Chronology and United Monarchy

A Methodological Review*

By Raz Kletter

Recently the research agenda of the study of early Iron Age Palestine has shifted. The focus on the Iron Age I settlement is ‘out’, to debate the historicity of the United Kingdom and the archaeological chronology of the 10th–9th centuries B.C.E. is ‘in’. ISRAEL FINKELSTEIN’s ‘low chronology’ (henceforward, LC)1 is debated against the former, ‘high chronology’ (HC)2. This paper does not offer a new chronology, nor does it compare pottery assemblages and strata. Rather, it reviews the whole body of FINKELSTEIN’s writing on the LC and discusses its methodology and development from a perspective of seven years (1995–2002)3. I assume that readers are familiar with the debate, and will not describe each argument and counter argument. I will study the achievements of the LC as stated by FINKELSTEIN; its theoretical basis and some of its basic conceptions, and comment on some related issues. This paper is not written in order to support the historicity of the United Monarchy (cf. Solomon, holding ‘his’ temple, Taf. 1).

In the course of this study, some phases of development are noted within the LC. Since the correlation between date of writing and date of publication is not always direct, these phases are not specified by exact years – I have no wish to start a debate about the chronology of the low chronology. I have tried to maintain accuracy when translating from Hebrew versions of LC papers.

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3 Some relevant studies reached my attention, or were published, after the present paper was completed. These were added to the reference list, but not reviewed. They include studies about 14C dates supporting the HC (BRUINS/VAN DER Plicht/MAZAR 2003); a review of Kinneret (KNAUF 2003); publication of the Tel Rehov strata (MAZAR 2003); and rejoinders by FINKELSTEIN (2003a; 2003b) and BEN-TOR (2003). For use of “chiefdom” – “state” for ancient Israel add MATTHEWS/BENJAMIN 1993, especially 96, 159–162. MCNUTT (1999, 104–138,213–215) made a detailed review of various models – and the problems inherent in their use – concerning ancient Israel. She noted that they lead to conflicting views (MCNUTT 1999, 213).
1. Observations on the Model of State Formation used in LC Papers

The concept of “states” that emerge from earlier “chiefdoms” is central to the LC. This concept has not been investigated, since critics accepted it at face value (e.g., BEN-TOR/BEN-AMI 1998, 36; DEVER 2001, 128).

1.1. The Model and its Use

FINKELSTEIN does not discuss the theory of the shift from “chiefdom” to “state”, since he seems to believe his model is valid. In 1996, he briefly referred to it:

“If we could safely identify the relevant strata, we would be able to produce a vivid picture of the material culture and settlement pattern of the tenth century BCE; deal much more effectively with the question of the probability of the existence of a developed state in Palestine at that time [. . .]. Checking the above-mentioned remains and other finds in terms of modern socio-political theory would indicate that the execution of such large-scale building activities would have required an advanced administration and a sophisticated system of management of manpower” (FINKELSTEIN 1996b, 177). Another reference is later: “First, a general question of theoretical orientation, is how to identify the socio-economic and political characteristics of a mature state organization. The second relates to material culture: what are the attributes of an ancient state which can be traced in the archaeological record? With both issues the main difficulty is to distinguish between the properties of a state and those of an alternative, often earlier, complex political entity like a chiefdom. These two issues have been dealt with exhaustively [. . .] and this is not the place to rehearse their complex intellectual history (but see, for example, different articles in Claessen and Skalnik 1978; 1981). My approach to these questions does not diverge from mainstream sociopolitical research, according to which, a full-blown state is characterized by a well-stratified society, one directed by a specialized public administration led by a ruling stratum which extends beyond the immediate kinship circles of the ruler. Writing systems are characteristic of full-blown states as are organized industrial production and the erection of monumental structures” (FINKELSTEIN 1999c, 39, cf. 1999a, 134; FINKELSTEIN/SILBERMAN 2001, 158–159).

FINKELSTEIN used the “neo-evolutionary model”, which is now sometimes called “old neo-evolutionary model”, developed mainly by FRIED (1967) and SERVICE (1962; 1975). This model formed a sort of a stepladder of evolution of human societies: “bands” – “tribes” – “chiefdoms” – and finally “states”. Apart from these scholars, FINKELSTEIN mentioned only CLAESSEN/SKALNIK (1978). The most recent theoretic reference in all LC papers is to CLAESSEN/SKALNIK (1981). FINKELSTEIN uses this model by identifying “chiefdoms” and “states” in ancient Palestine, based on a short checklist of archaeological traits. In his view, this proves that the United Monarchy was not a “full-blown state”, but a “chiefdom”, or an “early state”, while “full-blown” statehood was reached only later. Sometimes he wrote that Judah reached “full-blown statehood” in the 8th century B.C.E. (FINKELSTEIN 1998b, 172–173); at other times, it was the 9th century B.C.E. (FINKELSTEIN 2001a, 110–111) or the 7th century B.C.E. (FINKELSTEIN 2001a, 111); but we even hear that Judah perhaps never became a “full-blown state” (FINKELSTEIN 2000b, 133). Following are some of FINKELSTEIN’s statements about Judah:

“The Kingdom of David and Solomon could have been a chiefdom, or an early state, in a stage of territorial expansion, but with no monumental construction and advanced administration” (FINKELSTEIN 1998b, 185). “In fact, archaeology indicates that Judah reached full-blown statehood a bit later than the northern kingdom, mainly in the eighth century B.C.E. This is not to say that there was no David and Solomon, or [. . .] early state in Palestine in the tenth century with its centre in Jerusalem. Yet [. . .] Jerusalem was no more than a small stronghold for the ruling elite” (FINKELSTEIN 1998b,
172–173). “Judah never developed similar standard, monumental architecture, neither in the 8th century BCE when it reached fully developed statehood, nor in the 7th century BCE, when it reached its cultural zenith. Even in late monarchical times Judah remained a state of one large city surrounded mainly by rural population” (FINKELSTEIN 2000b, 133); “before the late eighth century B.C.E. Jerusalem in particular and Judah in general could not have functioned as the hub of a full-blown state with a developed bureaucratic apparatus” (FINKELSTEIN 2002b, 124). “Judah emerged as a full blown state in the late 8th century [. . .] with the first signs of statehood appearing a century earlier [. . .]. In the time of the Shosheng campaign Judah was a marginal dimorphic chiefdom [. . .] ruled from a small village” (FINKELSTEIN 2002a, 112); “though the ninth century finds in Jerusalem and the hill country of Judah indicate some development from the previous centuries, they do not mark a break-through from the state-formation point of view” (FINKELSTEIN 2001a, 109); “in the first half of the ninth century, under the influence of the Omrides, Jerusalem made the first steps in its development from a small, Amarna-type government stronghold, to an elaborate capital. And this was also the beginning of the rise of Judah as a state” (FINKELSTEIN 2001a, 110–111). David was a “highland chieftain” (FINKELSTEIN/SILBERMAN 2001, 135). “The earliest manifestations of statehood which can be related to Judah in the Beer-sheba Valley [. . .] should be dated to the 9th century BCE [. . .]. Judah emerged as a full blown state in the late 8th century” (FINKELSTEIN 2002a, 111–112; cf. 2002c, 132; cf. FINKELSTEIN/SILBERMAN 2002, 69). “Judah as an early state is an outcome of Omride political and economic ambitions. [. . . Judah] had the necessary infrastructure to make the big leap forward in the second half of the eighth century BCE. This last step to full statehood came with the destruction of Israel and the incorporation of Judah into the Assyrian world system” (FINKELSTEIN 2001a, 111; cf. 1999c, 48).

The part that relates to Jerusalem is based on negative evidence:

“over a century of archaeological investigations in the City of David have failed to uncover major evidence for a 10th century city” (FINKELSTEIN 2000b, 129). “Archaeology shows absolutely no sign of a great 10th century territorial state ruled from Jerusalem” (FINKELSTEIN 2001b, 339). “It is inconceivable that a large state, engaging in monumental construction in far away northern sites such as Megiddo and Hazor, would be ruled from a poor highland village, lacking any sign of developed architecture” (FINKELSTEIN 2000b, 130; cf. 1999c, 40).

FINKELSTEIN used the same model for Northern Israel:

“These processes include the formation of the first fully developed territorial states in Israel and Damascus” (FINKELSTEIN 2000b, 133; cf. 1999c, 40; cf. 2000a, 238). “All this made the Northern Kingdom at the time of the Omrides a potent regional state” (FINKELSTEIN 2001a, 109). “The Omride state was established on the solid foundations of a highly developed settlement and demographic system [. . .]. The Omrides had an ambitious agenda; they opted for expansion [. . .] and the creation of a large territorial, ‘multi-ethnic’ state” (FINKELSTEIN 2001a, 109). “Without archaeology we would never know that the Omrides established the first real territorial state in the Levant” (FINKELSTEIN 2001b, 329).

FINKELSTEIN applied the same model to Late Bronze Age Canaan, identified as a “chieftdom” and compared with 10th century B.C.E. Judah (FINKELSTEIN 1996b, 185; 1998b, 173 note 6). David was a “chief” like Abdi-Ḫepa, the “Davidic polity” was only “a dimorphic highlands chieftdom” (FINKELSTEIN 2002c, 149; cf. 1999c, 44; FINKELSTEIN/SILBERMAN 2002, 68).

FINKELSTEIN used this model for other situations, e.g., in identifying Ḥirbet el-Meššāš stratum II as a “desert chieftdom” (FINKELSTEIN 2002a, 115; “desert polity” in FINKELSTEIN 2002a, 116); or in identifying a “Saulide chieftdom” (FINKELSTEIN 2002a, 127).

This model permeated a large amount of writings about ancient Israel before the LC. Most influential was JAMIESON-DRAKE (1991), but the FRIED/SERVICE model was used for early Israel much earlier by FLANAGAN (1981; 1988), FRICK (1985; 1986), HAUSER (1986) and many others (cf. DEVER 1994, 213; 1997, 247–250). They already allowed Israel to climb the
ladder from “tribe” to “chiefdom” and “state”. FINKELSTEIN (and the Copenhagen school before him) merely shifted the steps, naming the United Monarchy “chiefdom”. The embeddedness of this model is reflected by NA’AMAN:

“using ‘modern’ sociological definitions, they suggest that Jerusalem became the center of a state no earlier than the eighth century B.C.E.” (NA’AMAN 1996, 17). “Jamieson-Drake […] used a sociological model and establishes baseline parameters to analyze the archaeological data of the kingdom of Judah. He suggests clear definitions for a chiefdom and for a state, and criteria for the transition from one stage to another [. . .]. Throughout his work Jamieson-Drake uses technical sociological nomenclature and presupposes a general definition of ‘chiefdom’ and ‘state’ […] His conclusions are sound and reasonable and accord well with the survey conducted recently in the area of Judah. [. . .] Anthropologists and sociologists […] use more ‘objective’ criteria for defining such entities. According to their criteria […] the political formations in the highlands in the Late Bronze Age are defined as chiefdoms, that is, political centers governing territories with mixed sedentary and pastoral population (Finkelstein 1992:206–207)” (NA’AMAN 1996, 21). NA’AMAN (1997a, 47 note 20) added: “Modern definitions might be more ‘scientifically’ accurate”.

It seems an uneasy statement, for the word scientifically is placed in brackets, but there is no explicit criticism. JAMIESON-DRAKE (1991, 32–47) mainly followed FRICK defined key archaeological variables which would be related to urbanization, integration of settlement, public works and luxury items. He took the “state” as granted, and concluded:

“there is little evidence that Judah began to function as a state at all prior to the tremendous increases in population, building, production, centralization and specialization which began to appear in the 8th century. The limited quantity of data could only account for this finding if a reason could be found for different recovery of 10th- and 8th-century materials. Lacking such an explanation, the disparity […] is best explained as reflecting the rates of deposition of those materials” (JAMIESON-DRAKE 1991, 138–139).

This is based on negative evidence, and a simple explanation for the different rate of accumulation exists (below). JAMIESON-DRAKE (1991, 140–144) noticed that scholars who use the FRIED/SERVICE model for ancient Israel reach contradictory conclusions. He never offered definitions of his own for “state” and “chiefdom”. He wrote:

“The transmission from chiefdom to statehood for Israel has been assumed often and explained seldom. The evidence provided in this study and technical definition of ‘chiefdom’ as it appears in the sociological literature, seem to indicate that this term may be one of the most applicable to the level of administrative control present in 10th century Judah” (JAMIESON-DRAKE 1991, 144).

Yet, he was cautious and stressed that his conclusions about the social complexity of the United Monarchy were tentative and “not satisfactorily answered” (JAMIESON-DRAKE 1991, 158). Unlike JAMIESON-DRAKE, FINKELSTEIN (1999c, 39) knows no hesitation: “The date of the rise of the Judahite kingdom as a full-blown state has recently been treated by Jamieson-Drake […] it is difficult to argue against his conclusions, as the archaeological data, or more accurately lack of data […] speak for themselves”. A claim that something speaks for itself is hardly a scientific argument especially if it is a lack of this something.

1.2. Terminology

A first crucial observation about FINKELSTEIN’s use of this model is the wide variety of terms he employed just for the “state”: “a state”; “an early state”; “a real, full-blown state”; “full blown statehood”; “real statehood”; “client state”; “national state”, “national, ethnic state”, \[\ldots\]
“potent regional state”, “territorial state”; and “territorial, multi-ethnic state”. That is apart from other, neutral terms, such as “historical polity”, “polity”, “entity” and “territorial entity”. The last denotes “city states for all purposes” (FINKELSTEIN 2001a, 108). FINKELSTEIN never defined any of these terms, or explained in what ways (if any) they differ from each other. These terms are added to the discussion according to convenience, but make his “state” concept vague. Why so many terms? Because just one term, “state”, will not prove the issue. Yet, these various terms are not part of the original model (below). FINKELSTEIN extended the “full-blown” concept to cities:

“The five 9th century sites discussed here served more as royal and administrative centers than usual cities. […] Larger, full-blown cities developed somewhat later, in the 8th century” (FINKELSTEIN 2000b, 122).

