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ARCHAEOLOGY AND THE ISRAELITE “CONQUEST”

The purpose of this article is (1) to summarize and evaluate recent archaeological data that bear on the early Israelite settlement in Canaan in the 13th–11th centuries B.C.; (2) to critique current explanatory models of Israelite origins in the light of this newer data; and (3) to suggest ways of harmonizing the archaeological and textual evidence that may eventually lead to better syntheses. The approach throughout is heuristic, rather than definitive in attempting solutions.

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A. Issues in the Discussion

The problem, although exceedingly complex, may be stated rather simply by focusing on the several levels at which the majority of scholars encounter difficulties. There is first the *historical* problem, which faces the task of ascertaining, if possible, what really happened in the Israelite settlement of Canaan, that is, of getting at the “history behind the history.” It is now recognized by nearly all that the biblical

tradition concerning Israel's emergence in Canaan, while our only direct literary source, is limited by its nature as theocratic literature—particularly the “conquest” version in Joshua. But is it possible to write a “secular history” of Palestine in this period that will elucidate Israelite origins better by placing them in a larger context, based chiefly on archaeological evidence?

This leads to a *methodological* problem. What are the possibilities and limitations of each of the two basic types of data from which history may be reconstructed? In short, how are the two histories related, if at all? Are they parallel, or does one take *precedence*?

Finally, there is what appears to be, at least, a *theological* problem. In Israel's recitation of the “mighty acts of God,” the central events are Yahweh's redemption of the Israelites from Egyptian bondage, his subsequent granting of miraculous victory over the Canaanites to the Twelve Tribe League, and finally his deliverance of the promised land to the settlers as the sign and seal of his covenant with them. These are the formative events that constitute Israel's existence, the very heart of her Credo. But if these events have no basis in the actualities of history, is not Israel's faith, and ours, without foundation? As one noted biblical archaeologist and theologian, G. E. Wright, put it in his classic *God Who Acts: Biblical Theology as Recital*: “In Biblical faith, everything depends upon whether the central events [i.e., Exodus-Conquest-Settlement] actually occurred” (1952: 126). But what if they did *not* occur?

B. An Archaeological Critique of the Conquest Model

The regnant theories that attempt to explain the emergence of Israel in Canaan are too well known to need documentation here. They are: (1) the “conquest” model, espoused chiefly by Albright and his followers in America, as well as by Yadin and several Israeli scholars; (2) the “peaceful infiltration” model, first proposed by Alt and the German school in the 1920s, but still widely influential; and (3) the “peasants' revolt” model, introduced originally by Mendenhall in the 1960s and recently elaborated by Gottwald. All of these models make some use of the archaeological data, but only the first is heavily dependent upon such evidence. Yet because these are models developed and employed mainly by *biblical* historians, the pertinent archaeological data have not always been adequately evaluated. As Syro-Palestinian archaeology and biblical studies have increasingly diverged (see ARCHAEOLOGY, SYRO-PALESTINIAN AND BIBLICAL), such a critique becomes a task for specialists. Let us look first at the “conquest” model. This model presupposes several sequential phases, each of which, however, presents archaeological difficulties.

1. Israel in Egypt. As is often observed, there is no direct archaeological evidence that any constituents of later Israel were ever in Egypt. The only Egyptian textual reference, the well known “Victory Stela” of Merneptah (now dated ca. 1207 B.C.; see further Stager 1985b) mentions “Israel” as a “people,” probably an ethnic element, not in Egypt but in Canaan, with no apparent knowledge of any Egyptian derivation. Nor is there anything in the material culture of the early Israelite settlements in Palestine that points to an Egyptian origin for that culture. The few Egyptian scarabs and possible house-types (as at Tel Masos; Fritz and Kempinski 1983) can easily be explained by the continuity of Egyptian elements from the local LB Canaanite culture into the early Iron Age.

Among the scant references in the Hebrew Bible to specific details of an Egyptian sojourn that might be identified archaeologically is the reference to the Israelites being in servitude in the Delta cities of Pithom and Rameses (Exod 1:11). Pithom is possibly to be identified with Tell el-Maskhuta, or with Tell el-Retabe (Holladay 1982: 3–6); and Rameses has now almost certainly been located at Tell ed-Dab.a near Qantir by the recent excavations of Manfred Bietak (1979). The significance of the new data is considerable. First, all three sites are among the few Delta sites that are now known from recent excavations to have been Canaanite colonies in Egypt in the Middle Kingdom (Dynasties 12–17, ca. 1991–1540 B.C.). Tell ed-Dab.a was, in fact, the Hyksos capitol of Avaris, destroyed ca. 1540 B.C. with the expulsion of the Hyksos at the beginning of the 18th Dynasty. Second, these sites also have Ramesside levels of the 13th or 12th centuries B.C. Thus Tell ed-Dab.a, although deserted throughout the New Kingdom after its destruction, was *reoccupied* precisely in the time of Rameses II, in the early–mid-13th century B.C. (Rameses II = 1304–1237 B.C.). (Cf. Bietak 1979; but see Dever 1985a for the raising of dates for the earlier levels.) Tell el-Maskhuta has no known Ramesside occupation, but Tell el-Retabe,

like Tell ed-Dab'a, was occupied in the Middle Kingdom, abandoned in the New Kingdom, then reoccupied in the 20th Dynasty and onward (ca. 1200 B.C. onward).

Is it merely fortuitous that these Delta sites, known to the biblical writers, *did* have a substantial Canaanite presence in the so-called Patriarchal period, and then were rebuilt under Egyptian aegis in Ramesside times, which is when an Israelite sojourn in Egypt would have to be placed archaeologically? The new evidence is not conclusive, of course (i.e., there are questions regarding the exact location and date of Pithom), but it may lend support to the long-held view of some biblical scholars that at least *some* constituent elements of later Israel had actually stemmed from Egypt, i.e., the “House of Joseph.” Only one thing is certain, and that is that the scant Egyptian evidence at least points unanimously to a 13th century B.C. *date* for an Israelite “exodus,” if any. (See Hermann 1973: 19–50; Miller *IJH*, 246–52; contra Bimson 1981.)

2. The Sinai Tradition. The “crossing of the Red (Reed) Sea” is obviously a miraculous tale that can in no way be validated or even illuminated by archaeological investigation. Furthermore, of the subsequent “wandering in the wilderness” theme (Num 33:1–49), little can be said archaeologically. If indeed the Israelites are to be pictured as a band of wanderers, or even as semi-sedentary pastoralists, we would still probably find no remains of their ephemeral camps in the desert. Thus all attempts to trace the “route of the Sinai crossing” have been doomed to failure, reduced as they are to inconclusive efforts to identify hazy topographical references in the Bible with modern Arab place names that usually have no clear historical associations. E. Anati has recently claimed (1986) that he has located biblical Mt. Sinai at Har Karkom, in the W Negeb near the present Egyptian border, but few will find the petroglyphs and other data convincing evidence that this is anything more than another of the “holy mountains” frequented from time immemorial by the nomads of the desert.

The only 2d millennium B.C. Sinai route that is attested archaeologically is the N route along the coastal dunes, which recent archaeological investigation has indeed illuminated, precisely in Egyptian New Kingdom times (Oren 1984 and references there). But this is precisely the route that was bypassed according to the biblical tradition, because of Egyptian control. All we can say is that recent, extensive exploration of the entire Sinai by Israeli archaeologists, geologists, and others has turned up virtually no MB-LB presence in the central or S Sinai. Our current detailed knowledge of this remote and hostile area calls into question the biblical tradition of a million-and-a-half or more people migrating there (Num 11:21) for some forty years (Deut 2:7). The barren terrain and sparse oases might have supported a few straggling nomads, but no more than that.

The description of a thirty-eight-year encampment at Kadesh-barnea (Deut 1:19–2:15), which is prominent in the biblical tradition and gave rise to an important pilgrimage-festival in the time of the Monarchy, has long intrigued biblical scholars and archaeologists. Following the topographical indications in the Bible, Kadesh-barnea has been quite plausibly identified since the 19th century with the well-known oasis at Ain el-Qudeirat, near Quseima on the modern Israel–Egypt border. The small *tell* near the spring was sounded in 1956 by Moshe Dothan and was then extensively excavated to virgin soil in 1976–1982 by Rudolph Cohen. The latter has shown conclusively that these remains consist of three successive Israelite forts (Levels I–III) of the 10th–7th/6th centuries B.C., with nothing whatsoever of earlier occupation, not even scattered sherds (Cohen 1983). Thus the Kadesh-barnea episode, on present evidence, has little historical basis and appears to have become significant only in the united monarchy, when the Exodus tradition was crystallizing.

