In the March 1999 issue of *Near Eastern Archaeology* (62/1), Israel Finkelstein published an article entitled "State Formation in Israel and Judah," in which he re-evaluates the pace and process of complex politics in Israel and Judah in the early first millennium BCE. The following is a response to that article, in which I deal with the data Finkelstein discusses, but mainly with his interpretation of those data. Finkelstein presents two lines of argument—one archaeological and the other historical—which I will treat in turn.

**Archaeological Considerations**

My definition of archaeology, shared with students during almost forty years of teaching historical geography, is that archaeology is the science of digging a square hole and the art of spinning a yarn from it. The interpretation of archaeological evidence has to be addressed anew with every generation of scholars. All of the new techniques for data retrieval in the field and in the laboratory are most welcome. Laser instruments...
archaeology is the science of digging a square hole and the art of spinning a yarn from it.

for measuring, digital photography and many other processes have enhanced even digging the square hole. Laboratory analyses of all kinds—pollen, clay, bones, organic remains and metals—is subjected to a plethora of tests and evaluations. Fine. But in the end, archaeologists are in many ways still on square one. Here is a post-modernist statement that deserves careful consideration; hopefully, the author will not object to its being taken out of the original context (a debate on similar issues):

there is no archaeological record as such, only fragmented material traces of the past (Hamilakkis 1999: 60).

One must avoid the common pitfall of thinking that the available data comprise a comprehensive picture, for any of the periods or phases of antiquity. Every year, as new data come to light, it is necessary to accommodate them within the parameters of previous understanding. But there will always be gaps. Furthermore, field archaeology is said to deal with “facts.” But the question arises, “what is an archaeological fact?” During the past fifty years, careful scrutiny has shown that most of the “facts” on which syntheses and historical interpretations have been based are mainly just the opinions of archaeological “authority figures.” Reference to a particular case in point will be made below. Some historians have objected that archaeological evidence is “mute,” an opinion that does not seem fair. The main drawback of archaeological evidence is its ambiguity. Every archaeological report is the result of decisions on the part of the excavator. What goes into the record is the archaeologist’s opinion of the meaning of his evidence. Statements like “the archaeologist thinks,” “in the excavator’s opinion,” are the bane of the profession. It is easier to rely on such shibboleths than it is to take a long, hard look at the evidence itself.

When evaluating the opinions of excavators in the light of their real data, one other temptation must be avoided: the imputation of motives. Debate and discussion must be confined to data; regardless of the scholar’s personal agenda or motivation (religious, anti-religious, and political); attacking the motivation does not serve to prove a point. In this regard, Finkelstein made one unfortunate statement in his article: “The older consensus model dated the strata associated with this long time period according to relative, circumstantial, theological, quasi-historical, and sentimental considerations (Finkelstein 1999: 36; emphasis mine).” Such a remark has no place in scholarly discussion.

Judean Society
Finkelstein argues that Judah was not a state and Jerusalem was not a capital city until the eighth century BCE. A hypothetical reconstruction of rural life in the Judean Hill country is presented as if it were an indication of the backwardness of Judah in relation to Israel.

Surveys carried out by Kochavi and Ofer indicate that until the eighth century the settlement system in the Judean hills was embryonic, consisting only of a limited number of small, poor sites (Ofer 1994). There is no evidence for a developed hierarchy, including medium and large size sites, which would attest to the existence of regional administrative and trade centers surrounded by peripheral, secondary villages (Finkelstein 1999: 42).

This is Finkelstein’s interpretation of the survey data. He suspects that the number of sites given by Ofer is inflated (Finkelstein 1999: 48 n. 4). In fact, the data can and are interpreted differently by the principle surveyor himself.