What is a “usual city” against “a royal and administrative center”, and a “full-blown city”? FINKELSTEIN even spoke about the “Pan Israelite idea”, which, to the best of his knowledge, “first surfaced in a full-blown shape at that time” (2001b, 326). This does not clarify much. Was this a new idea, or just an old one now re-surfacing? What shape did it have before it became “full-blown”?

1.3. Tensions in Using the Model

FINKELSTEIN admitted that he cannot decide if 10th century B.C.E. Judah was “a chiefdom, or an early state, in a stage of territorial expansion” (1996b, 185); or if 9th century B.C.E. Judah was a “client state (or better, chiefdom)” (FINKELSTEIN 2001a, 110). If his model is valid, can there be confusion between two ‘steps’? Furthermore, no step in the FRIED/SERVICE model is better in a moral sense. When we think about biological evolution, we do not appreciate the squirrel as better than an ameba. The words “client state (or better, chiefdom)” (above) probably mean that the definition of Judah as a “chiefdom” is better for the LC, since if it was a “chiefdom” in the 9th century B.C.E., it supposedly could not have been a “state” earlier (societies climb one way in the old model, from “chiefdoms” to “states”).

In 1999, FINKELSTEIN concluded that Judah was not a “developed territorial-political entity”, but a “10th century polity”, wrote that it could have been “an expanding early state” but not a “full-blown, mature state”, called it a “dimorphic chiefdom” and compared it to a city-state of the Late Bronze Age (1999c, 42–44). Then he spoke about key contrasts between a “dimorphic chiefdom” in Judah and an “urban polity” or “territorial state” in Israel (FINKELSTEIN 1999c, 44–47). Yet, he concluded that: “Israel, after all, was never a true national, ethnic state” in contrast to “the homogeneous, national states of the region (Judah and the Transjordanian entities)” (FINKELSTEIN 1999c, 48). If Judah reached the highlight of “national state”, it contradicts the idea that it never (or very late) reached “mature statehood”. How did an inferior entity, most of the time “immature as a state”, suddenly reach the high step of “national state” which the superior northern Israel never attained? Furthermore, how can the zenith of “full-blown statehood” be reached in a period when Judah entered the service of the ruthless Assyrian Empire? (FINKELSTEIN 2001a, 111; cf. 1999c, 48). A peculiar misunderstanding concerns peripheries. FINKELSTEIN wrote that a periphery can show “signs of statehood” (2001a, 106), and that:

“The Beer Sheba valley went through a significant change [in late 8th century], from a fringe area to a relatively densely settled and well-protected region of the Judahite state […] as long as the
Northern Kingdom prospered, Judah remained a marginal entity – a sort of client state – to its south” (FINKELSTEIN 2001a, 105).

He also spoke about “fringe landscape” in pre-eighth century Judah (FINKELSTEIN 2001a, 107). If an area was a fringe area, a fringe of what was it? There can be no periphery or fringe without a core or center. A state cannot create itself from an outside fringe, while, at the same time, have a void or a fringe at its core. On several occasions, FINKELSTEIN attributed “real statehood” to 7th century B.C.E. Judah, to the time of Manasseh (FINKELSTEIN 2001b, 328) or Josiah (FINKELSTEIN 2002c, 132.147). However, when he wanted to refute the idea that Judah of this period employed Greek mercenaries, he wrote:

“This is highly unlikely, as Judah was economically poor and politically (and probably militarily) dominated by Assyria” (FINKELSTEIN 2002c, 145; for Greek mercenaries see NIEMEIER 2002).

Can Judah rise into “full-blown statehood” with advanced administration and literacy, just to fall at the very same moment into such poverty that it cannot afford some Greek mercenaries? FINKELSTEIN wrote that Moab and Ammon had early, even Iron Age I, “advanced territorial-political” entities, but tried to separate northern Israel as a special creation. Israel was the only “fully developed state”, while “from the socio-political point of view, Moab, Ammon, Edom, and possibly also Judah, may be better characterized as ‘tribal states’” (FINKELSTEIN 1999c, 43; cf. 1999c, note 15). If one can put one foot near the bottom of the ladder (“tribe”) and the other on the top (“state”), yet remain whole, does it not show that these are not real steps that can be easily separated?

FINKELSTEIN changed his views about ancient Israel, but not his model. Pre-LC papers on the United Monarchy share the same model and the same conclusions about “dimorphic chiefdoms” for the Late Bronze and Iron Age I, followed by a “highland state” (FINKELSTEIN 1995b, 361; 1989, 46–48; 1993). Only, in pre-LC times, the state was the United Monarchy (FINKELSTEIN 1995b, 362; 1989, 62–64). Yet, at that period FINKELSTEIN was aware of limitations of the model:

“the standard classifications of human societies according to their sociopolitical status [referring to SERVICE 1962 and FRIED 1967] cannot be fully applied here, because they deal mainly with the sociological aspects of the emergence of complex societies rather than with their material culture” (FINKELSTEIN 1996a, 110). “The important stages for the study of the emergence of the Monarchy in Israel are the chiefdom and the state. The distinction between them in ancient societies is sometimes difficult […] especially between a chiefdom and an early state […]”. Because of the difficulties in distinguishing between the above mentioned socio-political systems, the few scholars who attempted to classify the first stages of the Monarchy (solely on the basis of the biblical evidence) could not have reached consensus” (FINKELSTEIN 1989, 47).

With the LC, limitations no longer exist. It should be stated that data from new surveys about the limited early Iron Age settlement in Judah was known before the LC. Back in 1993, FINKELSTEIN wrote:

“These data shed new light on certain aspects of the United Monarchy. The Judean hill country was still sparsely inhabited […]. In the 10th century, the sedentary population of the Judean hills was only ca. 3 percent of the total population of the country […]. David’s power (and Solomon after him) did not stem, therefore, from a solid sedentary population, but from the special composition of the population of Judah (which still had, at that time, a strong non-sedentary element), and from the personality of the monarch” (FINKELSTEIN 1993, 63, cf. 1989).
An explanation was found, within the same model, for what would be posited later in LC-studies as unfitting for a “stately” United Monarchy. Furthermore, the same data lead Ofer (2001, 27) to conclude that there was a United Monarchy, following the HC. In reality, data from surveys is limited, and hardly proves Finkelstein’s or Ofer’s claims.

1.4. An Outdated Model

The “neoevolutionary model” is more than 40 years old. It was not formulated by archaeologists, but adapted to archaeological use by pre-historians who almost always studied “pre-state” societies (Yoffee 1993, 71). The problems with it are acute. We will discuss here only the “chiefdom” and the “state” in this model, since the concept of “tribe” as a meaningful step in a linear ladder fell a long time ago (see, for example, Dever 1994, 214; cf. 1997, 246–247; Martin 1989, 115).

“Chiefdoms”

The concept of “chiefdoms” as the next step in a linear ladder became out of tune a decade ago, even before the LC was born. The “chiefdom” was projected into the past from modern ethnohistoric cases and defined in origin as kinship societies, as against “states” which are civil societies (Service 1962, 162–163; 1975, 15). Yet, anthropologists could not find a stable definition. “The essential criteria of the chiefdom have changed significantly over the years from the classical description of Service” (Yoffee 1993, 61). Scholars had to break the “chiefdom” into various sorts – such as complex and simple, or “group oriented” and “individualizing”. Renfrew’s group oriented chiefdoms (1974, 79) retain egalitarian features, but develop impressive public buildings. So much of the original defining characteristic of “chiefdoms” is no longer tenable, that the term became an empty shell (Yoffee 1993, 63). Paynter (1989, 387) concluded:

“the data from sequences of early state formation do not neatly fit neoevolutionary expectations”. Bawden (1989, 330) wrote: “Such obvious irreconcilable beliefs as to what a state (or, indeed, a chiefdom) is arise from the intellectual exercise inherent in classification theory [. . .]. It is time for us to reject typological theory in favor of a perspective that more closely conforms to observable evolutionary reality”.

Even those who continue to use “chiefdoms” acknowledge that one cannot use the old linear model anymore. Miller discussed early Israel, so his work is especially a good example:

“unfortunately, the few biblical scholars and Syro-Palestinian archaeologists who have read any anthropological literature on chiefdoms [. . .] have relied on the outdated anthropological theory of Elmar Service [. . .], Marshall Sahlins [. . .] and Raymond Firth [. . .] for their understanding of chiefdom as redistributive systems [. . .]; recent work has eliminated any remaining applicability of the redistributive model” (Miller 1998, 24). Miller follows Earle (1991) and Stieglitz, who separated complex and simple chiefdoms. Rothman wrote: “Few concepts in anthropological archaeology are more controversial than those of ‘chiefdom’ and ‘early state’; the neoevolutionist focus on delineating the formal structural characteristics of these societies [. . .] has beset virtually all evolutionary approaches for the last century [. . .]; their endeavour has constantly gotten bogged down in terminological debates about the specific variables chosen to define sociocultural change [. . .] Many of the criteria chosen as important for definition and for the resulting social organizational steps were abstracted from ethnographic cases [. . .] Recently, however, anthropologists have questioned the utility of traditional evolutionary typologies – most notably the commonly used categories of bands,
tribes, chiefdoms and states – as frameworks for the study of the origins and development of complex societies” (ROTHMAN 1994, 1–3). He added that contradictory definitions were given to the “chiefdom”: “For example, Steponaitis [...] defined some societies with three-tiered administrative hierarchies as chiefdoms, based on the differential access to goods and the diminished obligation for labour of the members of the leadership organization. Steponaitis’ classification contradicts that of Wright and Johnson, who define any society with three tier hierarchical levels in its central leadership as a state” (ROTHMAN 1994, 3).

ROTHMAN tried to keep “state” and “chiefdom” alive by distancing himself from the old model, using these terms as “flexible ranges of organizational variation rather than as tightly defined structural types” (1994, 4). I am skeptical, since how can archaeologists see things such as “diminished obligation to labour” in the archaeological record? Critics of the old model are harsh. In 1993, before the invention of the LC, YOFFEE wrote: “It may be churlish to remind an archaeological audience considering the utility of borrowing the concept of chiefdom into the archaeological record, and worrying about what the real essence of the ethnographic chiefdom is, that the subject of ‘chiefdoms’ is light-years away from anything that modern anthropologists study. This [...] reflects an agreement that the typological efforts to identify a chiefdom was and is useless” (YOFFEE 1993, 64). For Mesopotamia, he concluded: “If the notion of chiefdom is unhelpful in explaining the formation of the ‘civilizational’ boundary within which Mesopotamian city-states are embedded, it also fails to account for the kind of political struggle one observes within Mesopotamian city-states” (YOFFEE 1993, 66). “None of the supposed characteristics of ethnographic chiefdoms can ‘predict’ the form of Mesopotamian historic states [...] In fact, from just about any kind of chiefdom to a Mesopotamian state you cannot get” (YOFFEE 1993, 67).

YOFFEE formulated a “new social evolutionary” theory (1993, 69–74), but we need not review it here, since FINKELSTEIN did not mention it. YOFFEE adds:

> “the taxonomic labels of neoevolutionarism have falsely ranked the diversity of human societies [...] This labels have also been wrongly used by archaeologists who seek to ‘type’ a prehistoric society as a ‘state’ or a ‘chiefdom’ as if such a categorization might elevate their empirical research. [...] If chiefdoms exist in the ethnographic record, they do not predate the development of the state, but are alternate trajectories to it [...] Indeed, the model developed by anthropologists in the late 1950s and the 1960s, and employed by archaeologists ever since, now actively hinders modern research on state formation” (YOFFEE 1993, 72–73).

YOFFEE repeated his view in a conference in which FINKELSTEIN participated, published in 1995:

> “From this brief discussion of chiefdoms, it can be seen that in recontextualizing some categories which had been elevated to intellectual fetishes in social evolutionary theory, new insights can emerge. Certainly, we do not move forward by labeling Region X a ‘chiefdom’ at some point in time [...] Simple declarations that x-residues fit an abstract, ideal type and thus can earn a typological name should not be considered a model of advanced thinking by Region – X archaeologists newly ticketed on the good ship Anthropology” (YOFFEE 1995, 546). SHENNAN wrote: “much social archaeology has proven guilty. Thus, for example, Creamer and Haas [...] postulate the existence of tribes and chiefdoms, list a series of archaeological correlates, and then check them off for the two areas they are comparing [...] but the conclusion that one area represents a chiefdom form of organization and the other a tribe is a classic example of the fallacy of affirming the consequent, since the existence of tribes and chiefdoms is an unsubstantiated starting assumption which is not subject to testing, while the archaeological patterns could be accommodated to a range of different models. [...] The social evolution approach has nothing to offer in the analysis of such processes [...] albeit more sophisticated than those of Fried and Service, and largely oriented towards understanding the ‘rising of the state’ [...] it is based on the production of dubious synthetic ‘factoids’ [...] concerning social institutions [...]”
investigation” (Shennan 1993, 54–56). Joffe tried to avoid “to test the archaeological data against limiting models of socio-political evolution, which are then reflected back on the biblical texts” (2002, 452).

Today, the impracticability of this old model is such, that even studies by advocates of “chieftdoms” rarely discuss Fried and Service, being occupied with more recent theories (Schiffer 2000; Marcus 1998; Feinman 1998). For Hayden, “tribe” and “rank” are encumbered terms (1995, 16–19); while the starting point for Blanton (1998), is systems theory of the 1970s. The “chieftdom” as used by Finkelstein in LC papers is anything but modern archaeology.