3. The Transjordanian Campaigns. The first phase of the conquest of Canaan, according to biblical accounts, focused on central and S Transjordan, which the tribes of Gad, Reuben, and half-Manasseh are said to have occupied (Numbers 21). The incoming Israelites are portrayed as encountering a settled population in Ammon, Moab, and Edom. Among specific cities mentioned as taken (and by implication destroyed) are Heshbon and Dibon; transparently identified with the large *tells* of *Hesbân* and *Dhibân*, respectively. Yet the extensive excavation of both has revealed that neither had any LB occupation. Hesbân had scant 12th–11th century material, with Iron Age occupation beginning principally in the 10th century B.C. (Geraty 1983). Dhibân may have had some Iron I material, but nothing earlier, and most of

the Iron Age remains were 8th–7th century B.C. (Dornemann 1983: 45, 63; Sauer 1986: 8–18). Thus neither site can have been destroyed by the Israelites under Joshua in the mid-13th century as required by Num 21:21–30. The same is probably true of Madeba (Num 21:30), which has produced thus far only a 12th century tomb for this horizon (Dornemann 1983: 34, 35).

Elsewhere in Transjordan, the general picture of LB and early Iron I occupation is complex, but it is clear that there is relatively little sedentary occupation of *southern* Transjordan in LB. N. Glueck’s surveys in the 1930s–1940s already suggested this (although he interpreted the evidence as *supporting* the biblical tradition of early Israelite settlement). Subsequent correction and expansion of Glueck’s site maps, including the discovery of a few more LB Age sites farther N in the Jordan valley and up on the plateau, has not substantially changed this view. Newer excavated evidence from Amman, the Beqa’ valley, Sahab, Irbid, Tell es-Sa’idiyeh, Deir ‘Alla, Kataret es-Samra, and a few other sites, as well as surveys from N Jordan, the Jordan valley, and Edom, all yield the same picture. Moab and Edom were not yet established, fortified kingdoms that would have posed any threat to Israelite tribes moving through the area, and even Ammon was rather sparsely occupied and defended. (For the most authoritative review, see Sauer 1986: 6–14; and cf. Dornemann 1983: 20–24). Thus throughout most of S Transjordan in LB-Iron I, outside the few settled towns, pastoralists and nomads must have dominated the countryside, like the “Shasu” tribes well known from Egyptian New Kingdom texts (Giveon 1971). In Moab, Heshbon and Dibon did not become significant urban centers until the 9th–8th centuries B.C. (Dornemann 1983: 63; Sauer 1986: 10, 15, 16). Ongoing excavations of the Iron Age sites in Edom indicates that the majority of these, including ‘Arô‘er, Buseirah (Bozra), Tawilan, and Umm el-Biyarah, were first settled only in the 8th or 7th centuries B.C. (Dornemann 1983: 47–63; Sauer 1986: 14–19). Thus the notion of large-scale 13th–12th century B.C. Israelite military campaigns in S Transjordan, or even of peaceful settlement there, is no longer tenable; the occupational history of the region simply does not fit (contra Boling and Wright *Joshua* AB). As for destructions, the only known LB II destructions are farther N—at Deir ‘Allā, Tell es-Sa’idiyeh, and Irbid—in Gilead; and in all cases, both the biblical identification and the agents of destruction remain unclear.

4. The Conquest of Canaan. The biblical tradition of the main phases of the occupation of the land of Canaan W of the Jordan is too well known to need summarizing here (cf. the principal accounts in Joshua, plus Num 21:1–3 and Judges 1). Since the infancy of modern topographical research and archaeology more than a century ago, biblical scholars and archaeologists have sought to locate the numerous cities said to have been taken and to identify 13th–12th century “destruction layers” that might be attributed to incoming Israelites. Indeed, confirming the Israelite conquest of Canaan archaeologically became one of the major priorities on the agenda of the “biblical archaeology” movement led by Albright and his followers from ca. 1925–1970, which adopted almost exclusively the “conquest” model presented in the book of Joshua (see also Lapp 1967). This approach was also taken up by several prominent members of the “Israeli school,” notably Yadin (1979; but cf. Aharoni *WHJP* 3: 94–128). And the effort still continues among a few conservative biblical scholars, some of whom, however, opt for the now totally discredited “high date of the Exodus” (thus Bimson 1981). Rather than reviewing the vast bibliography (see Miller *IJH*, 213–84), the latest and best archaeological data can be summarized in chart form (see table; and see further the latest syntheses in Callaway 1988, and especially Finkelstein *AIS*).

Canaanite Sites Claimed to Have Been Taken by the Israelites

SITE REFERENCES	BIBLICAL DESCRIPTION; REMARKS	ARCHAEOLOGICAL EVIDENCE	
1. Zephath/Hormath	Num 21:1–3; Judg 1:17	“Destroyed.”	If Tel Masos, no LB occupation there.
2. Jericho	Judg 6:1–21	“Destroyed.”	No LB II

3. Ai	Josh 8:24	“Destroyed.”	occupation. No LB II occupation.
4. Bethel	Josh 8:17; Judg 1:22–28	“Destroyed.”	Destruction at end of LB II.
5. Jerusalem	Josh 10:1–27; Judg 1:8, 21	Texts contradictory	LB II occupation, but no evidence of destruction.
6. Libnah	Josh 10:29, 31	“Destroyed.”	Tell es-Sâfi? occupation?
7. Lachish	Josh 10:31, 32	“Destroyed.”	Level VI destroyed ca. 1150 B.C.
8. Hebron	Josh 14:13–15; 15:13, 14; Judg 1:10	Texts imply capture but no destruction described.	No Evidence.
9. Debir	Josh 10:38, 39; 15:15–17; Judg 1:11–13	“Destroyed.”	If Tell Beit Mirsim C, yes; if Tell Rabûd, no.
10. Makkedah	Josh 10:28	“Destroyed.”	If Kh. el-Qôm, no LB II.
11. Eglon	Josh 10:34, 35	“Destroyed.”	Tell el-Hesi IV?; no destruction.
12. Hazor	Josh 11:10–13	“Destroyed,” but described as still existing later.	Lower City, Gnl Str. XIII violently destroyed ca. 1200
13. Dan	Judg 18:11–28	“Destroyed.”	LB II occupation, whether destruction at end unclear.
14. Gaza	Judg 1:18	“Taken.”	No evidence.
15. Ashkelon	Judg 1:18	“Taken.”	No evidence.
16. Ekron	Judg 1:18	“Taken.”	No evidence.
17. Heshbon	Num 21:25–30	“Destroyed.”	No LB II occupation.
18. Dibon	Num 21:30	Destruction implied.	No LB II occupation.
19. Medeba	Num 21:30	Destruction implied.	No evidence.

It is obvious that of the nearly 20 identifiable LB/Iron I sites that the biblical writers claim were forcibly taken by the Israelites under Joshua or his immediate successors, only Bethel and Hazor have any *archaeological* claims to destructions, i.e., historical claims supported by extrabiblical evidence. And even here, there is no conclusive data to support the notion that Israelites were the agents of destruction. (The new evidence dating the destruction of Lachish VI to Rameses III or later, ca. 1150 B.C., is much too late; cf. Ussishkin 1985). Thus the “conquest” model derived principally from the book of Joshua, so promising in the beginning, is now seen to have fared rather badly in more recent research. We must

conclude that as an overall model for understanding the origins of Israel, the whole notion of a literal “Exodus-wilderness wanderings-Conquest” episode is now unproductive and indeed detrimental, since it is challenged by current archaeological and historical research. The possible experience of some tribal elements in Egypt and Transjordan, or the scattered violence accompanying early phases of the settlement in Canaan, were undoubtedly minor factors. The emergence of Israel must be seen rather as part of a larger, enormously complex, long drawn-out process of socio-economic change on the LB/Iron I horizon in Palestine with many regional variations. Newer and more sophisticated models, as well as a “secular history” of Palestine (particularly settlement history) are required if we are to understand Israel’s origins adequately. Furthermore, it may be the continuity with Canaanite culture, *not* the changes, that in the long run turns out to be the most significant factor.

C. New Data, New Models

Today there are considerable data to support “non-invasion” models of the Israelite settlement in Canaan. Although these data are recent and scarcely published, and thus remain largely unknown to most biblical scholars, archaeological excavations and surveys in the past fifteen years have brought to light hundreds of small, Iron I sites of the late 13th–11th centuries B.C. These are located primarily in the central hills, but are also found as far N as Galilee and southward into the N Negeb. Nearly all are very small, unwalled sites, many founded *de novo* in the late 13th or 12th century, and most abandoned by the 11th century. If we could identify these Iron I sites as “early Israelite” villages, they would yield the *first* such *external* evidence we have found of this phase of the Israelite settlement of Canaan. Yet we must defer that question for the moment.