In Iron Age IIA (ca. mid-eleventh-tenth centuries BCE), settlement in the Judean hill country almost doubled, compared to the preceding period and any other earlier period. In general, Iron I–IIA constitutes the most significant breakthrough in the settlement history of the Judean Hills. There are thirty-four Iron IIA sites (an increase of ca. 90%), of which thirty were actual settlements. Their total "average" area is 33.5 hectares (a growth of ca. 80%; 19.5 "calculated" hectares). The prominent sites are still Tel Hebron [Tell Rumeida] and Ras et-Tawil (ca. three hectares each) in the plateau of the central range. Sites of the second order, with a size of 1.5–2 hectares, are: Tekoa on the northern desert fringe; Kh. et-Tayyibe, Kh. ez-Zawiyye, and Halhul in the plateau of the central range; Kh. 'Attir in the southern part of the central range; and Khirbet Yaqin on the southern desert fringe. In this period settlement became denser in the south and on the northern desert fringe, and penetrated with great intensity to the southern desert fringe. Settlement activity noticeably diminished only in the northern part of the central range, in which an unprecedented settlement momentum had begun during the preceding period.

The RSI graph of Iron Age IIA is significantly arched (RSI = 0.560), also for the first time in the history of the region. This is a clear indication that the Judean hill country then constituted a part of a more extensive unit, with its center outside the region—in Jerusalem. This is true even with a minimal estimate of the latter’s area (ca. six hectares), and more so according to the estimate of ca. sixteen hectares (including the Temple Mount) (Ofer 1994: 102, 104).

In other words, according to Ofer’s interpretation, there is a marked increase from Iron I to Iron II. Of course, Finkelstein may argue that the dates of Ofer’s pottery are all wrong, a circular argument frequently resorted to in his “new
Judging by the rural nature of life in Judea, one could argue just as true today as it was in biblical and Herodian times. May readily admit that the Judean Hill country population was predominantly agricultural in its subsistence strategies. This is just as true today as it was in biblical and Herodian times. Judging by the rural nature of life in Judea, one could argue that King Herod did not exist, and never had a kingdom. But archaeological remains from sites all over the country (e.g., Caesarea, Sebastia, Herodium, Masada and Jerusalem itself) belie such an assumption. The same is true for Judah in Iron II A.

Shephelah and Negeb

The two main peripheral zones of the Judean kingdom were the Shephelah on the west and the Negeb on the south. The latter is primarily the modern Beer Sheva' Valley and the Besor basin, east and west of modern Beer Sheva' (Rainey 1984: 90). In the Shephelah, one has only to think of Gezer, Beth-shemesh and Lachish, all of which have archaeological remains that can be assigned to Iron II A, i.e., the tenth century BCE. The recent excavations at Beth-shemesh have begun to recover considerable pre-eighth century remains. Lachish, of course is a key site. Here the most recent excavator, David Ussishkin, assigns Level IV with its massive brick fortification wall and its triple chambered gate, to the building activity of either Rehoboam or Jehoshaphat (Ussishkin 1997: 319; see also 1983: 173; he could have suggested Asa as an equally likely candidate). Here he is beginning to hedge since in his earlier preliminary publications he was strongly in favor of Rehoboam. As for Stratum V, he now says, "it is usually assigned to the United Monarchy" (Ussishkin 1997: 319). Stratum V is not well-represented archaeologically, but it is clear that houses rimmed the perimeter of the site. It has been suggested that Stratum IV was destroyed by the earthquake in the reign of Uzziah (Amos 1:1; Zimhoni 1997: 200). This could fall anywhere between 782 and 753 BCE. So Lachish IV would have been wrecked sometime in the first half of the eighth century BCE. However, this earthquake theory is shaky in the extreme. Furthermore, the ceramic horizon of Lachish IV is quite different from that of Lachish III (destroyed by Sennacherib in 701 BCE; Ussishkin 1977) and the transitional ceramic phase between IV and III (noted at other sites) is missing at Lachish (Zimhoni 1997: 173, 208). Stratum III, a rebuild of Stratum IV now included some major administrative structures in the center of the mound. An alternate scenario, also tentative of course, is that Uzziah could originally have built Stratum III, which required considerable efforts and expense in its construction, in the first half of the eighth century. After the great earthquake, Ahaz or Hezekiah (more likely) could have restored it in the latter part of the eighth century BCE. That leaves Stratum IV for the ninth century, but since it, too, had several building phases (using the original massive wall and gate), it could have been first built by Asa, or even Rehoboam. Even if one insists on the later date, the existence of Lachish IV in the ninth century belies Finkelstein's theory that Judah did not become a kingdom until the eighth century BCE. Lachish was certainly not an independent state in the tenth or ninth centuries.