“States”

After “chieftdom”, the “state” as a next step faces similar difficulties. Because relatively few archaeologists dealt with historical periods until recently, the “state” as a neoevolutionary step has not yet received so much criticism like the “tribe” and “chieftdom”. The definition of the state is notoriously difficult. Fried was aware of this, and saw the state largely as “an organization of the power of society […] it is the task of maintaining general order that stands at the heart of the development of the state” (1962, 227). He saw the state as a “stratified” society with certain institutions, that deal mainly with population control of citizens; rules and laws; treasuries or bursars; taxation; established sovereignty, and an ideology of legitimation (Fried 1962, 235–240). Service followed the definition of the state as a bureaucratic governance by legal force: “a true state, however undeveloped, is distinguishable from chieftdoms […] by the presence of that special form of control […] a body of persons legitimately constituted” (Service 1962, 163; cf. “civil law and formal government” as criteria in Service 1975, 14.303–304). Yet, he wrote that states retain many characteristics of chieftdoms, and it is difficult to separate the two, especially in archaeological cases. At times, Service used tautologies to convince that: “the simplest way is to define these societies in their own terms, chieftdoms are chieftdoms and states are states” (Service 1962, 165). Both Fried and Service took the state as granted and did not discuss it in length. For Sahlins “the state is a society in which there is an official authority, a set of offices of the society at large, conferring governance over the society at large” (1968, 6).

Based on twenty cases, Claessen and Skalník (1978, 17–19.586–589) defined “early states” with these characteristics:

1. A certain population (exact minimum unknown, perhaps 500).
2. A definite, distinct territory, though the outer borders are often loosely marked.
3. A political organization and central government, with one center, but usually without codified law, professional judges and police corps.
4. Being independent; the ruler has supreme military command and a bodyguard, and can use the population for military service; he keeps the territory from breaking into smaller regions.
5. A minimal two-tier social stratification, i.e., rulers and ruled.
6. A regular surplus of goods that sustains state organization, generally not through regular taxes.
7. A state ideology that gives legitimacy to the ruler, which is supported by a priesthood.

In 1981, they made no major modifications. They used the same definition of the state, based on a “civic society” idea, but discussed also secondary and pristine state formation (Claessen/Skalník 1981, 339–340.343–344.349.485–487).
Within early states, Claes sen and Skalnik defined three development phases: “Inchoative”, “Typical” and “Transitional” (1978, 22–23). They noticed that there are many problems in trying to separate these phases. Some of the criteria of their “inchoate state” were used as markers of “chiefdoms” by others (1978, 590–593 and note 3, p. 629–633). Many criteria are difficult to identify in archaeological cases. Being aware of the problems, they criticised Wright’s archaeological study of ancient Larsa in these words:

“the analysis clearly shows that Larsa has attained state level. It is not clear, however, whether this conclusion could have been reached without the evidence of written documents. Another problem is that the definition [of state] developed by Wright can be used for most multi nationals, trade unions, and even some universities” (Claessen/Skalnik 1981, 487).

Wright defined Larsa as a state, but claimed that its government appeared hundreds of years later, while they pointed that the state cannot exist without government. They concluded that ethnographical data would not be useful for archaeologists who study early states (Claessen/Skalnik 1981, 489).

Kohl (1987a) and Earle (1987) separated industrial/secondary states from primary ones; but Kohl wrote: “Archaeologists have become increasingly critical of neo-evolutionary formulations for the development of complex societies that stress internal, typically environmentally related factors” (1987a, 1). He called attention to the fact that cultures and states are open and dynamic and suggested a wider, world-system attitude (unfortunately, Kohl 1987b is not available to me). Bawden harshly criticized scholars of Peruvian archaeology who called everything from late prehistory and later a state, but the state itself is only a “general catchall description of complex society in its most highly evolved forms” (Bawden 1989, 330; incidentally, these scholars inspired Finkelstein 1996a, 110). In the Andean societies, wrote Bawden, there are “mixtures of characteristics that have been used to identify chiefdoms and states, ranked and stratified societies. [. . .] The types are all states of mind” (Bawden 1989, 331). In 1994, Feinman wrote:

“Over the last decades, significant revisions have occurred in archaeological thinking on the ancient state. Many would agree with Bawden [. . .] when he suggests that it is time to reject typological theory in favour of a perspective that more closely conforms to evolutionary reality” (1994, 225). Feinman agreed that “the field has moved away from prime mover, strictly taxonomic, and narrow neoevolutionary frameworks” (1994, 231; cf. Marcus/Feinman 1998, 14–17).

He argued that the “chiefdom” and “state” can be used flexibly as general categories, thus escaping the problems of definition. Perhaps Feinman can use terms flexibly, but not Finkelstein (1996a), who holds the noble opinion that nomenclature must reflect with clarity the “current state of the art of archaeology”. In 1998 Feinman acknowledged that there is no threshold that distinguishes “state” from “chiefdom”, regarding size and numbers of people (1998, 96–104.108). Many states are very small, with even below 2500 people. And, he wrote, “many of these small states [. . .] overlap in scale and political complexity with ‘complex chiefdoms’. [. . .] Yet it seems somewhat of a conceptual stretch to describe many of the aforementioned polities as ‘chiefdoms’” (Feinman 1998, 104), because in other features they are similar to states. Feinman listed the “plethora of names” used for such entities, and admitted that this is a “taxonomic muddle” (1998, 103). He also realized that evolution is not linear, and “states” are dynamic and extremely variable in size and structure (Feinman 1998, 112–114,131–132). Feinman and most of those who continue to advocate “chiefdoms” and “states” objected sharply to the use of the term “city states”. Yet, what they rightly pointed
about “city-states” is no less true about the former two terms (e. g., MARCUS 1998, 92–93; MARCUS/FEINMAN 1998, 8–10). Severe criticism of the old model was made by UPHAM, who concluded:

“because cultural systems are too inclusive to be used as analytical units, taxonomic approaches that seek to identify ‘stages’ of cultural evolution are scientifically inadequate to explore changes in the complexity of cultural systems. Consequently, the use of terms like bands, tribe, chiefdom and state in study of social and political evolution are counterproductive since they define constellations of variables that may or may not covary and certainly cannot be measured” (UPHAM 1990, 89–90). In the same vein, YOFFEE wrote: “Unpacking is also just what is needed in considering the terms ‘city’, ‘city-state’ and ‘urbanism’ [. . .] all the earliest states [. . .] are not states at all, they are city states [. . .] all human social and cultural systems are complex. It seems to me that ‘complexification’ in archaeology (as in ‘complex hunters-gatherers’ or ‘complex chiefdoms’) [. . .] has resulted from a confusion in which the complexities of recovery and analysis in modern scientific archaeology have become a shorthand for the complexity of social organization that is being studied [. . .]; let us eschew the inevitably value-laden oppositions of ‘simple’ vs ‘complex’ [. . .] categories like chiefdom, city-state and ruralism must be unpacked and historicized” (YOFFEE 1995, 456–457).

The whole model and FINKELSTEIN’s various “states” are loaded with “civic ideas”, including “citizens”, “civil”, “government”, “legal systems”, etc. Such concepts are anachronistic for ancient periods. We presently live in a world of states, our conceptions about them seem part of the natural order of things, but as SCHATZER-LICHTENBERGER rightly stressed, “the state, and hence our preconceived notion of it, is a manifestation of modern times” (1996, 83). One must separate “early states” from “modern states”, like CLAESSEN and SKALNÍK (also HERION 1986, 79–83; MARCUS/FEINMAN 1998, 4). SMITH traced nations back to ethnic communities, his “ethnie”. An “ethnie” is

“a named human population with myths of common ancestry, shared historical memories, one or two more elements of common culture [. . .] a link with a homeland and a sense of solidarity among at least some of its members” (HUTCHINSON/SMITH 1996, 6–7; cf. SMITH 1971, 162; 1981, 66; 1991, 20–23; 2000).

SMITH observed that the “civic” conception – citizenship, legality and codified laws, government, fixed territories, etc. – is modern (SMITH 1971, 176.190–191; 1981, 84–85; 1991, 11).

The first to use SMITH’s “ethnie” for ancient Israel is JOFFE (2002).

In addition, the model used by FINKELSTEIN took for granted “biological” ideas of linear evolution and growing complexity. Doubts about these ideas have recently surfaced. MASCHNER and PATTON wrote in 1996 that:

“Anthropologists, and archaeologists in particular, have long used concepts derived from the biological sciences. Terms like evolution, adaptation, population pressure and carrying capacity are commonplace [. . .] perhaps no better example of this can be found than in Anthropological theories concerning the development of hereditary social inequality where most archaeologists writing on the origin of chiefdoms and states have used one or more of these terms borrowed from biology [. . .]. The recent Darwinian movement within anthropology has brought into question many of the assumptions that underlie such an approach” (MASCHNER/PATTON 1996, 89). They concluded: “Archaeologists must ask themselves why they are studying hereditary social inequality. If it is to better understand the rise of states and chiefdoms, perhaps they should look elsewhere [. . .] inheritable social inequality is a basic quality of all political groups and cannot be used as a unique defining characteristic of stratified societies” (MASCHNER/PATTON 1996, 102; cf. O’BRIEN/LYMAN 1995; DRENNEN 2000). Similarly, BLANTON in 1998 defined the state not “primarily as a hierarchically structured control mechanism. Rather, my behavioral approach views the state, wherever it is found, as the major social arena within which the competition for power is played out in society” (1998, 140).
He thought that centralization was too much stressed, and that there can be egalitarian behavior in state societies (BLANTON 1998, 149–152). BLANTON developed a concept of “corporate political economy” for archaic states, but it needs not be reviewed here, as the LC is not aware of it.

MARCUS and FEINMAN (1998, 5) answered the criticism by writing that blame must be put not on the inventors of the model, but on archaeologists who abuse it. This is no real consolation. They agreed that states are dynamic, evolution is not linear, and “archaic states were a lot more fragile and diverse than the archaeological literature would lead one to believe” (MARCUS/FEINMAN 1998, 10–11). They wrote that historical data is crucial to define ancient states, whereas archaeological tools are badly needed – but not yet developed (MARCUS/FEINMAN 1998, 13). FLANNARY tried to formulate such tools, but knew that definition for the state “must remain in the realm of anthropology and political science”. He could only give clues for archaeological identification of “archaic states”. These clues are not simple. For example,

“because archaic states were stratified societies with kings and queens, they often featured palaces”, but “in many regions there was a time lag between the first evidence of statehood and the first unmistakable palace” (FLANNARY 1998, 54).

A few cases can demonstrate the problems. FLANNARY suggested that Olmec period San Lorenzo “will prove to be the paramount center of a maximal chiefdom” (1998, 56–57), but was not a state. Perhaps so, but it is based only on negative evidence. WEBSTER wrote in 1998 about Polynesia, the ‘cradle’ of the concept of “chiefdom”, in these words:

“Behind the widely shared facade of ranking […] was an enormous range of sociopolitical complexity” (WEBSTER 1998, 312). Some Polynesian societies were defined as states, but other scholars, for the same societies, “retain the term ‘chiefdom’ while describing sociopolitical patterns that are in many respect ‘statelike’ […] Such confusion of terminology itself is revealing, since it signals the existence of complex sociological forms for which our comparative evolutionary terminology is inadequate. No issue discussed during the seminar generated more heated and diverse opinion than the utility of such labels and their associated evolutionary models” (WEBSTER 1998, 312, though he continues to work with a similar conception).

The case of the Indus civilization is revealing. It encompasses a million square kilometres with stratification, sophisticated technology and architecture, yet lacks temples, palaces and large statuary. Scholars debate if it was a state or a complex of chiefdoms. In 1998, POSSEHL reviewed the various views and wrote:

“There is an increasingly poor fit between the ‘facts’ as we know them and traditional evolutionary consensus […] Not much of the Harappan civilization appears to fit the state [model] as developed by Claessen and Skalnik, or Service. Nor do the archaeological markers of the archaic state as outlined by Flannary […] seem to fit the mature Harappan” (POSSEHL 1998, 279–280). POSSEHL defined the Harappan culture as something in between, a “nonstate”. He noted that one definition of CLAESSEN and SKALNIK for the state was “appropriate for tribes and chiefdoms as it is for the state; it is a good example of some of the fuzzy thinking that emerges from many discussions of the archaic state” (POSSEHL 1998, 286). He concluded: “whether or not we call the Harappan civilization a state is of less importance than the central issue of recognizing the diversity of organization and form among the sociological systems that are associated with this term […] using models from other places has a dismal record of failure […] one may wish to set the Harappan civilization apart, conceptually and terminologically […] It is at least an act of intellectual renewal and a candid admission that the ‘state’ paradigm does not work” (POSSEHL 1998, 290).
CLAESSEN, one of the developers of the old model, wrote in 2000 that SERVICE, SAHLINS and FRIED were only pioneers who made first attempts. CLAESSEN admitted that scholars choose which characteristics to use in the FRIED/SERVICE model, a process that involves subjectivity. SERVICE’s model was very popular because it was easy to use. CLAESSEN realized that archaeologists abused the model:

“Some archaeologists even showed the inclination to think, that when one or more of the characteristics attributed to a particular culture by Service was excavated, it was possible without any further demur to ascribe all the other characteristics to the people who had once lived there” (CLAESSEN 2000, 52).

Yet, societies are constantly changing, and definitions for each “step” are problematic. CLAESSEN himself once placed the Inca Empire together with small Tahitian principalities in the rubric of “states”.

“This example shows that the placement of societies in such diagrams is founded on the (pre)judices – the choices – of the scholar […] large differences are to be found between the units on one step […] a number of characteristics which are usually ascribed at the chiefdom level of development, either do not – or only after a long time – disappear after the evolution of the state […] the classification of societies within fairly narrowly circumscribed frameworks is strewn with difficulties. It even gets more problematic when we try to see the series compiled by Service as evolutionary process” (CLAESSEN 2000, 54). As for FRIED, CLAESSEN concluded: “any attempt to place the types of societies he developed in the social reality has proved to be a rather daunting task […] Khazanov […] exposes a clear cut division between the model and the reality. Not to mention the fact that some societies can be slotted in under more than one category” (CLAESSEN 2000, 55). Models are only arbitrary tools (CLAESSEN 2000, 161). His and SKALNIK’s 1978 book was an initial attempt. “In all the [21] examples the final result was an early state. This was not surprising in view of the fact that the point of departure for our research was early states”.