1. Survey and Excavation. The evidence can only be summarized very briefly here. Most of the new Iron Age sites noted above have been discovered through Israeli surface surveys, still largely unpublished: those of I. Finkelstein in Ephraim; of A. Zertal in N Ephraim and Manasseh; of several Tel Aviv University archaeologists in the Shephelah and along the Sharon Plain; and of Zvi Gal in Lower Galilee (see references in Finkelstein 1986; *AIS*; Gal 1989; Stager 1985a; Dever 1987; Dever *fc.b.*; Zertal 1987). Finkelstein, for instance, has discovered no fewer than 409 Iron Age I sites E of Aphek up into the Jerusalem hills, of which more than 75 were first founded in Iron I. In the central hills area, L. Stager (1985a) has shown that the number and density of settlements increased dramatically just after 1200 B.C.—from 23 LB sites, to 114 Iron I sites, 97 of them first founded in Iron I. Although the individual LB sites were larger urban Canaanite sites (median size = 12–13 acres) and the individual Iron I sites were smaller villages (median size = 2–3 acres), the total occupied area in the central hills region surveyed by Stager rose dramatically from ca. 175 acres in LB to ca. 475 acres in Iron I. Demographers would hardly attribute this sharp increase to natural growth alone. Clearly there has been an influx of new settlers; but who *were* they, and where did they *come* from?

Actual excavations have been undertaken on relatively few of these Iron Age I villages, notably by J. A. Callaway and R. Cooley at ʿAi and Radannah (the latter possibly biblical Beeroth), N of Jerusalem (Callaway 1985; 1988); by A. Mazar (1981) at Giloh, on the S outskirts of Jerusalem; by M. Kochavi, I. Finkelstein, and others at ʿIzbet Šarṭeh, near Aphek (probably biblical Ebenezer; Finkelstein 1986); and by A. Kempinski and V. Fritz (1983) at Tel Masos, in the N Negeb (possibly biblical Hormah; on all the above, see further, with bibliography, Dever 1987; Stager 1985a).

ʿIzbet Šarṭeh is particularly significant, since it can probably be identified with a known Israelite site; it has only three levels, all belonging to the 12th–10th centuries B.C., all relatively well exposed in excavation (up to 35 percent); and the material has now appeared in a definitive final publication (Finkelstein 1986). Stratum III, of the late 13th–early 11th centuries, consists of a simple “oval courtyard settlement” that may reflect a herdsmen’s encampment (contra Finkelstein’s settlement of “recently sedentarized desert nomads”; 1986: 108). After abandonment and a gap in occupation, it was succeeded by stratum II, a substantial village of perhaps 100 or so, with several four-room courtyard houses and many stone-lined silos, dating to the late 11th century B.C. Stratum I represents a decline, but continues into the early 10th century, after which the site was permanently abandoned. It is noteworthy that ʿIzbet Šarṭeh was peacefully established, and although it was abandoned at the end of each phase, there were no

restrictions. The economy, especially that of strata III–II, was based on agriculture and animal husbandry. The pottery is in *strong* continuity with the LB Age Canaanite repertoire, most closely paralleled by the Iron I N Shephelah and hill country sites (Gezer, Beth-shemesh, Giloh, Shiloh, etc.), but it also has lesser affinities with coastal Canaanite or Philistine sites (Aphek, Tell Qasile; on the above, see especially Finkelstein 1986; and cf. the greater stress on ceramic continuity in *AIS*, confirmed by Dever *fc.a.*).

Only preliminary reports are available for some of these sites, but already we may draw at least a *provisional* picture of the material culture—and possibly of the social structure and even of the ethnic identity—of these villagers.

(1) We can see a significant shift in *settlement patterns* from the LB to Iron Age I. The typical Iron I sites known thus far are located mostly in the central regions of Canaan, especially the hill country, not on destroyed LB *tells* (where they had been sought). They are founded *de novo* and peacefully, in the late 13th–early 12th century B.C., in a decentralized or nonnucleated pattern of settlement. This growth of new settlements resulted in a sharp rise in population in the central hills in early Iron I.

(2) We have a shift in *settlement type* as well, from large, walled urban to nonurban sites. Most of the Iron I sites are small, unwallled hilltop villages, with a population of from several dozen to as many as 300 or so. All these villages are characterized by a distinctive and homogeneous style of “four-room” courtyard house (often misleadingly called the “Israelite” type-house), which usually features rock-hewn cisterns and subterranean silos. Such self-contained courtyard houses—really “peasant farmhouses”—are ideally suited to an agrarian economy; and, indeed, similar houses are found widely throughout rural areas of the E Mediterranean, from ancient to modern times. See also HOUSE, ISRAELITE.

(3) The *economy* of these Iron I villages was largely self-sufficient, based mainly on small-scale but intensive terrace farming, with some admixture of livestock herding and primitive “cottage industry.” A few trade items, however, principally ceramics, indicate that these villages were not totally isolated, but had limited contact with the Canaanite urban centers some distance away.

(4) A changed *technology* is now in evidence, marked particularly by the mastery and extension of terrace agriculture to exploit the cleared hillsides, aided perhaps by the utilization of iron implements, now gradually coming into use in Palestine. Lime-plastered cisterns, while known long before, were now more widely adopted to solve the perennial problem of summer water shortage in the hill country. Ceramic production generally followed that of the LB Canaanite culture, with the introduction of a few new forms (including the popularization of the “collar-rim” storejar), generally inferior in manufacture.

(5) The *social structure* of these small Iron I villages appears to be much less “stratified” than that of the urban LB Age, with no indications of a hierarchically-ranked social order, no “elite” residences or palaces, and no public or administrative structures, not even sanctuaries or temples. The rather stereotypical house-plans show little variation and are clustered closely together; their type and arrangement do not even differ significantly from village to village. The general picture to be derived from the new archaeological data for Iron I is that of a simple, agrarian, cohesive society, probably kin-based. The villages are in relatively close proximity; they are apparently organized for internal occupation, but have little need for defense against external pressures. Although the simpler Iron I social and political structures mark a retrogression from the “state-level” organization of the LB to a “tribal level,” some sophistication is nevertheless still seen in a few epigraphic remains, including an abecedarium from ʿIzbet Šarṭeh that may suggest fairly widespread literacy.

(6) In terms of *provenance*, it must be stressed that there is no evidence whatsoever in the material culture that would indicate that these Iron I villagers originated outside Palestine, not even in Transjordan, much less in Egypt or the Sinai. There is nothing in the material remains to suggest that these are “pastoral nomads settling down”—on the contrary, they appear to be skilled and well-adapted peasant farmers, long familiar with local conditions in Canaan. What is “new” is simply the *combination* and adaptation of existing cultural elements—such as the courtyard houses, silos, and terrace agriculture—with a few novel elements. This distinctive “hybrid” material culture served as the basis for the agricultural settlement of the hill country and the emergence of a distinctive new social order, as well as,

in all probability, a new ethnic identity and solidarity. Nevertheless, the overall cultural traditions of these Iron I villages show rather strong continuity with LB Age Canaan, especially in the pottery.

(7) Finally, in terms of *duration*, nearly all of these Iron I villages were abandoned by the late 11th or early 10th century B.C., with the growth of a more concentrated urban culture at the beginning of the united monarchy and the emergence and full development of the true “Iron Age” or “Israelite” culture.

2. Early “Israelite” Villages? On the basis of the foregoing cultural traits, it would be tempting to conclude that these new Iron I sites represent the first definitive *archaeological* evidence we have had of the formative phase of the Israelite settlement in Palestine. These would then be the very early Israelite villages described typically in the book of Judges (thus A. Mazar 1981; Stager 1985a; Callaway 1985; 1988; Fritz 1987; Finkelstein 1986; *AI*S). If that proposition should be sustained by further data, these discoveries would constitute the most significant correlation yet between archaeology and biblical history.

Before we can be quite so sanguine, however, we must address several neglected, yet crucial, questions in the interpretation of the archaeological data—particularly as these data relate to textual analysis and historical reconstruction (see further Dever *fc.a.*).

a. Social and Economic Structure. Skeptics have often observed that “archaeologists do not dig up social systems.” Perhaps not; but they do uncover traces of social systems, since modern archaeology concentrates on recovering the “material correlates” of both individual and collective human behavior. See *ARCHAEOLOGY*. What do recent data reveal about the social and economic structure of the Iron I villages?