In the Negeb, one only has to cite Arad, Malhata and Tel Beer Sheva' as examples of fortified sites which required a central government to initiate such building projects. The local population resources could never have supported such projects. Stratum XI at Arad was founded either in the middle of the tenth century or in the ninth century. Its pottery repertoire seems to correspond to that of Lachish IV (see the remarks of Zimhoni 1997: 206). Therefore, it too must belong to the early ninth or the late tenth century BCE. In spite of many objections, there is still a strong possibility that Arad XI is the haqru destroyed by Shishak in 925 BCE. If only one could prove that haqru means "fortified citadel" (= haqrd' of the Talmudic period; Rainey 1998a: 445-46) or even *hgr (as in Phoenician hgr šmr ʾen "enclosure wall"), there would be no question that Stratum XI would have to be the fort destroyed by Shishak. In any case, Arad XI and the contemporary strata at Malhata and Tel Beer Sheva' (Stratum V) testify to central initiative during the early ninth and probably also the late tenth century BCE. Therefore, the real archaeological evidence for dating the beginning of the Judean kingdom is mainly in the Shephelah and in the Negeb.

Finkelstein also bases much of his argument on the work of D. W. Jamieson-Drake (1991). Actually, Jamieson-Drake's collection of archaeological materials only shows that in the tenth century BCE there is a pronounced outburst of building energy. The haphazard nature of most of his evidence, including the proliferation of stamp seals in the late eighth and seventh-sixth centuries BCE, provides no basis for Finkelstein's sociological deductions (Finkelstein 1999: 39). There is no way that Jamieson-Drake's dissertation can be made to support Finkelstein's socio-political reconstruction. Jamieson-Drake's arguments are directed at the possibility of "schools" in ancient Judah. Though epigraphic material is relatively abundant in the eighth and seventh-sixth centuries, this is to be expected given the usual state of archaeological remains in the various sites. Furthermore, building projects, which Jamieson-Drake admits for the tenth and ninth centuries BCE, require considerable knowledge of mathematics and geometry. They require organized logistics. Documents from the ancient Near East show that logistics require the keeping of accounts. That the main literate cadre was in Jerusalem, associated with the storehouses of the temple and the political management, should be obvious. The sparse remains of Jerusalem and the high probability that most public writing was on perishable materials belie the assumptions of Jamieson-Drake. Dependence on Jamieson-Drake's study is another weak point in the Finkelstein structure. Incidentally, it has recently pointed out (Master 2001: 127-28) that the state formation theory upon which Jamieson-Drake and Finkelstein have based their interpretations is now considered obsolete in anthropological circles. The developed state in the ancient Near East also had a fundamental component of tribal, clan and family associations (Master 2001: 128-30).
Jerusalem and Byblos

A major factor to be reckoned with is the nature of sites of antiquity in the hill country (Rainey 2000a). This characteristic was well-known to Albright, who described it in his critique of the view of Bergman (Biran) concerning the location of Anathoth:

Dr. Bergman's suggestion that the traces of occupation before the exile are not sufficiently numerous to warrant our locating Anathoth here, is unnecessary.... It is hard to overestimate the effect of more than two thousand years of denudation on such exposed summits, bearing the ruins of unwalled villages, at the top of the watershed ridge in central Palestine (Albright 1936: 36).

Today one can add the evidence from el-Jib, Tell en-Naṣbeh, Tell el-Fül, Ramat Rahel and Khirbet Rabûd. At all of these sites, bedrock is exposed or near the surface in the center of the tell. And one might add the ancient tell of Bethlehem upon which stands the Church of the Nativity; the sacred cave shows that bedrock would have been exposed on top of the mound (Prag 2000: 170–71, 178–79).

Concerning Jerusalem, the Herodian engineers simply did not leave any previous remains when they built the giant temple platform in Jerusalem. Hadrian, the Moslems and the Crusaders also must have radically changed the inner surface of the platform. The original tell of Late Bronze and Iron Age Jerusalem was surely the temple mount of today (Rainey 2000a; Knauf 2000: 76).

The ancient city of Byblos is another case in point. Nearly seventy Amarna letters were written there and sent to Egypt. They make it clear that this important commercial export city had a palace and at least one temple, all filled with riches:

“May the king, my lord, not neglect the city (Byblos), since there is very much silver and gold in it; in its temple there is much property” (EA 137: 59–62).