Many societies pass very different lines of development, not discussed there (CLAESSEN 2000, 154). After reviewing more recent models, CLAESSEN suggested a “complex interaction model” (CIM), not mentioned in LC papers.

Many scholars used the old model for ancient Israel, but reached contradictory results. FLANAGAN equated Saul with the “chiefdom”, David with the “early state”, and Solomon with the “fully developed state” (1981, 158; cf. 1988, 297–304). VICANDER-EDELMAN (1986, 9.31) saw Saul as “state builder”. HAUSER (1986) identified Saul’s reign as “chiefdom” or as a “state”. FRICK at first defined Saul’s and early David’s kingdoms as “chiefdoms” (1985, 69), later he identified Saul’s kingdom as an “inchoative state”, David’s later kingdom as “typical state”, and Solomon with the “transitional state” (FRICK 1986, 21; cf. BELLEFONTAINE 1987). SCHAFER-LICHTENBERGER used CLAESSEN and SKALNIK’s model to equate Saul with an “inchoative state” (1996, 96–99; cf. WHITELAM 1989). NIEMANN identified David as “chief” and Solomon as “typical oriental (small) ruler” (1997, 290), but he used “complex chiefdoms” (NIEMANN 2000, 62 note 1). Following THOMPSON (1992), GELINAS (1995), used the “chiefdom-state” model but replaced “state” with “kingdom”. Her conclusions read like FINKELSTEIN:

“the transition from a chiefdom to a monarchy with a strong central government […] does not find expression in the material record at Jerusalem” (GELINAS 1995, 228–229). Compare THOMPSON: “During the first part of the Iron II, Jerusalem was a small provincial town at best […] while these are negative references […] one may be excused for doubting that Jerusalem was a major power in the region at this early date […] Jerusalem became the capital of a regional state in the course of the
seventh century” (1992, 409, 411). Yet, GELINAS was aware that “a critical issue yet to be addressed is not only what would constitute an acceptable definition of a ‘state’ in ancient Syria-Palestine, but what are the discernible differences in the political structures of a ‘nation-state’, a ‘city-state’, a ‘town’ or a ‘village’ (1995, 236–237).

LEMCHE understood problems of models (1997, 316–333 note 57), and tried to avoid the old model by using “patronage societies” in analogy to Europe of the Middle Ages (LEMCHE 1996, 109–110). LEMCHE even refuted the very logic of doing sociology with the United Monarchy:

“To present a sociological analysis of the kingdom of Israel in the time of Solomon would in this way be impossible, as there was no kingdom to study” (1997, 312). Yet, he draws on archaeologists like JAMIESON-DRAKE and FINKELSTEIN when writing: “Recent scholarship has called into jeopardy the very existence of the united Israelite kingdom in the 10th century BCE. The archaeological remains from Judah seem to pre-exclude that a major kingdom could have been established here already in the 10th century, if at all in the Iron Age of Palestine” (LEMCHE 1997, 312).

The problem is that state interchanges with kingdom here, but these archaeologists proved nothing about kingdoms. However, LEMCHE is a world apart from FINKELSTEIN, and his views are beyond the scope of the present study.

SCHAFER-LICHTENBERGER stressed that it is dangerous to judge the lack of “state” by negative archaeological evidence, for, based upon such evidence “the states of Athens or Sparta of the seventh century BCE or the Carolingian empire of the eighth century CE would have their statehood denied” (SCHAFER-LICHTENBERGER 1996, 81). DEVER too was aware of problems with chiefdoms (1997, 226 note 17) and with defining states, but still followed the old model (DEVER 1997, 247–250). He wrote that archaeological data, checked against SERVICE’s definitions, corroborates the identification of the United Monarchy as a state. He would just replace chiefdom with “tribal state” (2001, 128), and he even praised FINKELSTEIN, for his “rather thorough acquaintance” with the theoretical literature on state-formation (DEVER 2001, 126).

MILLER criticized users of the old model (1998, 24). He used the concept of “complex chiefdom” and, not surprisingly, found it in Israel of the 12th – 11th centuries B.C.E. MASTER (2001) saw that scholars employing this model for ancient Israel used a limited number of categories based on SERVICE, FRIED, CLAESSEN and SKALNÍK:

“In their applications of the common definition of a state, each of these presentations relied on a set of trait lists to decide if tenth century society reached a developmental plateau […] implying a major systemic transition from kin-based to territorial based authority” (MASTER 2001, 125). However, more recent examples from the Near East show that states can keep kin-based structures, while tribes can form even large empires: “the distinction between kin-based tribe and a territorially based state is no more helpful in understanding 10th century Palestine than in categorizing modern Middle eastern societies” (MASTER 2001, 128; KHOURI/KOSTNER 1991; for ‘Nomadic states’ cf. KHAZANOV 1994, 228–231). MASTER, however, returned to WEBER’s concept of patrimonial state (cf. SCHAFER-LICHTENBERGER 1996, 84–85). He concluded: “archaeologists and biblical scholars have been working under a theoretical consensus inappropriate for the society at hand. Neither ancient Israel nor any other premodern Middle eastern kingdom or empire meets the criterion of territorially based statehood […] the jump to modern perceptions of beaurocratic statehood are misguided” (MASTER 2001, 130).

Before the LC, FINKELSTEIN claimed that Saul’s kingdom was a “chiefdom” or “national state”, while David created the “real territorial state” (1989, 48.62–63). He was certain that adding archaeology to the old model is the solution:
“some points taken from modern sociological and political theory seem to be out of context, because
the most important realistic historical reconstruction [sic!] – the archaeological data – were not
available” (FINKELSTEIN 1989, 53). Yet, he wrote: “I wish to clarify my own view, based on the
biblical sources only, as archaeology does not permit socio-political distinctions within the short
timespan of the monarchy’s formative period” (FINKELSTEIN 1989, 48).

In LC papers, FINKELSTEIN wrote that Judah was a “chiefdom” during the 10th century B.C.E.,
“real” state only later, or never. How to explain this change of view and the contradictory
results between all these various scholars mentioned above? All of them used practically the
same model and worked on the same case – ancient Israel. FINKELSTEIN (recently) explicitly
accepted Samuel I as a reliable source: “Regarding the sources, it is widely accepted that 1
Sam contains pre-Deuteronomistic material” (FINKELSTEIN 2002a, 128). So the contradiction
does not rise just from the use of different sources, allegedly a-historical Bible versus reliable
archaeological finds. Rather, it results from the flaws of the theoretical model employed by all
these scholars – it allowed each to find what he was looking for.

HERION wrote about using sociological theories for Biblical studies, that the “type” or
“model” is not real, but a construction of all common features, temporarily ignoring existing
differences and inconsistencies (HERION 1986, 83–84). Models

“are to be used to analyze existing data, not to serve as substitutions in the absence of data. They do
not conclude a study or provide definite answer, but rather they (a) summarize current thought or (b)
help to raise new questions” (HERION 1986, 84). “Perhaps every social-science study of ancient
Israel should begin not simply with a description of a particular model or theory but also with a
critical evaluation of it, especially noting how subsequent social-science study has qualified, modi-
ﬁed or revised that model or theory” (HERION 1986, 103). In light of this, how to understand
FINKELSTEIN’s words about following “mainstream sociopolitical research”? (FINKELSTEIN 1999c,
39; cf. 1999a, 134). CLAESSEN and SKALNÍK were aware that “there does not exist any deﬁnition of
the state that is accepted by the entire community of scholars [. . .;] almost every scholar evolves
his/her own deﬁnition” (1978, 3). Compare NORTH: “the long path of historical research is strewn
with the bones of theories of the state” (1986, 248).

What model does FINKELSTEIN follow? CLAESSEN and SKALNÍK separated early states from
full-blown or mature states (1978, 22–23). They do not have “full-blown/mature early states”.
The most developed form in their model is the “transitional early state”, which “already
incorporates the prerequisites for the development of the mature state” (CLAESSEN/SKALNÍK
1978, 23, cf. 634; cf. 1981, 492.499). Clearly, in their view, the “mature state” – the state in
the modern sense – appeared later than “transitional early states”. FINKELSTEIN cited them
brieﬂy in a pre-LC paper (1989, 47). However, in LC papers he did not mention inchoative,
typical, or transitional states, but two other, vague phases: “early/undeveloped/client states”
versus “mature/full-blown/real states”. This is a misunderstanding of the model itself, which
projects the “modern state” (CLAESSEN and SKALNÍK’s “mature/full-blown state”), into the
past. It seems that FINKELSTEIN referred to CLAESSEN and SKALNÍK, but did not internalize
their model.

FINKELSTEIN’s formulation of an Israel that shifts from “chiefdom” to “early/client/
regional state” and to “full-blown/real/territorial/mature state” is out of tune with CLAESSEN
and SKALNÍK (1978; 1981) and following scholars of state-formation. There was no “mature
state” or “full-blown state” in Iron Age Palestine/Ancient Near East. We remain with a
possible shift between two (unproven) forms, the “chiefdom” and the “early state”. Yet,
precisely about this shift, FINKELSTEIN declared that it is diﬃcult to separate the “chiefdom”
from the “early state” (1996b, 185; 2001a, 110). A serious evaluation of the United Monarchy
cannot be based upon labeling it “not a full-blown state”, since this label is meaningless for ancient periods. I do not claim that it is impossible to use “chiefdom” and “state” in studies of ancient societies. In cases lacking historical sources, scholars must conceptualize, and do so in their own terms. But they must define their terms with clarity, and check if an old theoretical model is still applicable.

1.5. Kingdoms

Rather than turn to Weber (like Master 2001), conceptualization of ancient Israel and Judah in Iron Age Palestine would do better without imaginary “chiefdoms” and “states”. There was one form of society which dominated the ancient Near East, certainly during the Iron Age II period: the kingdom. Finkelstein was so occupied with “chiefdoms” and “states”, that he attributed them to the Old Testament: “The Bible draws a picture of two sister states, Israel and Judah” (Finkelstein 1999c, 48). The Bible does not have “states”, it has kingdoms and kings. Even Finkelstein used names of kings for his entities – “Saulides”, “Davidides”, “Omrides” – though he preferred to present them as ephemeral beings: “To sum up this point, in the 10th and early 9th centuries the southern hill country was still characterized by what I would describe as ‘Amarna-like’ demographic and political conditions. A ‘king’ ruled over large, relatively empty, dimorphic countryside” (Finkelstein 2001b, 331). We should pay attention to the many ancient sources, of which some at least are reliable, that put so much stress on Kings and Kingdoms. Na’aman is absolutely right in writing:

“the importance of studying an ancient civilization in its own terms and within its own system of values has been commonly accepted by scholars since Landsberger’s seminal study [. . .]. In contemporaneous concepts, the territorial highland entities were regarded as kingdoms and their rulers as dynastic kings” (Na’aman 1996, 21, cf. 25). Compare Yoffee: “Neo-evolutionist studies, further, were seldom linked to the crucial evidence produced by ancient historical sources” (Yoffee 1993, 74). “To investigate evolutionary phenomena in prehistoric Mesopotamia, it seems to me, it is far more useful to see what happened in historic times than to rely on an abstracted (and disputed) ethnographic stage of chiefdom or to study some distant chiefdom that never would become a state” (Yoffee 1993, 67; cf. Lemche 1997, 334–335).

Instead of the limiting neo-evolutionary model, we can use the rich historical sources about the Levant in the Iron Age period. Even sources that reveal ideologies are important (e.g., Na’aman 1996; Liverani 1990). Then, we can compare archaeological remains from various kingdoms known from reliable sources, and check for correlation. And if we find correlation, only then we can start to pose the question of archaeological markers for existence of kingdoms (and which type of kingdom). This might be in vain, since so much of the debate hangs upon Jerusalem (in view of our current knowledge of 10th century B.C.E. Jerusalem). But there are no shortcuts in the shape of instant slot machines of “chiefdoms” and “states”. Finkelstein has by now accepted so much of the disputed texts, that he probably cannot refute the existence of the United Monarchy as a kingdom. He admitted David as a king and founder of a dynasty, with a capital, a palace, a shrine, and an elite in Jerusalem (Finkelstein 2001b, 331; 2001a, 107; Finkelstein/Silberman 2002, 63).

The picture regarding the theoretical basis of the LC is grim. Finkelstein took an old, outdated model and did not check if it is still applicable. He claimed that it reflects mainstream view, when there are many different, conflicting, views. He replaced basic definitions in the model with ill-defined terms, which do not fit it. The linear sequence of “chiefdom –
early state – mature state” for ancient Israel/Judah, presented as modern science, is based only on a misinterpretation of an outdated theory. It proves nothing about ancient Israel/Judah. This sequence is of paramount importance to the LC, and with its demise, it is accompanied by much of FINKELSTEIN’s so-called “archaeology of the United Monarchy”.

2. Dimorphism, Ethnicity and Nationality in LC Papers

FINKELSTEIN often used the term “dimorphic”, first coined by ROWTON (1973), for Jerusalem of Abdi-Ḥepa and for David, who “continued to rule over the dimorphic southern highlands” (FINKELSTEIN 1998b, 173 note 6; 2001a, 107–108; cf. 20001b, 331; 2002a, 111–112). It seems that the use of this term is intended to enhance the definition of the United Monarchy as a “non-state”. Yet, as BAR YOSEF and KHAZANOV stressed, dimorphism relates to an economic adaptation (1992, 2). Societies are flexible, they may have large or small segments of “enclosed nomads” or “pastoralists”; which may turn into urbanists or cultivators during a very short time, without a shift in the level of society as a whole. Existence of some pastoralists does not mean that all the society is such (BAR YOSEF/KHAZANOV 1992, 2; KHAZANOV 1994, 15.198; MARX 1992, 259). Dimorphism, then, does not differentiate “chiefdoms” from “states”.

SKJEGGESTAD (1992) criticized FINKELSTEIN for attributing simplistic ethnic labels to Iron Age I settlers. Since then, FINKELSTEIN often wrote against attribution of ethnic labels by others.