(1) Social Structure. As Stager has shown (1985a), the typical four-room courtyard houses, their clustering into larger units, and the overall village plan, all appear to be a direct reflection of the social structure embodied in the terminology of the Hebrew Bible, especially in Judges. Thus, in ascending levels of complexity, we can recognize: (1) in the individual house, Heb *geber*, which really designates the conjugal or “*nuclear family*” of 4–5 people; (2) in the compound, or cluster of 2–3 such houses, Heb *bēt āb*, “house of the father,” meaning “lineage,” or in sociological terms an “extended or multi-generational” family, of up to 20 persons; (3) in the whole village plan, with several family compounds, Heb *mišpāḥâ*, “family” in the larger sense of “clan,” with anywhere from several dozen to several hundred related persons; (4) in the grouping of many such villages, Heb *šēbet* or *maṭṭeh*, “tribe”; and (5) in the overall distribution of settlements, Heb *am yiśrā.el* or *bēnē yiśrā.el*, or “tribal confederation,” “nation” (the latter two *not* separated by Stager). If this analysis, probably the best example yet of the newer style of “biblical archaeology,” is correct, then the archaeological remains corroborate the textual evidence decisively. Early Israel was a kin-based (or “segmentary”) society, strongly egalitarian. Archaeology shows that the characteristic settlement type and distribution of the Iron I highland villages reflect the essential social structure of early Israel—almost precisely as the book of Judges (Joshua much less so) has faithfully preserved it in the written record.

(2) Economy. The subsistence system of the Iron I villages is equally clear. The economy is based primarily on small-scale but intensive agriculture, with some admixture of specialized stockbreeding. This is indicated by the relatively isolated location of the villages away from river valleys and major trade routes, but in areas still suitable for hillside farming and herding. Furthermore, the efficient size and compact layout of the villages, as well as the family-based social structure, are well suited to such an agrarian economy. The typical four-room courtyard house is an ideal “peasant farmhouse,” with provisions for the number of people, animals, and installations typically needed for an individual household production unit (Stager 1985a: 11–17). Finally, the new technology reflects a successful adaptation to hill country agriculture, particularly the near perfection of the art of terracing hillsides, excavating waterproof cisterns in the bedrock, and constructing stone-lined storage silos.

(3) The “Domestic Mode of Production.” The socio-economic structure that we confront in these Iron I villages is thus obviously simple and agrarian. On a cultural-evolutionary scale of development we could regard it as being at a “pre-State” level, either “tribal” or “chiefdom” (Service 1962). This is also suggested by the biblical sources, especially Judges-Samuel, in their vivid and often detailed description of conditions in the premonarchical periods (Frick 1985). Unfortunately, because of the “idealist” bias of

most biblical historians (and even the biblical writers themselves) little research has been done either on the material and technological basis of early Israelite culture, or on its social consequences.

Recently, however, there has been growing interest in early Iron Age agriculture (cf. de Geus 1976; Stager 1985a and references there; and especially Hopkins 1985; Borowski 1987). Certainly Gottwald (1979) has gone furthest in his determined “program of historical cultural-material research into early Israel” (1979: 650–63). Many regard this, of course, as economic determinism, others as simply 20th century Marxism projected back upon early Israel. But the archaeologist, who specializes in material culture, can only applaud Gottwald when he declares: “Only as the full *materiality* of ancient Israel is more securely grasped will we be able to make proper sense of its *spirituality*” (1979: xxv). Thus Gottwald’s *Tribes of Yahweh* expands upon the sociological and anthropological approach of Mendenhall’s “peasants’ revolt” model by looking not only at ideological factors like “Yahwism” as the driving force behind Israelite social structure and solidarity, but also at the agrarian economy and technology of the supposed peasants. Gottwald makes admirable use of what little the “new archaeology” could offer in the mid-1970s, sensing correctly that its research objectives are complementary to his own, but there were few data available then (see also Chaney 1983). In our view, Gottwald’s materialist perspective on early Israel seems more promising than most later treatments of a more conventional sort, based as they are almost solely on the biblical texts (thus Halpern 1983) which are clearly limited in their usefulness.

It is not merely early Israelite agriculture and technology at which we must look, however, but rather at the *total* subsistence system and its related social system. This is what Marx and Engels (unlike some later Marxist theorists) meant by “mode of production,” which delineated not simply an economic system, but a *social-evolutionary stage*. The “mode of production” included a society’s adaptation to its environment, technology, class structure, political organization, conceptual systems, and even religion.

Marshall Sahlins’ *Stone Age Economics* (1972) elaborates further on the “Domestic Mode of Production” (DMP) that often characterizes peasant or pre-State societies. At this level of social evolution, the individual self-sufficient household is the basic unit of production, and production is for consumption rather than for exchange, hence family labor and cumulative skills are more significant than technology. The DMP, according to Sahlins, tends to be “anti-autocratic” by nature, but it nevertheless obliges household groups to form voluntary social compacts, i.e., to pool labor and resources. Sahlins observes: “As the domestic economy is in effect the tribal economy in miniature, so politically it underwrites the condition of primitive society—society without a Sovereign” (1972: 95). This would be an astonishingly accurate portrait of early Israel, whose only sovereign was Yahweh. A unique “theology” was organically related in part to a distinctive technology, economy, and social order. While archaeology can get at the former only partially and with some difficulty (i.e., evidence of possible cult *practice*), it is superbly equipped for investigating the latter, even though this task has been neglected until recently. A beginning *has* been made, but much more must be done on peasant economy and society if we are to comprehend Israel’s origins in Canaan—especially if we are increasingly to employ the “peasants’ revolt” model.

b. Continuity and Coexistence. In asking what is new *archaeologically*, and thus culturally, in the Iron I villages, we must remember that Palestinian archaeology has long been dominated by a certain biblical notion of “political history” and has thus sought uncausal explanations of cultural change in general. Furthermore, in this case of the Israelite settlement, the almost-exclusive adoption of the “conquest model” by those of the Albright school has meant that stress was placed upon the *discontinuity* between the LB/Canaanite and Iron I/Israelite cultures. The result was twofold: (1) a reductionist view of the emergence of Israel, as due to a relatively abrupt, violent, and complete triumph of newcomers who overwhelmed Canaan ca. 1250–1200 B.C.; and (2) an overemphasis on the supposed cultural discontinuities throughout the transitional LB-Iron I horizon in the 13th–11th centuries B.C.

Recently, however, newly accumulating archaeological evidence has shown that the abruptness of the break has been greatly exaggerated (cf. Kempinski 1985). We now know that many LB II sites were not destroyed at all, by either Israelites or Sea Peoples. Also, Egyptian New Kingdom influence did not cease

with the 19th (First Ramesside) Dynasty ca. 1200 B.C., but extended perhaps as late as the time of Rameses VI, ca. 1140 B.C. (especially at sites like Megiddo, Beth-shan, and Lachish; cf. Tadmor 1979; Weinstein 1981; Ussishkin 1985; A. Mazar 1985). And Canaanite material culture flourished well into the 12th and even 11th centuries B.C. in some areas, particularly in ceramics, where, apart from imports or Philistine Bichrome ware, it is often difficult to distinguish 13th from 12th century pottery (A. Mazar 1981; Wood 1985; Dever 1987). Finally, not even the appearance of iron provides a firm criterion for the beginning of the “Iron” Age, since iron begins as early as the 14th century B.C. but comes into common use only in the 11th–10th centuries B.C. Furthermore, its connection with the new technology and culture is more debated than ever in recent research (cf. Stager 1985a: 10–11).

The logical conclusion to be drawn is that the “invasion hypotheses” of which earlier archaeologists were inordinately fond are almost never useful models, certainly not for the LB/Iron I transition. We must look rather at the largely *indigenous* factors in socio-cultural change. For example, we need posit no hypothetical external forces whatsoever to account for the actual changes that we observe in the material culture of Palestine on the LB/Iron I horizon (except for the impact of the Sea Peoples, who were indeed newcomers). It cannot be stated too categorically: the emergence of Israel in Canaan was not an isolated, “unique” event, but rather an integral (albeit small) part of a gradual, exceedingly complex set of socio-economic, cultural, and political changes on the LB–Iron I horizon in the Levant, with many regional variations. It was but one episode in the long settlement history of Palestine and cannot be understood apart from the larger context of that history (see especially Coote and Whitelam 1987). The early Israelites, who first appear in our textual sources at this time, may have come to constitute a distinct *ethnic group* by the late 13th–early 12th centuries B.C., but there is no archaeological evidence whatsoever that they were recent arrivals in Canaan, much less an invading military horde. And the ensuing struggle between Israelite and Canaanite culture continued for centuries, even to the end of the Monarchy (Dever 1984; see also Fritz 1987 and his “symbiosis hypothesis”).