But the excavations found no Amarna Age stratum at Byblos! From Crusader and Roman they went directly into the Middle Bronze Age! It is said that the excavator failed to recognize some elements of Late Bronze Age remains. Such a case is probably due not only to the poor methods used by the excavator, but also to the badly disturbed state of whatever Late Bronze Age strata had been there. It is also possible that a portion of the tell, perhaps with its royal palace, has been eroded away by the action of the sea (as happened to a large portion of Tel Michal on the coast north of Tel Aviv, and probably also at Ashkelon on the southern shore). Furthermore, the Tale of Wenamon attests to a major seaport at Byblos with a palace and a temple in the eleventh century BCE, but no eleventh century stratum was found. That story may or may not be pure fiction (it has some features that suggest a true diary report), but it's dating to the 21st Egyptian Dynasty can hardly be disputed. No one in a later Dynasty (such as the 22nd Libyan Dynasty) would have built the story on characters such as the high priest in Thebes and the first king of the 21st Dynasty. The royal inscriptions from Byblos, including the funerary inscription on the sarcophagus of Ahiram, the Yeḥimilk building inscription, the Abibaal inscription on the base of a Shishah Statue and the Elibaal inscription on a statue of Osorkon I, all attest to a rich occupation at Byblos in the tenth century BCE. Those Phoenician texts, especially the inscriptions on royal Egyptian statues, demonstrate strong commercial and diplomatic relations between Byblos and Egypt during the early reigns of the 22nd Dynasty. As symbols of those relations, Shishak and Osorkon sent statues to be placed in the temple at Byblos. The reigning Byblian kings "modestly" took credit for bringing this about and thus inscribed their names on the respective statues. It would hardly be a case of later Byblian rulers collecting antique statues to donate to their temples. But like the well-documented Amarna period, neither is there a stratum at Byblos from these tenth centuries! The epigraphic evidence is conclusive: Byblos was a thriving city with major buildings during the fourteenth, eleventh and tenth/ninth centuries. The absence of stratified archaeological remains from those periods is indeed regrettable but not determinative for the occupation history of the site. The absence of the appropriate strata only confirms the statement by Hamilalkis (1999: 60) cited above.

Na'a'am (1996) applied the testimony of the Amarna letters to the problem of Jerusalem. There is sufficient evidence within those texts to show that there must have been a vassal city-state there in the mid-fourteenth century BCE, e.g., the reference to the appointment of the local ruler to fill the office hereditary to his own family dynasty:

Look, as for me, it was not my father and it was not my mother who put me in this place; the strong arm of the king installed me in my father's house (EA 286: 9–36).

For the tenth century, Na’a’am (1997: 46) cited several biblical passages as having the nature of original sources: the list of David’s wives and sons (2Sam 3:2–5; 5:14–16); the list of David’s cabinet officers (2Sam 23:8–39); the list of Solomon’s high officials (1Kgs 4:2–6); the list of Solomon’s twelve administrative districts (1Kgs 4:7–19); and Solomon’s building activities (1Kgs 9:15, 17–18). To these passages one may add the detailed description of the public works departments employed in the Solomonic construction projects, namely the "forced labor" (1Kgs 5:13–14 [Heb 5:27–28]) and the "burden bearers and hewers of stone" (1Kgs 5:16–17 [Heb 5:29–31]; see Rainey 1999).

A crucial point in the current debate is whether the passages invoked by Naaman and Rainey are authentic ancient records incorporated into the biblical narrative or whether they are simply later concoctions (see below).

Northern Israel

In the Northern Kingdom, the arguments in behalf of occupation during the tenth century hinge on dating the strata at major sites like Megiddo and Hazor. Finkelstein bases his main arguments on his interpretation of Strata VI and V at
A Funerary inscription on the sarcophagus of Ahiram, king of Byblos. This text must be earlier than Alsilsaal and Ellisaal. Photo courtesy of the National Museum, Beirut.

An inscription of Abibaal, king of Byblos, appears on the base of this statue of Pharaoh Shishak (945-924 BCE). After Dussaud (1924: 145 fig. 5).