“This brings me to the question of ‘pots and people’, that is, to the assignment of ethnic labels to certain pottery types. Ethnographic studies have shown that such a straightforward connection should in most cases be rejected” (FINKELSTEIN 1999a, 220; cf. 1998a, 142). “Finally we reach the question of the ethnic identity of the inhabitants [. . .]. Removing the obstacle of a simplistic reading of the biblical text, there is no reason for hesitation: The material culture of Horbat Rosh Zayit is Phoenician” (FINKELSTEIN 2002b, 126).

However, what is exactly Phoenician culture, and is material culture equal with ethnic identity? An example about ethnic labels concerns Hazor. FINKELSTEIN dismissed BEN-AMI’s suggestion that Hazor XIII is Canaanite and Hazor XII – XI is Israelite:

“In short, this is certainly one case where the old ‘pots and people’ idea should be rejected” (FINKELSTEIN 2002b, 120). However, FINKELSTEIN identified Hazor levels VIII – VII as Aramaean: “This city [VIII – VII] was affiliated by YADIN with the Omrides, while I would place it in the second half of the 9th century and associate it with the growing power of Aram Damascus under Hazael” (FINKELSTEIN 2000b, 118). This labeling was made despite an earlier warning by FINKELSTEIN: “turning to the archaeological anchors for the early-Iron II, I would start by emphasizing that some of them, such as the attribution of destruction levels to Aramaean campaigns, are no more than vague archaeological assumptions and there is no need to deal with them here” (FINKELSTEIN 1996b, 180).

What is the basis for FINKELSTEIN’s Aramaean labeling of Hazor? There are two very thin arguments. One is FINKELSTEIN’s reconstruction of the Hazor citadel as a bīt hilānī, which, even if true, does not prove anything (see more below). The second argument concerned four inscriptions from Hazor stratum VIII, of which he identified two as Phoenician inscriptions and one as an Aramaean inscription; the fourth inscription could not be deciphered. Yet, if these inscriptions have any value, presumably Hazor should be called Phoenician, since two
inscriptions carry more weight than one. It is well known that at this early period, the script did not yet bifurcate into the local later scripts of Phoenician, Aramaean, Hebrew, etc. Hence, these inscriptions can be considered to be Hebrew, Phoenician or Aramaean just as well.

FINKELESTEIN claimed that his identification of such inscriptions was based on language, not on script (1999b, 67 note 19, cf. FINKELESTEIN/SILBERMAN 2002, 68). If we examine the inscriptions from Hazor stratum VIII, three of them contain three letters, the fourth inscription contains five or six letters (YADIN et al. 1960, 70–71). None of them preserves even a single whole word. One cannot establish languages (more dialects than separate languages) from such inscriptions. Lacking evidence, philologists would tend to define such inscriptions by using the more general term – Phoenician – but this does not imply much about the ethnicity of Hazor. The only evidence comes from the script, and it relates to one northern sign in one inscription (no. 3), a fact which was discovered by YADIN et al. (1960, 71). FINKELESTEIN admitted: “The material culture of Stratum VIII cannot disclose the identity of the rulers and inhabitants of Hazor at that time” (FINKELESTEIN 1999b, 61; cf. BEN-TOR 2000, 11–12). Should calling Hazor Aramaean on such thin basis be praised, but calling it Israelite be mocked?

In another case, FINKELESTEIN explained lack of Philistine monochrome pottery at sites such as Lachish on the grounds of chronology, and objected the idea that this lack can be related with ethnicity, namely, that the population at Lachish did not import or use such pottery:

“One could entertain such an idea if the Monochrome vessels were restricted to cult, culinary or burial practices, all of which may, in certain circumstances, represent religious convictions, ideology or deep-rooted traditions of ethnic groups” (FINKELESTEIN 1995a, 220; cf. 1998a, 142; 1996b, 180; 1999b, 65; 2000a, 241; but see BUNIMOVITZ/FAUST 2001; NA’AMAN 2000a, 2–3).

He gave a similar explanation to Hirbet el-MudeÅyine, in order to claim that this is not an 8th century B.C.E. Moabite site, but a 9th century B.C.E. site related to the Omride kingdom (FINKELESTEIN 1999c, 49 note 14; 2000b, 128; this is unconvincing, see DAVIAU 1997; DAVIAU/STEINER 2000). FINKELESTEIN’s list of “cult, culinary or burial practices” reflects a misunderstanding of how ethnicity is reflected in material culture. I follow GELLNER (1983), ANDERSON (1983), and especially SMITH (1971, 186–191; 1986; 1991, 14.20–22.43–51; 1992). Ethnic communities do share some cultural elements, and in the past, language, religion, and material culture were seen as objective ethnic markers. However, SMITH stressed that it is the feeling attached to a particular object that set it apart as an ethnic marker (1991, 23). There are no clear lists of material traits that define ethnicity, since each community may develop its own specific features, and these change with time (SMITH 1991, 23–25). Ethnic markers can be overt (flags, hymns), or embedded within culture, when the group itself is not aware to them (such as certain dietary habits, style of decoration, the way one moves a hand for greeting). Since BARTH (1969), scholars have tried to determine archaeological correlates for ethnicity, but there is no fixed rule. For example, KAMP and YOFFEE were not certain “exactly what types of behaviour are most indicative of ethnic identities” (1980, 96–97). EMBERLING wrote that

“recent anthropological work on ethnicity suggests that differences in almost any crucial feature can distinguish one ethnic group from another” (1997, 305.310; cf. JONES 1997; HALL 1997; JOFFE 1999; 2001; KLETTER forthc.).
If any type of material object can be used as ethnic marker, the above mentioned lists of “cult, foodways or burial practices” are meaningless. Finkelstein wrote against A. Mazar, who allegedly

“adheres to the orthodox biblical ideology of the singularity of Israel, whilst not appreciating that this concept emerged in the troubled later days of the Judahite state/or during the constraints of the post-exilic period” (Finkelstein 1998b, 172).

The consequences of this accusation are grave. There is no ethnic community without some sort of singularity, since ethnicity is based on defining “us” from the “others”. The “us” must feel some uniqueness, be singular in something, in order to feel separate from all the “others”. Israel had a profound sense of distinctiveness, found in all types of biblical sources, certainly from the 8th century B.C.E. at the latest (see the brilliant study by Machinist 1991; also 1994; Sparks 1998). If, as Finkelstein claimed, there is no Israelite singularity before the 7th century B.C.E., that can be no Israelite ethnic community (and he used Israelite to denote here both Israel and Judah). Yet, he accepted Israel and Judah as ethnic communities in the 9th – 8th centuries B.C.E. (e.g., Finkelstein 1996a, 120; 2000b, 132).

Finkelstein (1996a, 120.122 –123; 1999c, 48; 2000b, 132) often used terms such as “national state”, “proto-national state” “national-territorial states” and “nation-states”. He wrote:

“Israel, after all, was never a true national, ethnic state” while Judah and the Transjordanian kingdoms were, in contrast, “homogeneous, national states” (Finkelstein 1999c, 48). Compare “national-homogeneous states” (Finkelstein 1999a, 138); “a cohesive territorial, ethnic, national entity” (Finkelstein 1999c, 48). “This [9th century B.C.E.] was therefore the moment when territorial and national boundaries had to be defined” (Finkelstein 2000b, 132).

Most scholars today agree that the nation is a modern creation of roughly the 18th century C.E. In 1983, Gellner stated:

“Nations as a natural. God given way of classifying men […] are a myth. Nations are not inscribed into the nature of things […] it is nationalism which engenders nations, and not the other way round” (1983, 55). Smith wrote in 1971: “In dealing with the Assyrians, Medes, Hittites, Egyptians and Phillistines, we are confronted not by ‘nations’ in any sense of the term, but by the simpler and commoner formation of the ‘ethnie’ […] This ancient pattern lasts right up to the Revolution in France” (Smith 1971, 190 –191). Grosby wrote: “for more than two hundred years now […] scholars of the Old Testament have referred to ancient Israel as a nation. Various positions have been held […] as to when ancient Israel became a nation […] in contrast to this usage among scholars of the Old Testament, it appears that in the period of the last twenty five years the majority within the discipline of the social sciences believe that nationality is exclusively a modern phenomenon” (Grosby 2002, 13).

Grosby (2002, 204 –206) objected this reasoning and believed in some primordial features, such as territoriality; but he did not go back as far as “ethnically homogeneous nation-states”. Finkelstein shows no awareness to these twenty-five years of research on nationalism and nationality.
3. Remarks on Chronological and Stratigraphical Arguments in LC Papers

3.1. Dating by Sherds

On the one hand, FINKELSTEIN wrote that sherds cannot be used for dating, because they are small and often intrusive:

“In a multiple period site such as Ta’anach, the use of sherds for dating is extremely dangerous, as older stray sherds may find their way into later assemblages. At Megiddo, for instance, a large number of earlier sherds, including Chalcolithic and EBI (!), are found in almost every Iron II deposit. They found their way through bricks, fills, make-up for floors and roofs, etc.” (1998c, 211). He also wrote in 2002 that: “I tried to concentrate on [whole] vessels, as sherds can surface from earlier occupation layers (FINKELSTEIN 2002a, 118). On the other hand, he does use sherds not only to determine end of strata, but their beginnings as well: “In the final report of the excavation [. . .] I argued that the collection of sherds and few vessels uncovered at Kh. Ed-Dawwara included both Iron I and early Iron II types. Since the site was abandoned by its inhabitants, and as its features only one occupational phase, the sherds represent an accumulation during the entire life-span of the settlement, the earlier ones dating its foundation and the later ones indicating the date of its abandonment” (FINKELSTEIN 1996b, 182).

This is a dangerous conception. It may be used with great care for one period sites, like Hirbet ed-Dawwara, if there is no other means. It also must be used to indicate a rough estimation, not exact dating. The reason is that an assemblage does not necessarily reflect the date of the establishment of a site. This concept is certainly not valid for dense, multi-layered sites. This is because a stratum does not furnish an accumulation of its entire life span. Assemblages from strata are “late biased” – this well-known archaeological rule states that most finds from a stratum will reflect its later occupation. This rule applies to abandoned sites as well as to sites destroyed by force. It applies to sherds as well as to whole vessels. One cannot be certain that there will be any remains from the early timespan of a stratum. As for small sherds, these may be intrusive, and in both ways, not only upward. The exception is tombs, where an assemblage might represent an entire life span (cf. MAZAR 1997, 163; BUNIMOVITZ/LEDERMAN 2001, 143). Yet, the whole LC debate was about strata, not about tombs. When FINKELSTEIN reviewed DEVER’S excavations at Gezer, this rule did stand:

“dating at Gezer was done according to sherds. This may be sufficient in the case of a small, one-period settlement. But in the case of a large, multi-layered mound such as Gezer, one must expect older sherd to surface in almost every stratum [. . .]. At Megiddo, for instance, Early Bronze I sherds [. . .] appear in almost every Iron II locus. Naturally, they are easily identified and put aside, but what about early Iron II sherds in late Iron II strata? These are not necessarily easy to distinguish” (FINKELSTEIN 2002d, 269).

If so, how can FINKELSTEIN himself easily distinguish early and late sherds within the same Iron Age II stratum in a densely occupied site? This is just what he claimed: “Sherds can (and should) be used to determine the earliest possible date of strata” (FINKELSTEIN 1998c, 211).

“The early pottery types found in it [Megiddo VI] should be considered as leftovers from the beginning of occupation of this settlement” (FINKELSTEIN 2002a, 120).

How can one know that these sherds are early, from the beginning of stratum VI, and not intrusive sherds from stratum VII – or V? The whole debate concerns a short period of 50 – 100 years, and many other scholars have stressed the similarity of pottery assemblages throughout the 10th – 9th centuries B.C.E. (BEN-TOR/BEN-AMIT 1998, 30; BEN-TOR 2001, 302–303; MAZAR/CAMP 2000, 50; MAZAR 2002, 274.277).
Chronology and United Monarchy. A Methodological Review

Nobody is above the basic rules of archaeology, and they also explain why Jameson-Drake’s conclusions are not necessarily valid (see above, part 1.1). Rate of accumulation or deposition of archaeological finds is not a fixed constant, which is directly related to the level of wealth or poverty of a certain society. It is “late biased” for all finds from strata. Furthermore, it is related to existence of destruction levels. General or large-scale destruction – for example, Sennacherib’s campaign against Judah – is reflected by many finds. Peaceful periods are poorly represented in archaeology. This may be one reason to explain why, in Judah, the 10th – 9th centuries B.C.E. seem much poorer than the 8th – 7th centuries B.C.E.

3.2. When do Levels Begin?

Finkelstein believes that the LC lowers strata downwards as a whole, but this is not exact. The LC, like any archaeological chronology (excluding 14C ones, for which see below), determines only dates of end of strata. If the LC is correct, certain strata reached an end in the 9th century B.C.E. Dates of establishment of these strata remain flexible, unless “pegged” by secure chronological datum from preceding strata at the same site. And even then, there may be gaps between the end of the former stratum and the beginning of the next one. For the early Iron Age in Palestine, we have no secure pegs.

Hence, there is nothing that precludes the possibility that 9th century B.C.E. strata (LC) were established a hundred years earlier, around the middle of the 10th century B.C.E. It is perfectly possible that massive buildings (such as Megiddo 1723 and 6000) endured a hundred years or more. Earlier levels (10th century, LC) could be of shorter duration than what the LC states. This possibility, a sort of Middle Chronology (MC), accepts the LC’s date of the end of strata, but raises their beginning, say, by a few dozen years. For example, Megiddo VA – IVB ended in the second half of the ninth century B.C.E., but began around 960 B.C.E. Its impressive buildings could be erected by Solomon/David. There are problems, for example, lack of destruction by Shishak (Shoshenq I). Accepting the biblical date of his campaign at ca. 925 B.C.E., it is possible that Megiddo surrendered and was not destroyed (cf. Ussishkin 1990, 72; Finkelstein 2002a, 122). The MC is similar in some ways with the chronology developed by Mazar (2001, 82–85) and others, but I wish to stress here the methodological aspect, not to suggest a working solution, which necessitates a much wider discussion.