Having enunciated a general interpretative principle, however, we need to be as specific as possible on the elements of continuity/discontinuity, since so much hangs on this question, and yet previous discussions seem inconclusive.

(1) Continuity. Of the diagnostic feature enumerated above for the Iron I villages, the pottery, in particular, shows strong continuity with the 13th century LB Age repertoire. For instance, the pottery of ʿIzbet Šarṭeh, which is no doubt Israelite, is virtually *identical* to that of the 13th–12th centuries B.C. at nearby Gezer, which both archaeology and the biblical tradition agree is non-Israelite (i.e., LB Canaanite, with some new Iron I Philistine elements). The *only* significant difference is that ʿIzbet Šarṭeh has “collar rim” storejars, while Gezer does not; and Gezer has much more Philistine pottery. This is precisely what we should expect. It is interesting that Finkelstein had argued in his Hebrew dissertation (1983) that the ʿIzbet Šarṭeh pottery reflected a “Transjordanian pastoral-nomadic” origin, but in the full publication of the site (1986) he quite correctly makes no mention of such a possibility. (See SETTLEMENT OF CANAAN, which also posits a Transjordanian background for early Israelite culture, but cites no *archaeological* evidence; Boling 1988 adduces some data but none that is decisive). It needs to be emphasized that not only is the Iron I village pottery in direct continuity with the typical local LB ceramic repertoire, but its further development in the 12th–early 11th centuries B.C. cannot be explained *otherwise*. This is seen in all the principal forms: storejars, kraters, bowls, cooking pots, even juglets, chalices, and lamps (cf. A. Mazar 1981; Finkelstein 1986: 38–92; Dever *fc.a*). The principal continuity between the LB Canaanite material culture and the early Iron I “Israelite” material culture is seen in the pottery. Yet it must always be remembered that among archaeologists and anthropologists pottery is regarded as our most sensitive medium for perceiving cultural contact and cultural change.

(2) Discontinuity. On the other hand, several diagnostic features of the Iron I villages are clearly innovative, specifically settlement type and distribution; and an almost total shift to a nonurban, agrarian economy and social structure (see above).

(3) Continuity, Discontinuity, and Adaptation. Still other features show a mixture of continuity and discontinuity and must therefore be evaluated most judiciously. For instance, hillside terraces, rock-hewn

cisterns, and stone-lined silos now appear in relatively greater proportions, and they do indeed seem to characterize the technology of our early “Israelite” villages. But it is only the *combination* and *intensified* use that are new. All these elements have clear antecedents in the MB-LB Age, and even earlier (Stager 1985a: 5–10 and references there).

The case of the four-room courtyard house, or “Israelite type-house” (Shiloh 1970), is even more instructive. It is true that only in the Iron Age, and most often in the Iron I villages in question, does this distinctive house style become ubiquitous. But a few prototypes do appear in the LB; and a growing number of fully developed houses of this style are now known from obviously *non*-Israelite Iron I sites both in Palestine and in Transjordan (cf. A. Mazar 1981: 10, 11; Stager 1985a: 5–10; Finkelstein 1986: 121–24). Thus the four-room courtyard house was not so much an “Israelite invention” (and therefore a reliable diagnostic trait) as it was a successful adoption and modification of a common Iron I style of peasant farmhouse, one which was peculiarly suitable to early Israel’s agrarian economy and social order. See also HOUSE, ISRAELITE.

Finally, we note the problem of the “collar rim” storejar, which Albright, Aharoni, and others had thought another “Israelite type-fossil.” These pithoi are simply large variants of the LB-Iron I storejar, with a reinforcing band around the neck. They are particularly suitable for storage of liquids and foodstuffs, and it is probably for that reason that they are especially common in the Iron I villages we have discussed. But they are now known from LB contexts, as well as from non-Israelite sites in both Palestine and Transjordan (Ibrahim 1978; A. Mazar 1981: 27–31; Finkelstein 1986: 76–84).

All the above elements do indeed *become* “Israelite”; but they are not exclusively so, they are not necessarily innovations, and the individual elements in themselves cannot define “Israelite ethnicity.”

c. Ultimate Origins. Another, larger aspect of LB-Iron I continuity must now be examined, namely the question of early Israel’s *ultimate* origins. Is the demonstrable continuity with local LB Canaanite culture compatible with the customary models for the Israelite settlement; and, if so, is the archaeological evidence decisive for any *one* of them?

(1) “**Conquest.**” Clearly, from our discussion the conquest model is ruled out. The founders of the Iron I villagers do not appear to have been newcomers to Palestine, much less settlers displacing Canaanites in the urban centers by military force. The few sites actually destroyed ca. 1200 B.C. were destroyed either by the Philistines, or by unknown agents; and none is resettled within a reasonable time by people who could be implicated in the destruction, or could otherwise be identified as “Israelites.”

(2) “**Peaceful Infiltration.**” The “peaceful infiltration” model has fared somewhat better, in that it always eschewed sudden conquest in favor of a process that envisions the Israelites emerging in Canaan gradually and largely without armed conflict. That may not have squared very well with the archaeological picture as viewed a generation ago when Alt and others advanced the “peaceful infiltration” model in the face of the dominant Albrightian interpretation, but the newer data surveyed above tend to confirm it in general. In certain *specifics*, however, this model, although relatively sophisticated, is not broad enough to accommodate some of the newer data (contra SETTLEMENT OF CANAAN). In its classic form, it assumed that the Iron I hill country settlers were pastoral nomads immigrating from Transjordan at first seasonally and then gradually becoming fully sedentarized (see Weippert 1971; 1979). More recently, however, this notion of *Siedlungsgeschichte*, or the sedentarization of nomads, has come under criticism from better informed ethnographic studies of pastoralists. (In addition, the “nomadic ideal” posited by some scholars as basic to the biblical writers has been shown to be largely a modern fiction). Furthermore, nearly all the archaeological data we have seen in the material culture of the Iron I sites, now that they are finally being investigated, contradicts *both* these notions of Transjordanian and pastoralist backgrounds. The Iron I pottery derives directly from LB traditions, which *must* be local to W Palestine, since there is no appreciable LB occupation in S Transjordan (contra both this model and the biblical tradition).

As for “pastoral origins,” relatively few of the new Iron I villages suggest nomads gradually becoming farmers. Only Giloh and ʿIzbet Šarṭeh III appear to be “fortified herdsman’s encampments,” and even so there is no evidence that the occupants are either newcomers or former nomads. The houses at Tel Masos

have been interpreted as modeled upon bedouin-like tents (Fritz 1981), but this is generally disputed (Stager 1985a: 17). Furthermore, the evidence of cattle breeding and of sophisticated ceramics at Tel Masos suggests anything but pastoralists settling down. Elsewhere, the Iron I hill-country sites exhibit a very advanced, multifaceted agricultural technology, one that was labor-intensive but nevertheless almost ideally adapted to high-risk agriculture under difficult conditions in this former marginal zone (see Hopkins 1985; Stager 1985a: 5–9; Borowski 1987). These are hardy first-generation farmers, i.e., refugees from the cities, much less recently sedentarized pastoralists (or “urban peasants”). They appear rather to be farmers who already had a thorough knowledge of local agricultural conditions in Canaan and needed only to adapt their experience to the hill country. The fact that the new technology is really a *combination* of strategies already utilized in the MB-LB Age, and thus well-proven, is further evidence of the Iron I villagers’ local Canaanite derivation. It could be argued on the other hand that they merely borrowed this technology. But if they were really recently sedentarized Transjordanian pastoralists, they would have had little access to the source, isolated as they were in the hill country far from the centers of Canaanite culture.

All things considered, both the ethnographic and archaeological evidence militate against the “peaceful infiltration” model for the emergence of Israel, despite the fact that its notion of Transjordanian origins is consonant with some strands of the biblical tradition.