The bust of Pharaoh Osorkon I (924-889 BCE) on which the inscription of his contemporary, "Elibaal, king of Byblos, son of Yehimilk, king of Byblos," is clearly visible. These Phoenician texts demonstrate strong commercial and diplomatic relations between Byblos and Egypt during the early reigns of the 22nd Dynasty and confirm for the author the importance of Byblos in the tenth century BCE despite a lack of supporting archaeological data from the site. Photo courtesy of the Louvre Museum.

Megiddo, where he is co-director of the current excavations:

At Megiddo, the key stratum is VIA, remains of an elaborate city which was destroyed in a terrible conflagration. The most characteristic feature of its pottery assemblage, according to both the Oriental Institute and the recent excavations, is the absence of Philistine Bichrome and collared-rim jars (both types which appear in the previous stratum in the Megiddo sequence, VIB). This means that even according to the prevailing chronology Stratum VIA can hardly be dated in the 11th century BCE. The assemblage of Stratum VIA is the last at Megiddo to depict "Canaanite" motifs. This stylistic root encouraged attempts to date the stratum prior to the time of the United Monarchy. The pottery of the next two strata in the Megiddo sequence, the poor VB and the monumental VA–IVB, is very different. All "Canaanite" features disappear and typical Iron II types are introduced (Finkelstein 1999: 38).

At the end of the 2000 season, Finkelstein told visitors that he now has Carbon 14 dates for Stratum VIA that place it in the tenth century BCE. One must wait for the publication of that evidence but even if it seems to be correct, this does not cancel the testimony of written sources (discussed below) for the existence of a united Israelite monarchy in the tenth century. The clear indication of the biblical sources is that the population in the plains and valleys was Canaanite and only now being incorporated into "greater Israel." Stratum VIA could easily be the stratum destroyed by Shishak, or it could also be a stratum destroyed by David and his forces. The exact correlation between that destruction level and the written sources will probably never be conclusively established. The testimony of any written evidences must be established on their intrinsic merits and not on the opinion of a site centered archaeologist.

Finkelstein and Ussishkin dismiss the statement about Solomon's fortification of Gezer, Megiddo, and Hazor (1Kgs 9:15) but it is not the archaeologists who can determine the validity of a biblical verse. Finkelstein and Ussishkin have entangled themselves in a web of circumstantial arguments based on weak stratigraphic evidence from Tel Jezre'el, plus their preconceived notions about Megiddo. The writing of Israelite social and political history cannot be based on such circular reasoning.

**Historical Considerations**

"Significant Historians"

The historical aspect of Finkelstein's article consists mainly of the invocation of various modern day historians who espouse his own views on the basis of his archaeological deductions. One must object to the assumed right of archaeologists to be the final arbiters as to who is a valid historian and who is not. Finkelstein refers to a string of "minimalists" as supporters of his position:

Coupled with this re-evaluation of the material evidence, the rise of the minimalist school in biblical studies (e.g., Garbini 1988; Davies 1992; Thompson 1992; Lemche 1994) is particularly significant. The work of this group of scholars, and, in fact even earlier currents in research (e.g., Van Seters 1983; Miller and Hayes 1986), has caused even less radical scholars to reconsider the weight given to the biblical material in reconstructing the early history of Israel (Finkelstein 1999: 36).
In a recent work, one of those writers has made reference to “the Ranke game” (Lemche 1998: 22–34). He admits that Ranke’s approach to history based on the analysis of written sources (Ranke 1824) is valid. But he dismisses the Hebrew Bible as a source for history. Since in his opinion there are no genuine historical sources in the Hebrew Bible and its versions, there is no scope for playing “the Ranke game.” It is the contention of this paper that there are such sources in the biblical text. I am not denying that there are poetic and folkloristic passages. But if scholars are to identify truly historical data in the Hebrew Bible, they must be trained in the study of real ancient Near Eastern sources. Biblical studies without the discipline of ancient Near Eastern research is simply chasing one’s tail, bringing up the same cud chewed by theologians and religious pundits throughout the ages.