If the MC is possible, and I believe it is, the LC claims about stripping the 10th century B.C.E. from monumental archaeology is an unproven assumption. Finkelstein’s own down-dating can be accommodated into an archaeological chronology that fits a flourishing United Monarchy, not by using any debated biblical sources – but by realizing the nature and limitations of archaeological methodology.

3.3. Negative Evidence and Gaps

Negative evidence can be meaningful, and I have tried to use it elsewhere (Kletter 2002). The LC uses negative evidence to unprecedented excess. It is seeping with negative equations and gaps, of pottery types, strata (Taanach, Megiddo, Hazor) and architectural features. Pottery resumes life of its own, as if it constitutes a temporal period. We read about “Bichrome phase” and “Bichrome free phase” (Finkelstein 1998b, 168); “collared rim phase” (Finkelstein 1996b, 182); and “Bichrome pottery sites” (Finkelstein 1996b, 184). The excessive use of negative evidence had been rightly criticized (Ben-Tor/Ben-Ami 1998, 31;
NA'AMAN 2000a, 2; MAZAR 2002, 265–270.275), because conclusions drawn from it are limited and should be used with utmost caution.

See NA'AMAN's remarks about Monochrome and Bichrome pottery. In Philistia, this pottery is common and found in three levels (not together) at sites such as Tell el-QasøÅle, Ekron and Ashdod. In northern Israel, it is rare, usually few sherds limited to one level at each site. Hence, “there are no 'Philistine' strata in north Palestinian sites, but rather 'local Canaanite strata' with some imported Philistine sherds” (NA'AMAN 2000a, 5). One should not assume — like FINKELSTEIN — a long duration for this pottery in northern sites. This erodes the need to restore gaps at these sites, which lead to lowering dates of levels. It further erodes far-reaching historical conclusions based upon such gaps, e.g.:

“The absence of Philistine pottery at Beth-shan, if indeed indicating a gap [...] in the 11th century, supports the Low Chronology for the Archaeology of the United Monarchy [...]. From the textual point of view, the possibility of an occupational gap at Beth-shan in the 11th century BC must be considered in any attempt to evaluate the biblical narrative of the battle of Gilboa and the death of King Saul [...]. The question of whether Beth-shan was inhabited or deserted in the 11th century B.C.E. is a revealing case of the uneasy relationship between text and archaeology [...] archaeologists working at Beth-shan or on the Beth-shan material, ignored the straightforward archaeological data or compromised them, in order to adjust the finds to vague texts, one of them — the biblical narrative of the death of Saul — possibly pseudo-historical in nature” (FINKELSTEIN 1996c, 178–179).

The question is, rather, the uneasy relationship between the creation of a gap without evidence, apart of negative evidence, and basing historical conclusions upon such an assumed gap (MAZAR 2002, 275).

3.4. Solution by Carbon 14

When BEN-TOR and BEN-AMI suggested that \(^{14}\text{C}\) dates give a solution (1998, 34), FINKELSTEIN answered that the evidence is not conclusive and even contradictory (1998b, 170). Later, he thought that \(^{14}\text{C}\) dates favour the LC and mentioned them as key for solution:

“The Low Chronology scheme for the strata in the north is now being supported by radio carbon dates from Dor, Rehov, Megiddo, Tel Hadar and other sites” (FINKELSTEIN 2001b, 340; cf. 2000a, 244 and note 7; 2002b, 125). “Megiddo, in particular, has produced some stunning contradictions to the accepted interpretations. Fifteen wood samples were taken from roof beams. Since some of the beams could have been used in earlier buildings, only the latest dates in the series can safely indicate when the structures were built. Indeed most of the samples fall well into the tenth century – long after the time of David [...] these dates have been confirmed by test of parallel strata such as [...] Tel Dor [...] and Tel Hadar” (FINKELSTEIN/SILBERMAN 2001, 141; cf. 2002, 66; for Sûh Hîd/TEL Hadar see MAZAR/CAMP 2000, 50).

This is questionable. Using only “latest dates” leads to late dates – one should present all dates. Roof beams could be replaced during renovation. The words “well into the tenth century” lack clarity. Perhaps it is “long after the time of David”, but what about Solomon? If one considers deviations, can we be certain about these dates?

SHARON (2001) and GILBOA/SHARON (2001) claimed that \(^{14}\text{C}\) dates from Dor support the LC. They used statistic treatment in order to “date the transitions between the different horizons” (GILBOA/SHARON 2001, 1345). They gave correlation to Cyprus and to Cypro-Phoenician pottery, but the chronology of Cyprus is not independent in the period under discussion (MAZAR 2001). They dated a stratum at Dor to “975–870” and wrote that it “by
and large postdates the reign of David”. Yet, David supposedly ruled between 1000–965 (as mentioned by them), and the United Monarchy continued much later.

MAZAR and CARMI (2001) presented detailed dates from Tell es-Sārem/Tel Rāhōv that support the HC (cf. MAZAR 2002, 273). Yet, it seems to me that at present, $^{14}$C dates do not solve the debate (cf. ZEVIT 2002, 20–1, note 33). If one takes maximal possible deviations into consideration, the ca. 50–100 years range between the HC and LC is too small to decide by current $^{14}$C techniques. This argument is valid not only for the HC (e.g., KNAUF 2002, 19), but for the LC as well.

**Excursus: Size of Nomenclature Matters**

FINKELSTEIN called the HC “traditional chronology” (1986b, 179) or “conventional dating” (1999b, 59), but sometimes “prevalent chronology” (1998c, 210) or “prevailing chronology” (1999c, 36–37), and even “old chronology” (1999c, 39). I use here the neutral form HC, in order to avoid any undesired connotation that it is old-fashioned or religiously conservative. FINKELSTEIN almost always wrote LC with capital letters, but HC with small letters. For example, “Low Chronology” versus “conventional dating” (same page, FINKELSTEIN 1999b, 59); “opting for the Low Chronology” against “the only real challenge to the traditional chronology” (same page, FINKELSTEIN 1999b, 179); “Low Chronology” versus “conventional chronology” (same sentence, FINKELSTEIN 2000a, 236); “Low Chronology” versus “traditional chronology” (same line, FINKELSTEIN 2000b, 124). Rarely, he used small letters for the LC (“low chronology”, FINKELSTEIN 1995a, 229; 1999b, 66 note 1; 1999c, 39). This shows that the use of capital/small letters is not a new grammatical rule, but a way to enhance the LC: big ideas merit capital letters. In one case, FINKELSTEIN denied capital letters from a supporting colleague: “Ussishkin did not elaborate on the […] implications of his revolutionary proposal, and as a result, his low chronology has been unanimously dismissed”. Three lines later, capital letters re-appear in relation with FINKELSTEIN: “I wish to support the Low Chronology” (FINKELSTEIN 1995a, 218).

In 2001, FINKELSTEIN proposed new terms: CC (for “Conventional Chronology”) and LC (FINKELSTEIN 2001a, 106). Nonetheless, he continued to use capital letters for the LC alone: “to avoid confusion, I have marked my dating of the finds (according to the Low Chronology system) ‘LC’ and other scholars’ views (according to the conventional dating system) ‘CC’” (FINKELSTEIN 2001a, 111 note 2). He also continued to use former terms, instead of his new CC/LC, for example: “conventional dating system” (FINKELSTEIN 2001a, 112 note 8, cf. note 14; cf. 2002a, 111); “Low Chronology” (FINKELSTEIN 2001b, 340; cf. 2002c, 122 –123; 2002d, 284). Meanwhile, a misnomer appeared – “the Finkelstein correction” (FANTALKIN 2001, 118). A correction must be correct before it is called a correction.

4. The Alleged Achievements of the LC

4.1. *The LC provides destruction layers for Hazael*

“it [the LC] provides the missing destruction layers for the campaign of Hazael, king of Aram Damascus, against Israel in the second half of the 9th century” (FINKELSTEIN 1999c, 39).

Yet, in archaeological terms, there is very rarely a secure identification of those who cause destruction. There is no shortage of possible causes of destruction for northern Israel in the Iron Age II. They may have included natural causes (earthquakes) or human causes (Shishak, Hazael, Jehu). Providing the above mentioned destruction layers to Hazael (LC) is done at the expense of Shishak (HC). We do not know for certain which one of the two caused more havoc; perhaps some sites were destroyed by Shishak, and others by Hazael. The attribution of destruction layers to either Hazael or Shishak is dependent mainly on the evaluation of written sources, not on archaeology, so it does not constitute a real achievement of the LC.
4.2. The LC provides sites for Shishak in the Negev

“it [the LC] provides a set of sites in the Negev Highlands for the Negev toponyms mentioned in the list of settlements conquered by Pharaoh Shishak in his campaign to Palestine in 926 (according to the old chronology these sites date to the 11th century)” (FINKELSTEIN 1999c, 39).

First, these Negev sites could have been established in the late 11th century and have functioned until Shishak’s campaign. Second, nobody can accurately date these sites between the late 11th and 10th centuries B.C.E., since their scant finds are composed mostly of Negev pottery, whose range of use is long (MESHEL 1994, 58–59). FINKELSTEIN at that time wrote: “it is extremely difficult to differentiate between the pottery of the late 11th and that of the early 10th centuries BCE” (1984, 193). Third, most scholars dated these sites to the 10th century B.C.E., and explained them as sites related to the United Monarchy and ruined by Shishak (including AHARONI at first, see COHEN 1979; MAZAR 1990, 390–396; MESHEL 1994; HAIMAN 1992, 160–194; 1994, 56–59). The late 11th century dating was not an accepted HC fact, but a debated opinion by a minority. The early date accepted by FINKELSTEIN at that time was not based on archaeological facts, but on the assumption that these sites belong to nomads in process of sedentarization. This reasoning is highly doubtful, since pastoralists cannot settle in a desert lacking sources of livelihood, unless they find outside support (HAIMAN 1992, 190–192; 1994, 59).

FINKELSTEIN admitted in 2002 that the HC dated these Negev sites to the 10th century B.C.E., but still HC scholars were wrong, not he, since the Negev sites “predate Arad XI and Tell es-Saba’ V – the strata which were thought at that stage of research to represent the time of the United Monarchy” (2002a, 114). Before the LC, even he was tempted to relate these sites with the United Monarchy, but this “would require lowering the date of the pottery assemblages of these sites to the late 10th century BCE, which is difficult to accept” (FINKELSTEIN 1984, 202 note 6). First, he dated these sites early and disconnected them from the United Monarchy; then he did not mention his own early dating; finally he lowered these sites down to the period of the United Monarchy (FINKELSTEIN 2002a, 114), but refuted the existence of this Monarchy.

The fact remains that by true LC standards, he should have lowered these sites (and Arad XII) to the 9th century B.C.E., because their common HC date was the 10th century B.C.E. (MAZAR 1997, 161; BUNIMOVITZ/LEDERMAN 2001, 143 note 47). This would leave Arad and the Negev empty during Shishak’s campaign – an unlikely scenario, since there is no debate about the historicity of the campaign and the place of the Negev in it (NA’AMAN 1985; 1992, 81–83; 1997b, 59). The LC, not the HC, faces a Negev problem in relation to Shishak.

4.3. The LC ‘saves’ 10th century B.C.E. Jerusalem

“it [the LC] ‘saves’ 10th century Jerusalem, in the sense that the Iron I material found there probably continued to be in use at that period of time (no significant ‘traditional’ 10th century pottery has ever been found in Jerusalem)” (FINKELSTEIN 1999c, 39). FINKELSTEIN called this ironic argument the most important one. The negative evidence about 10th century B.C.E. Jerusalem is, indeed, a major component in the LC. “It [Solution] should also be sought in wider arguments, such as a negative evidence from early Iron II Jerusalem […] which makes a prosperous United Monarchy highly unlikely” (FINKELSTEIN 2000a, 244). Compare FINKELSTEIN in 2002: “The poor material culture of Judah in the 10th century leaves no room to imagine great wealth in the Temple” (2002a, 112).
HC Scholars pointed that excavations are impossible in the temple mount, and that 3000 years of ongoing occupation and re-building could obliterate early remains. Na’aman (1996, 18–19.21–24) compared this situation with the almost complete lack of archaeological remains from Late Bronze Age Jerusalem, despite the reliable historical sources. Finkelstein vehemently opposed the idea that 10th century B.C.E. Iron Age Jerusalem was obliterated by later strata, but in 2001 he admitted it is possible for the Jerusalem temple and palace area (Finkelstein 2001a, 110). Also, there are remains and pottery attributed to the 10th century B.C.E. [HC] in Jerusalem (de Groot/Ariel 2000, 93–94; Mazar 1997, 2001, 83–84; Ben-Tor/Ben-Ami 1998, 36). Steiner (2001, 281) wrote:

“It seems that in the tenth/ninth century BCE Jerusalem was an administrative centre of at least regional importance [...] a town with impressive public buildings [...] a regional administrative centre or the capital of a small, newly found state”.

According to the LC, scarcity or lack of 10th century B.C.E. material is prescribed for many sites, not just Jerusalem. It is a feature of the LC, not an achievement of it.

4.4. The LC closes the 9th Century B.C.E. Gap in Judah

“The widely accepted view on the archaeology of the tenth century has left the ninth century as a ‘black hole’ in the archaeological sequence” (Finkelstein 1996b, 181). “Pushing the dates of the post-Philistine strata to the late-tenth-early-ninth centuries closes the ‘dark age’ of the ninth century BCE” (Finkelstein 1996b, 182; cf. 1996b, 183). Finkelstein presented this as a major achievement. He called it the ‘prime advantage of the LC’, and the gap the ‘Dark Age’ of the Iron II sequence, the elusive ninth century” (Finkelstein 1996b, 184). He repeated this claim frequently (Finkelstein 1999a, 134; 1999c, 39; 2001a, 112 note 14; 2002b, 123).