3. “Peasants’ Revolt.” The “peasants’ revolt” (or “internal conquest”) model seems more compatible with current archaeological data and theory than any other—especially in the modified form advanced by Gottwald (1979), with its emphasis on the role of technology and economy in social change. This model presumes that the early Israelite movement was made up of various dissident elements of LB Age Canaanite society, mostly dispossessed peasant farmers, who colonized new areas in the hinterland and there adopted a less stratified social order better suited to an agrarian economy. That appears very similar to the picture derived from the newer archaeology, except that Gottwald’s “revolutionary, egalitarian” social (and religious) force presumed to be behind this movement is not susceptible to direct archaeological illumination. Of course, these former Canaanite “peasants” were already “liberated” (to use Gottwald’s phrase) by the time we encounter them in the Iron I hill-country villages, so that they are now freeholders and self-sufficient homesteaders. But their *background* as peasant farmers is still clear in the archaeological record, as is the distinctiveness of their emerging social structure *vis à vis* old Canaan. Thus at least some aspects of the “peasants’ revolt” formulation are now well attested archaeologically—a measure of support (if not confirmation) that no other model can boast. The nucleus of later Israel appears to have derived from the local LB culture of Canaan through relatively normal social processes of peasant withdrawal and what has been termed “retribalization,” rather than originating outside Canaan and then either being superimposed on the local population or displacing them entirely in the early Iron I period. (For the possibility of *some* extra-Palestinian elements, however, see below.)

D. Archaeological Identification of “Israelite” Ethnicity

If the point has been made that the early Israelites in Canaan were largely of local derivation, we still have not answered the question of who they *were*. That is, how did they differ from Canaanites, how and when did they come to identify themselves as “Israelites,” and what did that self-consciousness mean culturally? These, of course, have always been recognized as the fundamental questions, all attempting to ascertain: What was “*unique*” about ancient Israel, and *when* did it emerge as such? (cf. Lemche 1985; Thompson 1987). But until recently both biblical theologians and historians have sought the answers almost exclusively through the analysis of *texts*. That may be methodologically sound, as far as it goes, for “ethnicity” is usually defined mainly in terms of self-image; a social group may constitute a separate ethnic group if the majority of its members feel themselves to be a distinct “people.” And certainly some biblical texts do posit a strong sense of “Israelite” ethnicity as though it characterized the tradition from beginning to end. Yet it is increasingly recognized that the biblical texts are often late, elitist, and propagandistic. And because the Bible is theocratic history, Israel is often portrayed in such radically disjunctive terms with respect to neighboring peoples that the result is more caricature than

characterization. (To be fair, many other biblical texts do portray Israel in a less flattering—and thus more realistic—light.)

What was it that really constituted the distinctiveness of Israelite culture, spiritually *and* materially? It is suggested here that the biblical texts alone cannot and should not be expected adequately to illuminate “ethnicity,” and, moreover, that it is only through the contribution of archaeology that we can achieve a more balanced picture, both by putting the biblical texts into larger context, as well as by supplying some of the missing information. It may be objected, of course, that even the “new archaeology,” with its incomparably more sophisticated techniques, is ultimately limited, too, in its ability to discern thought processes *behind* material culture remains, and thus is powerless to illuminate such matters as self-consciousness or “ethnicity.” It must be admitted that without the Merneptah reference to “Israel” ca. 1210 B.C. and the later biblical texts, we would not be sure that our Iron I villages are indeed “Israelite.” That is, we could recognize the emergence of a distinctive new culture ca. 1200 B.C. in Canaan, but it would remain anonymous, we would not be able to affix a specific *ethnic label* to it. But even so, the mere name, however valuable, does not define, much less “explain,” the nature and origin of this new culture. Only the *combination* of the textual and artifactual data—of history and archaeology—can aid in this inquiry.

Archeology can certainly contribute more than it has thus far to the identification of “Israelite ethnicity.” It can do so first because the “new archaeology” is multidisciplinary in nature and therefore attempts to elucidate culture in *all* its aspects, not merely to describe “ceramic culture” and then proceed immediately to the writing of “political history” (as the older-style “biblical archaeology” really did). Surely ancient Israelite culture had its secular components, no less formative than its religious components, and it is these that archaeology can often illuminate uniquely and brilliantly. And *both* aspects of a culture must be so illuminated if it is to be comprehended in its own terms, rather than in some “idealistic” scheme that robs it of its variety and vitality.

Second, archaeology today is strongly behavioral. It focuses not merely on artifacts in themselves, but on the archaeological record as a whole, which is seen to reflect the “material correlates of human behavior.” Archaeologists may not be very well equipped to be “paleo-psychologists” (as Binford reminds us); but if they cannot get at ideology, they nevertheless have an unparalleled opportunity to analyze the material *consequences* of human behavior, insofar as they reflect upon the thoughts and actions that produced the artifacts they study. Surely these “material correlates” of behavior, if anything, are clues to ethnicity.

Here we are clearly advocating a structural-functional model, at least at the fundamental level of analysis—without, however, denying the validity, and indeed the necessity, of a historical-cultural model at the higher level of synthesis (much as Gottwald 1979: 622–49; also 1985: 230–38). Thus the attempt at an archaeological identification of “ethnicity” need not be susceptible to the charge of reductionism, or material determinism. Nor is such an attempt necessarily confined to the old-fashioned “trait-list” approach that most archaeologists today would find unproductive.

Whatever model we may adopt in assessing the archaeological evidence for “Israelite ethnicity,” we must begin by assuming that no matter what *else* early Israel was (or later thought itself to be), it was *also* a minority ethnic group in a multi-ethnic society in Iron I Canaan. By “ethnic group” we mean, at minimum, a social group that: (1) is biologically self-perpetuating; (2) shares a fundamental, uniform set of cultural values, including language; (3) constitutes a partly independent “interaction sphere”; (4) has a membership that defines itself, as well as being defined by others, as a category distinct from other categories of the same order; and (5) perpetuates its sense of separate identity both by developing rules for maintaining “ethnic boundaries,” as well as for participating in inter-ethnic social encounters. (See further Barth 1969: 9–38.) It is especially important to note certain ways in which ethnic groups typically originate, maintain themselves, and assimilate or otherwise change. The *origins* of ethnic groups, in particular, are difficult, often impossible to ascertain (cf. Barth 1969: 17, 18), even where we have historical documentation; but we can point to some *reasons* for both the existence and the persistence of such groups.

It would seem that early Israel clearly qualifies as an ethnic group in the above definition, although that does not imply the unity that later biblical writers presupposed. The question here is simply to what degree *any* of this ethnic identity may be reflected in the archaeological remains, particularly of the early Iron Age village culture surveyed above. And would a positive identification of these villages as “Israelite” help one to choose between the various models proposed for understanding Israelite origins (whether by confirming, contradicting, or modifying biblical tradition)?

The results of our inquiry, even at best, may seem meager, for the question of archaeological identification of ethnicity is one of the most vexed interpretive issues in current archaeology. (For orientation, see Kamp and Yoffee 1980, with programmatic suggestions that the authors evidently regard positively but that are actually unachievable.) The usual “ethnic markers” would consist of such features as language (including “body language”), physical type, dress, food preferences, kinship patterns, general cultural and social values, religion, and the always-nebulous “self-identity.” It is obvious that *none* of these traits will be very well represented, if at all, in the archaeological record—even if we regarded a “trait-list” approach as adequate. And their origins will remain even more obscure. But *collective behavior* will often be reflected archaeologically, i.e., the economy in settlement types and distribution; technology and subsistence practices in both artifacts and “ecofacts”; social structure in house form and function; social stratification in elite goods in tombs and elsewhere; ideology in expressions in art; and even religious practices in cultic remains.

We have already treated the archaeological data on settlement patterns, technology, subsistence, and social structure. We concluded that *all* the newer data are consonant with some strands of the biblical tradition, especially in Judges-Samuel. Thus we regard Stager’s seminal work on the “archaeology of the family” in early Israel (1985a) as an almost ideal model of the proper dialogue between the “new archaeology” and biblical history, indeed a point of departure for all future studies. Yet even Stager begs the question. He *assumes* that the Iron I hill-country villages are “Israelite,” and his own research goes further than anyone else to date in demonstrating that, but nowhere does he explicitly state his conclusion (or, for that matter address the radical implication for biblical history and scholarship).

Can we do better? Perhaps; but not without vastly improved research designs and much more survey and excavation focused *specifically* on this problem. For instance, we have very few Iron I cemeteries, which potentially would be most revealing; and even fewer cultic installations (on the latter, see Dever 1987). We may even hope, in time, for definitive epigraphic discoveries. Meanwhile, we need many more excavation and research projects that are conducive to *cross-cultural* comparisons. In practice, this would entail excavating, with identical research designs and preferably simultaneously, several small one-period Iron I sites in various areas of Israel and Transjordan that could be presumed on independent (textual) witness to be: (1) Canaanite; (2) Philistine; (3) Israelite; and (4) Ammonite-Moabite-Edomite. The excavations would concentrate on total, systemic retrieval of all cultural deposits, which methods already introduced by the “newer archaeology”; then on exhaustive intersite comparisons. The results of such a deliberate archaeological research program—carried out over a ten-year period or so, and properly integrated with ethnographic, textual, and other studies—might well prove decisive. The question “Who were the early Israelites *archaeologically*?” is now theoretically answerable. And when we do answer it, one suspects that the “peasants’ revolt” or “indigenous peasant” model—already the most fruitful for research—will be further enhanced. (The most explicit attempt thus far at resolving the problem *archaeologically*, based on new data, is Finkelstein, although he adopts a modified “peaceful infiltration” model and at first scarcely refers to Gottwald’s work; see Finkelstein 1986: 201–13; and further *AIS*; for a critique of Finkelstein, see Dever 1989).