LEOPOLD RANKE (1795–1886)

The German historian Leopold von Ranke was a pioneer in the development of critical historical scholarship. He applied critical methods of text analysis to the study of modern history, insisting on the careful examination of primary sources. Ranke’s method of historical research became the model for serious historians in the nineteenth century. But he was not simply a fact-oriented positivist; he believed that great moral forces were manifest in history. In his major works he traced the development of the modern European world. His heavy reliance on documents meant that he focused on the foreign affairs of the great powers and on military events. Yet he also gave considerable emphasis to religious ideas. Today, perhaps more emphasis is placed on the economic, social, and cultural aspects of history, but nevertheless, it is incumbent on historians to utilize documentary evidence in doing so. The ancient Near East has produced an abundance of documents in cuneiform, hieratic and Northwest Semitic scripts, that furnish homely details about the lives of individuals and groups.

Finkelstein is finding support for his new archaeological rewrite of Israelite history in the plethora of recent publications that can legitimately be called “Theologized History.” Here one can make no distinction between the extreme minimalists and the extreme fundamentalists. Ideology aside, it would appear that there is no intrinsic difference in method between a Thompson (with his “mythic present”) and a Bimson, between a Lemche and a Livingston. Essentially, they are all commentators on the Bible who do not employ Ranke’s methodology of source research in ancient documents. The ability to read ancient documents in their original language and script and the talent for analyzing such materials for their historical information is essential to any effort at evaluating biblical passages that may have an historical basis. The late H. J. Polotsky once lamented that when oriental studies were being established in nineteenth century universities, the Semitic languages were essential to their curriculum, but that Hebrew had remained in the hands of theological faculties (Polotsky 1964: 100). One could say the same thing about Biblical history. Theology is a legitimate field in its own right, but historical analysis should not be based on theological premises, negative or positive. It must be achieved through “the Ranke game.”

Perhaps enough has been said about the minimalists. Instead, one may address a more “mainstream” school of historians, who are also cited by Finkelstein as supporting his approach. Among the “earlier currents of research” Finkelstein refers to J. Maxwell Miller and John H. Hayes (1986) whose work has had a great impact on the study of Israelite history. Their textbook is considered a standard in the field, at least for undergraduate courses, and no one seems to question the authority of their views. As a sample test case, attention will be directed to what was originally the basis for Miller’s entire reinterpretation of the biblical history of the ninth century BCE in Israel. This is especially cogent since the nature of ninth century Israel is a central focus in Finkelstein’s exposition. The Miller-Hayes approach is epitomized in the following statement:

The concluding summary of Ahab’s reign in 1 Kings 22:39–40 states that Ahab “slept with his fathers”—a phrase normally used in the Genesis–II Kings account to refer to the burial of a person who died a natural death. (Miller and Hayes 1986: 274; see also Miller 1966, 1967, 1968).

Miller and Hayes’ interpretation is based on a superficial argument by Alfred Jepsen (1942: 155–59). According to Jepsen, a king who “slept with his fathers” could not have died a violent death; therefore, Ahab did not die in battle; therefore Ahab did not engage in wars with Aram. Jepsen’s view was soundly refuted by Bin-Nun (1968) over thirty years ago. She pointed out that the expression “slept with his fathers” pertained to any king who was succeeded by his rightful heir. Naturally, a usurper followed the assassinated kings of the Northern Kingdom, so the dead king was not accorded the rites due to him from his heir and successor. Ahab, on the
other hand, was followed by his son, Ahaziah, who in turn was followed by his own brother, Joram. Ahab died a hero's death on the field of battle. But even in the case of Amaziah, who was assassinated, it is said of his rightful successor, Uzziah, "He built Elath and restored it to Judah after the king slept with his fathers" (2Kgs 14:22).

The main point here is that, as a result of adopting Jepsen's erroneous theory, Miller, Hayes and their disciples created complete chaos in ninth century Israelite history. They took all of Ahab's battle passages and assigned them to later kings of Israel. Their work was only cursorily linked to the data from Assyrian inscriptions. But since their book has become a standard, their views have become a widely accepted position.

Chronological Pegs

The whole trouble with seeking a link between archaeological strata and historical sources for the tenth century BCE is that firm links do not exist. Finkelstein has articulated this lamentable fact:

There is not a single chronological anchor (that is, an archaeological find that provides an absolute date to fix an associated assemblage) between the days of the Egyptian XXth Dynasty rule in Canaan in the twelfth century BCE and the Assyrian campaigns in the late eighth century BCE. This is an archaeological "dark age" of over four centuries, one which covers most of the Iron I, the days of the United Monarchy, and the entire history of the northern Kingdom of Israel (Finkelstein 1999: 36).