Preliminary results of new excavations suggest the existence of significant 9th century [HC] strata that are earlier than Lachish III, that close this ‘gap’ without lowering dates of strata. The excavations include Beth-shemesh; Tell es-Süfi level A5 and Ras Abû Humûd/Tell Humûd VII–VI (Bunimovitz/Faust 2001, 14–143; Maier/Ehlich 2001; Wolff 1999, 55–56).

Even if Finkelstein is correct, how does a shift in date of levels close a gap? Rather, he has shifted the gap to the 10th century B.C.E. Perhaps a 10th century gap in Judah is not a problem, because in his view there is no “full-blown” United Monarchy, just a small “chiefdom”. But is a 9th century gap in Judah a dangerous black hole? The nature of the archaeological record should be considered. If the 9th century in Judah passed in peace, without major destruction horizons, then we would find a relative dearth of archaeological artifacts from this period. This fits the Biblical description about a period relatively peaceful in a small, secluded kingdom like Judah. In that case, the gap would be imaginary. Hence, the assumed prime advantage related with its closure is of no importance (cf. Bunimovitz/Lederman 2001, 143).

Similarly, other gaps presented by Finkelstein as thorny problems, which only the LC solves, have all other simple explanations. First is the “problem of the ‘missing’ 10th century in Transjordan” (Finkelstein 1999c, 39). It is not certain that there were early kingdoms in Transjordan, and the situation there is comparable to Judah in that it could have been a relatively peaceful period. Second is

“the gap of a century or more between the beginning of monumental architecture (at Megiddo) and the appearance of other manifestations of developed administration in Israel, such as monumental inscriptions, administrative ostraca and inscribed seals and seal impressions” (Finkelstein 1999c, 39, cf. 1999a, 134).
Early Bronze Age Palestine shows monumental architecture, for example, the palace at Hirbet Yarmuq (DE MIROSCHER D 2003, 169) and very large sites, but no inscriptions, ostraca or inscribed seals. Yet, we do not lower the date of the Early Bronze Age remains to the time when inscriptions appear, in order to match their appearances, because that would be impossible (for inscriptions see also NA’AMAN 2000b).

4.5. The LC solves a Problem related to the Chronology of Proto-Geometric Pottery

Supposedly, the LC “closes the one century gap between the dating of the Late Proto-Geometric pottery in Greece (in the 10th century, not only according to Levantine synchronisms and its appearance in the Levant in strata usually dated to the 11th century BCE” (FINKELSTEIN 1999c, 39; cf. 1999a, 134).

This argument was added in 1999. It is incorrect for several reasons:

1. The LC does not solve this problem, because it lowers the date of later Iron Age strata, for example, at Megiddo itself. There are Greek imports in these later strata as well, so the LC lowers their date in accordance. This shifts the gap to later strata rather than solve it (cf. FANTALKIN 2001, 119: “It seems that we have a closed loop”).

2. There is no chronological anchor in Greece for the early Iron Age pottery. It is dated by finds from Near Eastern sites, based on local chronology. This was realized before the LC (e.g., FRANCIS/VICKERS 1985, 131; cf. FORSBERG 1995, 18–24; cf. WALDBAUM and MAGNESS: “the absolute chronology of early Greek pottery is based on the presence of a few pieces of sub-Proto-geometric and Geometric pottery at Near Eastern sites such as Megiddo, Samaria, Tell Abû Hawâm, el-Mina and Hamâl, all of which have problematic stratigraphy” (1997, 24–25).

3. Doubts about the dating of early Greek imports in the Levant strengthened after a Protogeometric vessel was discovered at Sêh Hîdr/Tel Hadar stratum IV (KÖCHAVI 1996, 191). It seems that FINKELSTEIN followed WALDBAUM’s ideas about this vessel (1994, 57): “It comes from a context that the excavators say can be no later than the late 11th century [HC]. According to standard (and now questioned) Greek chronology, however, it should date to the late tenth century B.C.” Yet, she questioned Greek chronology, not the Levantine one, and stressed that Greek sherds from Palestine are completely useless for dating purposes (FRANCIS/VICKERS 1985, 133–135; WALDBAUM 1994, 57–59; FORSBERG 1995, 18–24; WALDBAUM/MAGNESS 1997, 24–25; WALDBAUM 1997, 2–4; FANTALKIN 2001, 120–123).

4. The gap resulted from wrong dating of Greek sherds at Megiddo (FRANCIS/VICKERS 1985, 134–135; WALDBAUM 1994, 57; FANTALKIN 2001, 119); and from the fact that COLDSTREAM, who formed the basic chronology of this pottery, followed a former low chronology for Megiddo and Samaria by CROWFOOT and KENYON. Their low chronology was later abandoned in favour of the HC, but remained in use by scholars of Greek pottery (FANTALKIN 2001, 120–122). In other words, the gap is not real. FANTALKIN, who favoured the LC, had to conclude (2001, 121): “one obviously cannot use dating of Greek Protogeometric and Geometric pottery to support Finkelstein’s Low Chronology, as the dating of this pottery is based on the mistaken conclusions of CROWFOOT and KENYON from Samaria”. WALDBAUM and MAGNESS (1997, 25. cf. FANTALKIN 2001, 122) criticized “some Syro-Palestinian archaeologists [who] use the standard chronology of Greek imports to help date local material without properly considering the shaky Near-Eastern foundation for that chronology”. Rightly so, but FINKELSTEIN, who tried to bolster the LC with the use of such Greek sherds, is one of them.

5. The tension is not between the HC and Greek chronology, but between the different local chronologies. FANTALKIN tried harmonisation, still suggesting LC superiority: “It may be concluded that, paradoxically, the Protogeometric and Geometric Greek pottery finds do not support ‘Finkelstein’s correction’, but rather that Finkelstein’s ‘Low Chronology provides, for the first time, a basis for absolute chronology of that pottery [. . .] critics of traditional Greek absolute chronology [. . .] will now have to respond to Finkelstein’s reasoning’” (FANTALKIN 2001, 122). This contradicts all his former conclusions. How can the LC provide absolute chronology, when it itself is in dispute? Note that the situation is paradoxical only for FINKELSTEIN, who raised the issue in order to support the LC.
In fact, the LC is at a disadvantage. One should follow the archaeological rule that dating of small sherds is doubtful. Greek sherds from Samaria prove this rule. There, six fragments, presumably from one vessel, were found in four levels, including Hellenistic and Roman period levels (Francis/Vickers 1985, 133–134; Waldbaum 1994, 57; Fantalkin 2001, 120). Therefore, we should prefer Şeh Hidr/Tel Hadar IV as basis for the chronology of Protogeometric pottery, since there we have a whole vessel from a good context (of course, we still need an independent dating for stratum IV at Şeh Hidr/Tel Hadar). The LC claim regarding the problem of Greek pottery is not an achievement, but a mistake.

4.6. The LC solves the Problem of Dating bit hilānī Palaces

“adhering to the prevailing system, one is forced to date the bit hilānī palaces of Megiddo at least a century earlier than their supposed prototypes in north Syria. The Low Chronology would make the monuments of the two regions contemporaneous” (Finkelstein 1996b, 185 note 3; cf. 1999a, 134; 1999c, 39).

This alleged achievement was presented in 1996 in a note. In 1999, Finkelstein suggested that the Hazor citadel should be reconstructed as a bit hilānī (Finkelstein 1999b, 61), relating Hazor with the Arameans. He saw these buildings as part of the Omride “kit” of architecture, but at the same time, as imports from the north: “the bit hilānī palace, which was imported to Palestine from the north” (Finkelstein 2000b, 126). He also became trapped by being unable to resist Ussishkin’s identification of Solomon’s palace as a bit hilānī (1966):

“If one accepts Ussishkin’s idea (1966), that the description of the temple in I Kings 7 refers to a bit hilani, it would be impossible to assign it to the tenth century. The bit hilani concept, originally a Late Bronze design (Frankfort 1952), re-emerged in Syria only in the early ninth century (for the Tel Halaf palace, the most important building for dating the appearance of the Iron Age hilani, see, e.g., Winter 1989; contra Albright 1956) and was imported to Palestine by the Omrides in the first half of that century [. . .]. It is not possible to envisage the construction of a bit hilani in remote, marginal Jerusalem prior to the appearance of its prototypes in Syria” (Finkelstein 2001a, 110).

There is some confusion here between Solomon’s temple and palace. Some scholars do identify parts of the temple with bit hilānī (Ouellette 1969, with references back to 1892), but Ussishkin’s identification (1966) concerned the palace, not the temple. Finkelstein’s readers are perplexed, because for the first time they hear about a Late Bronze Age design, but the implication of this discovery is hidden by a new term: “Iron Age bit hilānī”. What is a Bronze Age bit hilānī and in what way does it differ from an Iron Age one? Niemann (2000, 70) followed Finkelstein: “If palace 6000 is a bit hilani, it could be Hazael’s palace”. Finkelstein/Silverman (2001, 140) continued to embrace the

“even more troubling chronological problem: the bit hilani palaces of Iron Age Syria – which were supposed to be prototypes for the Solomonic palaces at Megiddo – appear for the first time in Syria in the early ninth century BCE [. . .]. How would it have been possible for Solomon’s architects to adopt an architectural style that did not yet exist?”

However, the whole argument is not valid, because of the many following reasons:

1. Without proof for the origin of the bit hilānī, why assume a northern Syrian origin, and not an origin in Megiddo? Historically several examples are known of new ideas or inventions which came from Cisjordan.
2. The whole issue depends on one building at Megiddo (building 6000). The Jerusalem temple and palace did not survive, their reconstruction is open to debate. The identification of the Hazor citadel as bit hilani is a reconstruction by Finkelstein, since the superstructure did not survive. Other scholars identified the Hazor citadel as a four-room building or as an Assyrian one (Milson 1991). Arav and Bernet identified a building at et-Tell (Beitsaida?) as a bit hilani and dated it to the 10th century B.C.E. [HC] (2000; cf. Arav/Freund 1999, 44–53). However, its designation is not certain – it is not clear if the entrance had pillars (Arav/Bernet 2000, 54.69), and the main hall might have been a courtyard. Finkelstein did not express a clear opinion about the definition (1999b, 61; 2000b, 125), but rejected the dating (Finkelstein 2002b, 126–127). Another building at Jericho lacks floors and doorways – only foundations exist (Arav/Bernet 2000, 75–77). Palaces at other sites did not survive enough to be clearly identified. At Jezeel, even the exact location of the palace is unknown (Ussishkin 2000, 249). For Megiddo, Finkelstein himself wrote: “Palace 1723 has been described as a bit hilani, though its plan is much different from the typical bit hilani in Syria” (2000b, 120). Megiddo palace 6000 is all that is left (Finkelstein 2000b, 120). Yet, Finkelstein concludes about the Omride palace type: “In most (?) cases it was built according to a bit hilani plan” (2000b, 122).

3. The definition of bit hilani is problematic, since it relates to written sources that focus mainly on the entrance or portico, often with pillars, but the superstructure of both of the Megiddo buildings did not survive. The restoration of palace 1723 as a bit hilani (Ussishkin 1966, 182, cf. Mazar 1990, 382–383), was rejected by Fritz (1983, 54–56; followed by Kempinski 1989, 162–163; Herzog 1997, 212; Arav/Bernet 2000, 72–74). The position of the entrance of building 6000 is unknown, because of the poor state of preservation (see Herzog 1997, 212). Arav/Bernet admitted that: “the palace’s structure cannot be determined on the basis of existing ground plans” (2000, 74–75). It could be a palace with a central court and not a bit hilani (Fritz 1983, 57–58; cf. Reich 1987, 175, fig. 3; Ouellette 1969, 371, note 38).

4. Frankfort traced the architectural origin of the bit hilani to the 15th century B.C.E. palace at Alalah, where the form is already fully developed (Frankfort 1952). He was followed by Ussishkin, who wrote: “the origin of the bit-hilani must be looked for in Syria in the second millennium B.C., as shown by Frankfort” (1966, 186). This was also the opinion of Fritz (1983, 43–44), Reich (1987, 175), and Ouellette (1969, 370–371). Margueron reported a bit hilani palace from 13th century B.C.E. Emar (1979, 160–170; 1982, 25; cf. Pitard 1996, 17). Margueron thought that the palace at Alalah is not directly related to the bit hilani at Emar (1979, 174). Hence, in his view, the origin of this type of buildings lies elsewhere – and earlier than the 15th century B.C.E. McClellan thought that the Emar house was perhaps domestic (1997, 30–31). However, he accepted the designation of the 15th century B.C.E. palace at Alalah as a bit hilani (McClellan 1997, 37). If the bit hilani appeared in the 15th century B.C.E., the Megiddo buildings, even if true bit hilani ones, pose no chronological problem. Finkelstein was not aware of these scholars, or did not bring their views, which contradict his argument.

Because of the problems of defining the bit hilani, naming a certain building as such does not say much about its origin or the identity of its builders/owners. Megiddo buildings 1723 and 6000 – like four-room houses or six-chambered gates – can be part of a wide repertoire of architecture not related solely to one specific political entity or ethnic community. Such buildings are no more Aramaeans than Israelite, unless this can be corroborated by other evidence than the plan alone (e. g., inscriptions). A simplistic “bit hilani and people” identification is not better than a simplistic “pots and people” one.

4.7. The Historicity of the United Monarchy

The most controversial aspect of the LC is its relation with the United Monarchy, and this aspect brought the LC into the limelight. Prior to the LC, Finkelstein referred to an existing United Monarchy, archaeologically and historically (Finkelstein 1988; 1989; 1993; 1995b; cf. 1999a, 132). At the beginning of the LC, he gave one modest evaluation:
“Needless to say, all this has nothing to do with the question of the historicity of the United Monarchy.” But then, he added: “The Kingdom of David and Solomon could have been a chiefdom, or an early state, in the stage of territorial expansion, but with no monumental construction and advanced administration” (FINKELSTEIN 1996b, 185; cf. 1999c, 42).