E. Toward a New Synthesis of Archaeology and Biblical History

The problem with which we began this survey has not yet been resolved, either on the basis of textual or archaeological evidence.

1. Facing the Dilemma. The dilemma is simply that in ancient Israel’s Credo and epic literature—indeed in her cult and tradition as a whole—the Exodus-Sinai-Conquest themes are absolutely fundamental. It is the “conquest of Canaan” that is the fulfillment of Yahweh’s promise, the constitutive

event that brings his Israelite people into existence. Yet if there is little *real* history in Israel's proclamation of her "salvation-history," is the tradition any more than a pious fraud? (For the latest, most radical view, see Lemche 1985.)

All critical scholars recognize that the biblical sources in the Pentateuch and Deuteronomistic History are relatively late, composite works that simply cannot be read at face value as history. Even the *earliest* written materials, the several archaic poems such as Exodus 15, are probably not eyewitness accounts. Thus concerning Exodus 15, dealing with the "crossing of the Red Sea"—which Cross dates to just after 1200 and regards as "a primary source for the central event in Israel's history" (*CMHE*, 123)—we must ask: "What event?" Are there any external data that would confirm that we are dealing here with history, and not myth?

Today, skeptical voices dominate the scene. As G. E. Wright himself acknowledged in his Introduction to the Anchor Bible *Joshua*: "In this book ancient Israel recorded her belief as to how the nation came to live in ancient Palestine. Yet during the last century a majority of those attempting to apply the methods of modern historiography to Hebrew tradition have said the book is wrong; it never happened that way at all" (page 4). And now, as we have seen, the cumulative results of a century of *archaeological* investigation powerfully buttress the negative view of the conquest, much to the consternation of those who expected the "archaeological revolution" to confirm the tradition. Where does that leave us?

2. Methodological Considerations. Assuming that we cannot simply dismiss either the textual or the artificial evidence, there are nevertheless some methodological approaches that may lead at least to partial solutions.

a. Two Traditions. First, we need to recall that there are *two* strands to the tradition as elaborated by the Deuteronomistic History: that preserved in Joshua, and that in Judges. While modern archaeology may call into question the historicity of Joshua, it provides rather dramatic corroboration of the account in Judges, even in obscure details. In the view advocated here, it is futile, indeed unnecessary, to attempt to reconcile these two conflicting versions (as Wright 1946). Instead of trying to "salvage" Joshua archaeologically, future research into Israelite origins should concentrate on Judges, as by far the most realistic and reliable source. There, the results will likely *not* be so negative.

b. Reconciling Joshua and Judges? If one asks, however, *why* the Israelite historiographers preserved two diametrically opposed versions of their own history, we can at least offer new critical approaches that may prove constructive. Recent structuralist analyses, such as those of Polzin (1980), Gottwald (1985), and others, have suggested that the redactors of the DH were fully aware of the radically divergent nature of the Joshua and the Judges materials they incorporated, but they left them in tension in a deliberately dialectic manner. As Gottwald (1985: 258) puts it: "By counterposing speech about how the Canaanites *must be* and *were* destroyed against speech about how Canaanites *remained* in the land and were even *accepted* into Israel, DH [the Deuteronomistic history] weaves an ironic exposition on the problematic of carrying out God's commands."

Another explanation for why the tradition has deliberately obscured much of its own origins is offered by Coote and Whitelam (1987). They suggest that the story of Israel's humble origins in social conflict did not suit the propagandistic purposes of the elite "House of David" in its rise to power and were thus downplayed, a point that Mendenhall had made earlier.

Finally, of the Joshua tradition, however folkloristic it may be, it must nevertheless be acknowledged that these materials may contain some raw source-data for the historian: (1) Archaeologically, we should observe that the evidence does not rule out the possibility that *some* constituents of the later Israelite tribal confederation may have derived from Egypt. In this connection, it is worth remembering that many biblical scholars have long held that only the "House of Joseph" (or elements of the later tribes of Benjamin and Judah—which have so obviously shaped the tradition) was ever in Egypt. The fact that most of the numerous Iron I villages we now have are precisely in the area of the Benjamin–Judah tribal territories may seem to lend credence to that suggestion. However, even if this area is the chief locus of early Israelite occupation, there is no direct *archaeological* evidence of Egyptian origins for the settlers, as we have seen, so the question must remain open.

(2) Even the minimalist “peasants’ revolt,” or “internal conquest,” model would allow for *some* of the military actions against the Canaanites described in the book of Joshua. Thus armed conflict may have been a contributing factor in the cultural struggle, even if not the principle cause.

3. Toward a Synthesis? Is it possible to move further, however, in reconciling what may seem to be conflicting approaches to archaeological and textual reconstructions of Israelite origins? There would seem to be two primary possibilities for a synthesis, at least of method, if not of results. (See further Dever *fc.b.*, for a symposium on this problem.)

a. Parallel Histories. The notion of producing two *alternate* versions of early Israelite history has recently become an option for the first time, due to the growing sophistication and maturity of the newer archaeology. At the “descriptive” level, the first approach would assay a history of ancient *Israel*, based on the biblical texts and focusing mainly on political and religious history. The second, or “secular,” approach would utilize archaeological remains and extrabiblical texts to outline a socio-economic history of *Palestine*, primarily in the Bronze–Iron Ages in this connection (but extending, of course, from earliest to relatively modern times). As the “normative” level, the first approach might result in a history of the religion of Israel (or at least a history of the literature *about* that religion), and possibly even in an OT theology. The “secular” approach, however, would necessarily be confined to the illumination of Israelite cultic practice in its larger ANE context, without reference to theology, i.e., its significance for the modern religious community, Jewish or Christian. (See the provisional efforts of Lemche 1985; Thompson 1987).

Each of these separate histories would concentrate on one class of data, and each would be pursued by competent specialists. Needless to say, these two disciplines would both be devoted to legitimate, truly historical tasks. Yet however ideal such a division of labor might seem in theory, in actual practice these two histories would tend to remain *parallel*. They might be complementary, but they would never converge; each would present but a partial view of the total reality we seek to comprehend, the phenomenon of ancient Israel in all its richness and diversity.

b. Converging Histories. Far more preferable, it would seem, would be a *combination* of the two approaches, “sacred” and “secular”—or at least a dialogue between the two, which would point toward a truly multidisciplinary synthesis. And today there are indeed signs of such a development, bringing together the insights of the discipline of Palestinian archaeology in its newer guise, coupled with the newer sociological and anthropological approaches to the early history of Israel.

We have already outlined the nature and contribution of archaeology in this cooperative task, both here and elsewhere. See ARCHAEOLOGY. In biblical studies, the most promising trends are seen in the early analysis of Gottwald in his *Tribes of Yahweh* (1979), together with de Geus’ *Tribes of Israel* (1976). More recently, the work of Marvin Chaney (1983) points in the same direction, as does Gottwald’s *The Hebrew Bible—A Socio-Literary Introduction* (1985) and N. P. Lemche’s *Early Israel* (1985). We have also called attention to two recent, specialized treatments of early Israelite agriculture, one by a biblical scholar (Hopkins 1985) and one by an archaeologist (Borowski 1987). Perhaps the most ambitious interdisciplinary synthesis yet may be the work of R. B. Coote and K. W. Whitelam, *The Emergence of Israel in Historical Perspective* (1987), based on a comprehensive settlement-history of Palestine from earliest to modern times. Finally, although it deals with a slightly later horizon, Frick’s *The Formation of the State in Ancient Israel: A Survey of Models and Theories* (1985; see also Frick 1979) also rests upon the newer methodology and approach. All these recent analyses embody socio-anthropological models, as well as the newer archaeology.