Precisely at this point one can take note of the chronological pegs (derived from archaeological discoveries) firmly established by sources outside of the biblical text. For present purposes, it will suffice to mention the reference to Ahab in Shalmaneser III's inscription for 853 BCE (Kurkh Monolith, 11, 91–92; Grayson 1996: 23; ANET 279a) and for Yehu in 841 BCE (Black Obelisk, epigraph; Grayson 1996: 149; ANET 281a). The fourteen years assigned to the Israelite kings Ahaziah and Joram for this twelve-year period confirm Thiele's contention that the northern rulers numbered their years of reign by the non-accession system. But the campaign of Pharaoh Shishak I in 925 BCE, which took place in the fifth year of Rehoboam, is also a factor. By using Thiele's calculations of the dates of the Israelite kings, we work back from 841 to 853 to 925 and on back to 931–930 BCE, the date when both the Israelite and Judean dynasties had their beginning! This works out mathematically; it is not an accident (Thiele 1983: 87, 98 et passim). So the written sources in the Book of Kings are anchored to firmly dated Assyrian and Egyptian records and indicate that 931–930 BCE was a crucial date in Palestinian history. Two separate dynasties were born. This in itself is a major argument in favor of a previous unified monarchy under Reho-bo'am's father, Solomon.

That the Judean dynasty was known by the name of its eponymous ancestor David has been confirmed by the discovery of the Tel Dan Aramaic inscription, which mentions a "king of Israel," alongside a "king of the house of David."
Further corroboration was furnished by Lemaire's discovery of the name "House of David" in the Mesha inscription, which also deals extensively with a rival entity called Israel, initially ruled by Omri. The Tel Dan inscription sent the minimalists into a frenzy of activity: the Tel Dan text was said to be a blatant forgery and anyway supposedly did not mention the "house of David," but rather the "temple of Død," or something similar (Thompson 1997). Now it becomes incumbent upon them to deconstruct the Mesha inscription. Their efforts to do so will not make any impression on serious scholars who are trained in Northwest Semitic languages and historical geography (Rainey 2001).

In the ninth century, the Judean kingdom was called the "House of David." It bore the name of the very ancestor whom the Bible happens to credit with its founding in the tenth century BCE. This is not just archaeological evidence; it is not conclusively linked to any archaeological stratum. And furthermore, its validity seems incomprehensible to archaeologists not trained in sound linguistics and historical philology.

### Tenth Century Solomonic Administration

Among the biblical passages cited by Na'aman as having the character of authentic
Towards the Future

Some archaeological colleagues, especially younger ones like Finkelstein, may view this paper as just an old dinosaur arguing for outmoded methodology and disciplines. Yet there is a thriving generation of young Assyriologists and Egyptologists of great talent who are devoting their energies to the first hand reading of ancient documents. Ancient Near Eastern philology and linguistics are alive and well (with little attention being given to post-modernism).

Finkelstein's archaeological arguments are based largely on his personal interpretation of the data and involve a great deal of subjective selectivity. The fundamental issue that must be dealt with here is whether an archaeologist, untrained in detailed historical linguistics and philology, can use his subjectively interpreted archaeological data to create a history that is in contradiction to the written sources. Must the properly trained historian be forced to adjust his interpretations to the dictates of the archaeologist? Is field archaeology (with its associated scientific techniques) really the new substitute for study of the biblical text and other ancient Near Eastern documents? Or should one insist that historical linguistics and philology, dealing with a specialized type of archaeological artifact, namely inscriptions, are primary tools for reconstructing the ancient history of Israel and the ancient Near East to which archaeological evidence can be compared and critiqued? The readership of NEA will have to confront these issues.

These comments are intended as an appeal to scholars and teachers concerned with the history of ancient Israel. Will you ignore the original texts in favor of the "latest trends" or will you get serious about the "Ranka Game"? In the past fifty years of biblical study, generation after generation has chased its tail around some panacea, some "new explanation" (archaeologists and historians alike). You, the individual, must decide how the ensuing decade will be spent.

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