to examine the Ugaritian “house of the father” as both fact and symbol. By relating the architecture of houses and neighborhoods to contemporary legal and administrative documents, on the one hand, and to the characters and plot structures of Ugaritic narrative literature, on the other, we can uncover and explain typical patterns of social action in order to understand better the ancient lifeworld to which they refer.

In putting it this way, I am drawing on Paul Ricoeur’s important distinction between the internal “sense” and the external “reference” of human works, discussed in chapter 2. The structural sense is to be explained in objective correlational and genetic terms, not as an end in itself, but in order that its reference to a human lifeworld may be subjectively understood. We can say that the immanent sense of the Ugaritic Ba’lu myth, in particular, consists of three interacting “logics of correspondences” between the human world and the sacred, operating on the natural, political, and familial levels. But this structural sense of the myth, outlined in chapter 14, has an external reference to a time-bound way of being-in-the-world (to use Heidegger’s term), just as the patrimonial social structure adumbrated in the myth and in other texts is, like all social structures, an attempt “to cope with existential perplexities, human predicaments, and deep-rooted conflicts” (Ricoeur 1991:166).

The mute domestic architecture of Ugarit has an internal structural sense also, a logic of organization of the kind much discussed in recent “interpretive” archaeology by Ian Hodder and others. But these material remains are not themselves a “solid” metaphor; they have an external reference to a way of being-in-the-world only through the agency of a linguistic symbolism that is based on the root metaphor of the “house of the father.” That symbolism links pre-reflective embodied existence within the physical house—the lived experience in which the metaphor itself is rooted—to a repeatable, shareable meaning that is available for us to interpret. Human social action is mediated and constrained by such meanings, and our study of such action, and of historical changes in habitual patterns of action, must take account of the ways in which ordering symbols are interpreted and reinterpreted from generation to generation.

This is where Max Weber’s sociohistorical methodology comes into play, acknowledging as it does the shaping effect of the material and economic conditions of human social life, which must be studied from a nonhermeneutical external perspective, while at the same time modeling the subjective meaning of human behavior in terms of ideal types of motivated action that we as human beings can intuitively understand. Enduring changes in these types of action are the product of a complex and inherently unpredictable dialectic of fact and symbol, for which nonlinear “complex adaptive systems,” so prominent in recent archaeological and social-scientific discussion, provide a useful heuristic analogy from the realm of the natural sciences.

In pursuing this sociohistorical approach, I have endeavored to show that the textual and archaeological evidence from ancient Ugarit and the rest of the Bronze Age Near East conforms quite well to Weber’s patrimonial household model, rather than to a functionalist bureaucratic model of ancient society, or to the two-sector Marxist model of the “Asiatic mode of production.” The homology between center and periphery that this implies for the Bronze Age Levant has implications also for the understanding of Iron Age social and political developments. In particular, the emergence of Israel in Canaan can no longer be seen as a kin-oriented “tribal” reaction against the “urban” ethos of the Late Bronze Age city-states. No urban-rural dichotomy existed in Ugarit—a Bronze Age urban center par excellence—or, by extension, in the rest of Syria and Palestine. The kinship ethos and agrarian culture of early Israel is therefore a mark of similarity, rather than of difference, between the Israelites and their Canaanite forebears.

Furthermore, if Ugarit and other second-millennium Near Eastern kingdoms can be best understood
in terms of the patrimonial model, then by the same argument the kingdoms of Israel and Judah and other “national” polities in Iron Age Palestine were also patrimonial regimes, at least until the advent of the Assyrian empire late in the eighth century B.C., which damaged and in some cases destroyed the existing social and political fabric of the Levant. Kingship in Israel did not replace “egalitarian” kinship, as some have argued. Rather, the emergence of monarchy in Israel in the tenth century B.C. entailed a reconstruction and territorial expansion of smaller-scale kin-based modes of organization (at the expense of local autonomy, to be sure) without, however, requiring a fundamental symbolic or ideological transformation (see Stager 1998:149–51; 171f.). We must look to a later period of Israelite history for evidence of the symbolic innovation that undeniably occurred.

That symbolic transformation is evident in biblical literature. As we have seen, Ugaritic narratives conform to the logic of correspondences that is characteristic of polytheistic religions. In Ricoeur’s essay “Manifestation and Proclamation” (reprinted in Ricoeur 1995), this logic is contrasted with the “logic of limit-expressions” that is characteristic of Jewish and Christian narratives and paranetic discourses, beginning with the Israelite and Judahite prophets of the Assyrian period (see also Ricoeur’s comments on limit-expressions in his essay “Naming God,” reprinted in Ricoeur 1995:228ff.). In Judeo-Christian religion, myth is “broken” and the world is desacralized by the paradoxical and hyperbolic use of traditional modes of discourse. The intensification wrought by limit-expressions points not to correspondences between the cosmos and the numinous, but to personal “limit-experiences,” whether of crisis or culmination. Thus they call for a “hermeneutic of proclamation” rather than a “phenomenology of the sacred.”

But Ricoeur goes on to propose a mediation between these poles, between the sacred and the kerygma, the numinous and the word, noting that cosmic symbolism was not abolished in biblical discourse but rather “reinterpreted according to the requirements of proclamation” (p. 65), namely, the hyperbolic proclamation of a desacralized monotheistic world. As he says: “Everything indicates therefore that the cosmic symbolism does not die but is instead transformed in passing from the realm of the sacred to that of proclamation. . . . Every new language is also the reemployment of an ancient symbol” (p. 66).

This is especially true of the symbol of the “house of the father,” which was so fundamental to Bronze Age Canaanite society and was by no means abolished in later Israelite and Jewish culture. Not surprisingly, the fatherhood of God was rarely invoked by the biblical prophets and many subsequent Jewish writers, intent as they were on the elevation of Yahweh to absolute supremacy through the desacralization of the mundane sphere, in opposition to traditional Canaanite religion. But this ancient symbol was not forgotten, and in certain circles it again came to the fore, having been radically transformed as a vehicle of kerygmatic proclamation (see the essay “Fatherhood: From Phantasm to Symbol” in Ricoeur 1974; cf. Strotmann 1991; Vanoni 1995).

The complex history of this transformation is the subject of a planned second volume on the “house of the father” in the ancient Levant, under the rubric Tradition and Rationalization in the Axial Age. As that title indicates, the transformation of the symbol will be explored in terms of the Weberian concept of “rationalization.” In chapter 5 above I have made the point that rationalization operates on a number of interacting social levels and in a variety of social contexts. We must pay attention not just to the theoretical rationalization carried out by priests, prophets, and other intellectuals, but also to practical rationalizations motivated by pragmatic self-interest under changing material conditions. If we do this we will not lose sight of the irreducible dialectic between the patriarchal household as a socioeconomic fact and as a religio-political metaphor that is rooted in that fact.
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sōkinu “royal representative, commissioner” (see Ug. šin /sākinu/; note that in Amarna letters EA 256:9 and 256:9 maškīmaššu is glossed by sī-ki-ni and sī-ki-na, respectively [Moran 1992:xxvi n.70]), 252, 315

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