Despite the recent proliferation of works on early Israel, however, many remain more traditional, based almost exclusively on the biblical texts, such as Halpern’s *The Emergence of Israel in Canaan* (1983); Åhlström’s *Who Were the Israelites?* (1986); and Miller and Hayes’ *A History of Ancient Israel and Judah* (1986). These and other attempts at synthesis may be faulted for making little or inept use of the abundant archaeological data now available. Even B. Mazar’s *The Early Biblical Period: Historical Essays* (1986; see also Mazar 1981), while masterly, is almost totally out of touch with both the methods and results of modern archaeology. It is regrettable that most biblical scholars still cling to the notion that

artifacts without texts are “mute”; that archaeological evidence can only serve to “clarify matters of historical detail,” or is useful “only when correlated with specific items in biblical history” (thus *HAIJ* 102, 189; cf. also Miller 1976: 5, 40–48; 1982; *idem* in Knight and Tucker 1985: 1–30). On the contrary, given the skepticism of most biblical historians on the value of the biblical tradition for the premonarchic history of Israel, archaeology is rapidly becoming our *primary* datum (see also Callauay 1985; 1988; Lemche 1985: 385).

Clearly we are implying that a *multidisciplinary* approach to reconstructing early Israelite origins is preferable, indeed infinitely superior. Why do we assume that? In the first place, this approach helps to break the circular reasoning inevitably involved when the biblical texts alone are utilized to write the early history of Israel. One has only to browse through the various essays in the recent handbook *The Hebrew Bible and Its Modern Interpreters* (Knight and Tucker 1985) to see how inconclusive—indeed how devoid of much *real* history in the usual sense—most of current biblical criticism is when largely text-based and confined to the biblical tradition. This is true whether the methods are those of literary, form, traditional, canonical, or structuralist criticism (see especially the essays of Ackroyd, Knierim, and Knight). The result tends to be simply a history of the *literature* concerning the religion of Israel and her self-understanding of her experience in Canaan. Only new data from other sources, and perhaps new models from other disciplines, can break that impasse.

Albright’s original intuition—that nothing except the “external” evidence from archaeology could throw *new* light on the tradition as received—was sound; he erred only in assuming that archaeology would always confirm, never challenge, the “historical” reconstruction presented in the Hebrew Bible. Archaeology, in particular, allows us to get at the “history *behind* the history.” At the same time, it broadens the picture by supplementing political history with socio-economic and cultural history.

Second, the multidisciplinary approach, particularly in the new “secular” archaeology, allows us to focus on factors in cultural change in addition to primarily *ideological ones*, especially on the role of ecological adaptation, subsistence and economy, technology, and social structure. These *material* factors are obviously powerful agents in shaping history, yet concerning them the Bible is largely silent.

Finally, the multidisciplinary approach to history-writing—like archaeology today—is more systemic, more processual, and is thus more truly “explanatory” than the traditional, purely descriptive approach. We may be able to learn not only *what* happened in the past, but *why* (i.e., apart from “theological explanations”).

c. Faith and History. A final issue must be addressed, if only to acknowledge its fundamental importance to the current discussion of Israelite origins. The issue is simply this: if archaeological data are now as “primary” as those in the biblical texts, they may force a radical rewriting of the early history of Israel, one at variance with the tradition at crucial points. But does that not *undermine* the religious significance of the tradition? To put it another way, how shall those who espouse biblical faith remain *historians*, and not take flight into the realm of “supra-history”? (Cf. further Porteous 1970–71.) It may be a matter of balance. That is, while the fallacy of historicism has by now been well exposed, how shall we avoid the other extreme, that of existentialism? It was precisely this issue, never resolved, that vexed the earlier “biblical archaeology” movement, aligned as it was with Neo-orthodox-style “biblical theology” in the 1950s–1960s (cf. Dever 1980; 1985b). The new “secular” archaeology might simply declare this a “non-issue,” but in doing so it would forfeit any possibility for the dialogue that we regard as essential to *both* disciplines (specifically on the issue of the Israelite settlement, see Dever *fc.a.*, Dever *fc.b.*).

The “faith and history” issue has generated a literature much too vast to be surveyed here. What we *can* do is to point to two methodological principles that should govern the input of archaeology. First, insofar as we are historians, it really *is* important to ascertain “what happened in history,” as nearly as possible. Obviously, this historical inquiry must proceed independent of any theological presuppositions or biases, but the inquiry is not thereby *irrelevant* to questions of belief, as several current schools of biblical interpretation seem to imply. History cannot be allowed to become merely hermeneutics. (See further Knierim’s enlightening analysis of the current situation in biblical criticism in Knight and Tucker 1985: 123–65.) Second, whatever the results of our historical investigation, the outcome should not be

considered determinative in matters of religious belief, one way or another. Stendahl's prescient exposé of the weaknesses of the "biblical theology" movement criticized G. E. Wright's notion that religious *meaning* for us consists of our affirming the Bible's claims concerning "God's saving acts in history." Stendahl (*IDB* 1: 424) reminded us that "History does not answer such questions: it only poses them." And *archaeology* cannot answer these questions of faith, either. Archaeology can illumine historical events, but it cannot confirm the theological inferences drawn from those events, past or present.

F. Conclusion

Ancient Israel's problem in comprehending her own history was the same as ours: how to account for the unique *reality* of the people of Israel. The biblical writers fell back on the only analogy they had, historical experience, which for them was their own first-hand knowledge of the power of Yahweh over their pagan neighbors, and his ability to save and shape them as his people—despite their obscure origins, their lack of merit, and their disobedience. In the end, the biblical writers concluded that Israel's election was nothing less than a "miracle"; and who are we, their spiritual heirs, to disagree?

Although archaeology may be successful in recognizing in the material remains certain elements of human behavior and social organization, it reaches its limitations when it comes to ideology. Archaeology does not yet, and probably cannot, comment on the complex, diverse, tangled, and on occasion conflicting political or religious motivations behind the emergence of ancient Israel. We may tend to agree that "Yahwism," whether a revolutionary social movement or not, was probably the driving force. But we cannot define "Yahwism" archaeologically beyond describing religious *practice*. We can only suppose that in the cultural vacuum following the collapse of Canaanite society in the 12th century B.C., there arose in central Palestine a new ethnic consciousness and solidarity, a new polity, a new social order. The emergence of this ethnicity need not have been accompanied by a "revolt" at all; it may be viewed rather as simply a normal and even predictable historical development in the evolution of complex society. Archaeology may provide an "ecology" in which socio-economic change becomes explicable, but it cannot explain the ultimate *derivation* of that change. Insofar, however, as the ideology of the Israelite movement found concrete expression in new economic, social, and religious *forms*, we can hope to trace these forms in the archeological record, since this comprises the "material correlates" of human behavior.

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MONARCHIC PERIOD

A. Sources

1. Biblical
2. Epigraphic
3. Archaeological

B. Origins of the Israelite Monarchy and National State

1. Settlement of the Tribes in Canaan
2. Early Attempts at Monarchy

C. Davidic–Solomonic Empire

1. Sources
2. David
3. Solomon

D. Kingdoms of Israel and Judah

1. Collapse of the Davidic–Solomonic Kingdom
2. Politics of the Two Kingdoms
3. Economy of the Two Kingdoms

E. Israel and Judah: Jeroboam I—Fall of Israel

1. Period of Conflict
2. Period of Cooperation
3. Revolution and Its Aftermath
4. Period of Prosperity
5. Fall of Israel

F. Final Years of Judah

1. Judah and the Neo-Assyrian Empire
2. Reign of Josiah
3. Judah and the Neo-Babylonian Empire

A. Sources

There are three types of sources that the historian can use in reconstructing the history of the monarchy in ancient Israel: the Bible, epigraphical material, and archaeological data. While these sources make the monarchic period the best documented era in the life of ancient Israel, still significant problems remain. The most obvious of these is the chronology of the Israelite monarchy. No universally accepted dating system existed in the ANE. Compounding this problem is the apparent artificial scheme that the Deuteronomistic Historian used in determining various time periods—for example 40 years for the reigns of David (2 Sam 5:40) and Solomon (1 Kgs 11:42) and 480 years from the Exodus to the building of the temple (1 Kgs 6:1).

The first securely dated event in the history of the two kingdoms is the surrender of Jerusalem to Nebuchadnezzar on March 15–16, 597 B.C.E. The Babylonian Chronicles (*ANET*, 564) supply the exact date. All other dates in the chronology of the two kingdoms are approximate within a range of ten to two years. The closer one gets to the fall of Jerusalem, the more precise dating becomes. Theoretically, with the date of Jerusalem's fall in hand, it should be possible to date the other events in the history of the two kingdoms by using the relative chronology provided by the Bible. That is not possible since the Bible's chronology does not allow for precise calculation. It may reflect a schematic rather than an exact presentation of the chronology of the monarchic period. Second there are serious discrepancies between the MT and the LXX that make it difficult to use the Bible to reconstruct this chronology. Any such