However, on the same page FINKELSTEIN gave a very different evaluation, which became dominant in all other LC papers:

“Accepting the LC means stripping the United Monarchy of monumental buildings [. . .]. In all probability, the ashlar palaces of Megiddo should then be dated, together with similar construction in Samaria and Jezreel, to the Omride dynasty. From this point of view, the northern kingdom of Israel would emerge as the first real, full blown state in Iron Age Palestine” (FINKELSTEIN 1996b, 185). He wrote that the question of the United Monarchy was an aim of research, which achieved a solution: “To start with the conclusion, it would be fair to say that the identification of the archaeology of the United Monarchy is far from being a decided matter. Actually, it is a classic case of circular reasoning and dead reckoning. In what follows, I wish to discuss the search for the archaeology of the United Monarchy free of any conventional wisdom, text bias, or irrelevant sentimentality” (FINKELSTEIN 1996b, 178). He added: “The debate on the archaeology of the United Monarchy is much more than a dispute over the dating of strata and pottery assemblages in the main mounds of Palestine: it is part of a quest to emancipate Iron Age archaeology from Bible archaeology [. . .] it is an effort to scrutinize the distorted roots of Bible archaeology and to establish the archaeology of the Iron Age II on independent foundations” (FINKELSTEIN 1998b, 167). He combined the LC directly with the historical debate about the United Monarchy: “The principal disadvantage [of the LC] is for Biblical history [. . .] the alternative scheme puts the monuments previously dated to the second half of the tenth century in the early ninth century BCE. This, of course, would change the entire understanding of the history of Israel and of the emergence of the state in Iron II Palestine” (FINKELSTEIN 1996b, 184). He referred here explicitly to the entire history of Israel – not to archaeology. He wrote: “In historical terms this means a shift from the days of the United Monarchy under Solomon to the Omrides of the Northern kingdom of Israel. Consequently I have argued that the monumental architecture in the relevant strata [. . .] show that the Northern Kingdom of the early 9th century was the first fully developed state in early Israel” (FINKELSTEIN 2000b, 114; cf. MAZAR 2002, 267 note 7). Again, the terms are historical here, not archaeological. FINKELSTEIN found, at this stage, common ground with the Copenhagen school (cf. ZEVIT 2002, 25 – 26). “Coupled with this re-evaluation of the material evidence, the rise of the minimalist school in biblical studies [. . .] is particularly significant” (FINKELSTEIN 1999c, 36).

Yet, he hardly discussed Biblical sources during the first LC papers. Citations of Biblical verses were rare, and there was no detailed discussion of Biblical sources. This resulted from a general conception. Namely, that the new LC is based on an independent, scientific archaeology, free of “Biblical bias”, and hence, there is no need to discuss Biblical sources. FINKELSTEIN joined (only at first) the cry to “free” Iron Age Palestine/Israel from the Old Testament. He has succumbed to rhetoric in this matter (ZEVIT 2002, 25–26). However, refutation of the historicity of the Bible, or parts of it, is only another interpretation of it. It is legitimate as long as it follows the methodological rules that govern academic study of any written source. It can only be achieved (if at all) with the same meticulous reading and analysis of the very same Bible, in order to refute its reliability. This is an imaginary freedom, since it is not the Bible (or any other source) that chains us, but the very process of writing about the past. To change the name of ancient Israel into Palestine or “ancient Israel” (with quotation marks) is only to change a mask in front of the past. The many works related to the Copenhagen school do prove that there is indeed a real impossible thing for scholars in our field: i. e. not to write a history of ancient Israel – even if it would be a history of its denial.
The United Monarchy was also denied with the argument that the first (Iron Age) states in the area appeared in the 9th century B.C.E. 

INKELSTEIN/SILBERMAN wrote that: “Nowhere else in the region – from eastern Turkey in the north through western Syria to Transjordan in the south – was there any sign of similarly developed royal institutions or monumental buildings in the tenth century BCE” (2001, 140). However, in 1999 FINKELSTEIN admitted that ŠehêHûdâr/Tél Hadar stratum IV and Kinneret (Tell el-Orêmè) stratum V are a sort of an Aramaean “state formation” from the 10th century B.C.E. (LC; FINKELSTEIN 1999c, 47; for Kinneret see FRITZ/MUNGER 2002). He acknowledged “stately status” to Iron Age I Moab and Ammon (FINKELSTEIN 1999c, 43). If so, the appearance of a state in the 10th century B.C.E. is no longer an exception. Moreover, there are royal inscriptions and an early kingdom at Byblos (HANDY 1997, 156). Recent redating of Gordion, based on $^{14}$C and other dates, supports an early Phrygian kingdom in Anatolia (VOIGT/HENRICKSON 2000). It also seems that some Neo-Hittite kingdoms in Syria were established before the early 9th century B.C.E. (HAWKINS 1995, 87–95; CHAVALAS 1997, 168.171–174; NA’AMAN 2000b, 92–95).

LC papers tend not to keep a clear distinction between archaeology and history even in terminology. One can refer to the archaeology of the 10th century B.C.E., or the historical/archaeological United Monarchy; but “Archaeology of the United Monarchy” is a mixture of the two disciplines. Similarly, FINKELSTEIN’s separation between his “biblical archaeology”, “the archaeology of the biblical periods”, and his opponents’ “Bible archaeology”, “archaeology which is dictated by uncritical reading of the Bible” (1998b, 167 and note 1), is an odd mixture. On the one hand, not everybody else except FINKELSTEIN reads the Bible uncritically. On the other hand, what are “biblical periods”, especially if one maintains that significant parts of the Bible are not historically reliable? This stands in sharp contrast to what FINKELSTEIN wrote in 1993 about the debate between ‘Biblical Archaeology’ and ‘New Archaeology’: “the only meaningful distinction is the one between good archaeology and bad archaeology” (1993, 56).

The picture changed with more recent LC papers, when FINKELSTEIN backed away from the Copenhagen school:

“Scholars in the third camp – the centrists, to whom I subscribe – accept a late-monarchic date for a large portion of the Pentateuch and the Deuteronomistic History [. . .] the center camp is not too far from the radical, the difference being only a matter of dating the texts [. . . but] the centrists accept the value of biblical texts, mainly the books of kings, for reconstructing the history of monarchic Israel and Judah” (FINKELSTEIN 2001b, 324–325).

He still claimed that “The possibility that the Deuteronomistic History retains a memory from the tenth century BCE is highly unlikely” (FINKELSTEIN 2002b, 125), but he realized that the Deuteronomistic History does include reliable early material. It is just the material about the glorious 10th century B.C.E. United Monarchy which is not reliable. Pre-Deuteronomistic layers become abundant (FINKELSTEIN 2001a, 110, note 11; 2001b, 334; FINKELSTEIN/SILBERMAN 2002, 64). “I would dare to suggest that the Deuteronomistic Historian had access to written Northern sources” (FINKELSTEIN 2001b, 334, hardly a daring novelty). In opposition to the first phase of the LC, FINKELSTEIN acknowledged that even etiological legends have an historical kernel of truth (FINKELSTEIN 2001b, 337; 2002b, 125 [about Cabul]).

At this phase, FINKELSTEIN found again many reliable historical aspects of the United Monarchy. He accepted David as a historical figure: “Stories about the activity of David and his band in the south does contain, so it seems, valuable information [. . . that] represent early
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materials” (FINKELSTEIN 2001a, 107). If David was a runaway (from whom?), why dismiss him as a king? King Idrimi did not remain a “runaway chieftain” all his life – had he done so, probably we would have known nothing about him. FINKELSTEIN accepted that Jerusalem had a palace in the 10th century B.C.E., just not a bit hilâni one (2001a, 110); it also had a temple – or, at least, a shrine (FINKELSTEIN 2001a, 110; 2001b, 331). “Memories” of David have an historical kernel, he was the founder of a dynasty (FINKELSTEIN 2001a, 107; FINKELSTEIN/SILBERMAN 2002, 63–64). There are “early Davidides” and “Saulides” (FINKELSTEIN 2002a, 127–129). This stage is marked by a very liberal use and quoting of Biblical sources (FINKELSTEIN 2002a; 2002c). It is as if FINKELSTEIN’s scientific, independent archaeology of the former phase, presented as the solution to the problem of the United Monarchy, finds itself now in need of Biblical support. FINKELSTEIN backed away from the Copenhagen school, but did not change the ‘archaeology of the United Monarchy’. This created tensions. FINKELSTEIN/SILBERMAN wrote:

“many of the archaeological props that once bolstered the historical basis of the David and Solomon narratives have recently been called into question” (2001, 124). Yet, in the same breath, they wrote that the Dan inscription is a “dramatic evidence of the fame of the Davidic dynasty less than a hundred years after the reign of David’s son Solomon” (2001, 129).

If this inscription restores the debated historical fame of the United Monarchy, does it not restore some of the debated archaeological fame of it as well? If David “could have dominated areas in the northern hill country, possibly near Bethel and maybe even further to its north” (FINKELSTEIN 2001a, 108; FINKELSTEIN/SILBERMAN 2001, 135) – more than Judah of the late Iron Age II, probably, – why insist on his being a “highland chieftain” ruling only a “highland village”? A significant lowering of the negative appreciation of the historicity of the United Monarchy requires a comparable lowering of the highly negative archaeological appreciation of it. After all, the archaeological interpretation in this case is largely dependent on the interpretation of the written (Biblical) sources.

With such swift changes in the LC, evaluation becomes difficult. Perhaps we should evaluate only what survived intact. Regarding the United Monarchy, this seems to be a denial of its ‘glory’ (expansion, wealth, etc.). As FINKELSTEIN/SILBERMAN wrote: “our disagreement with him [DEVER] is mainly about the historicity of a vast and powerful United Monarchy” (2002, 64). Yet, scholars denied the United Monarchy before the LC (the Copenhagen school), or estimated that its depiction as a golden period is an exaggeration of the Biblical writers, without far-reaching historical implications. The LC addition was an archaeological refutation, but based only on the “chieftdom/state” concept, in turn based on negative evidence (especially the lack of remains from Jerusalem, FINKELSTEIN 2000a, 244; 2001a, 110; 2002a, 112).

Because of the changes and tensions, reliance on questionable negative evidence, lack of clear terminology, and use of an outdated theoretical model, the LC does not solve “archaeologically” the debate about the United Monarchy. NA’AMAN reached a similar conclusion from a different perspective:

“There is no chronological obstacle to the assumption that either Stratum VB or VA–IVB [at Megiddo] was founded by King Solomon. Whether he actually built in Megiddo depends on the evaluation of the biblical traditions about Solomon” (NA’AMAN 2000a, 5; cf. 1996; KNOPPERS 1999).
Accepting the few more modest declarations of FINKELSTEIN is a better avenue, but this means that the LC loses its last alleged achievement. The LC, therefore, should be regarded as a different chronological arrangement of early Iron Age strata, which is not yet proven. This arrangement can fit several interpretations of the biblical data, and does not prove the lack of existence of the United Monarchy.

5. Conclusions: a New Paradigm?

FINKELSTEIN presented the LC as a new paradigm: “It seems suitable to end this paper with a quote from Thomas Kuhn (1970): «The man who embraces a new paradigm [. . .] must have faith that the new paradigm will succeed with the many large problems that confront it, knowing only that the older paradigm has failed with few»” (FINKELSTEIN 1998a, 145).

Paradigm is a complex term. We all admire KUHN’s work, and need not carry this debate to his doorstep. Yet, paradigms must be proven as better agents of explanation and accepted by a community of scholars in a certain field as a whole. These conditions are lacking for the LC at present (MAZAR 2001, 82 note 51; ZEVIT 2002, 22, note 35–36). FINKELSTEIN/SILBERMAN (2001, 66–67) listed 15 supporters of the LC, but most are from the close circle of FINKELSTEIN: students, Tel Aviv colleagues and members of the Megiddo and Jezreel excavations. MAZAR does not belong here. Many of these 15 scholars support only small segments of the LC, not the whole of it.

In what sense does the LC form a new paradigm? The achievements allegedly brought by it are insignificant or do not exist. Most of its components existed earlier. The historicity of the United Monarchy was, of course, debated by LEMCHE (1985; 1996; 1997), THOMPSON (1992), DAVIES (1992) and others (this was a fundamental change, regardless of our opinion about it). USSISHKIN (1985) invented the low chronology for Philistia. CROWFOOT and KENyon created a low chronology for Megiddo and Samaria. A low chronology was debated in the 1950s about Tell Abū Hawām (MAZAR/CARMI 2001, 1340). WIGHTMAN advocated a low chronology which is very similar to that of FINKELSTEIN: “The article presented an outline for a ‘low chronology’ of the tenth and ninth centuries B.C. in Palestine [. . .] downdating many of the formerly Solomonic buildings to the first half of the ninth century B.C.” (WIGHTMAN 1990, 19–20; cf. 1985). He understood that both chronologies, high and low, are possible. Scholars realized that Palestine’s archaeology of the 10th – 9th centuries B.C.E. lacks secure ‘pegs’ (NA’AMAN 1992, 83). The step model of “chiefdom” to “state” was widely used for ancient Israel. Denial of the United Monarchy as a “state” was made by JAMESON-DRAKE (1991), THOMPSON (1992), and GELinas (1995). The LC is not based on sound methodology, but largely, on negative evidence and on an outdated model of social evolution. It does not include methodological contributions, but rather suffers from some several methodological errors. Hence, the LC is not a new paradigm.

The LC does not solve the debate about the United Monarchy. The shift in the middle of the debate, away from the Copenhagen school and back to a quite historical United Monarchy, empties much of the claims about the historical importance of the LC. KNAUF’s accusations (2002, 18) against FRITZ and MÜNGER fit much better the LC than the HC. The LC ‘wins’ only in using sharp language and in presenting itself in terms of certain facts. These should not be recipes for accepting academic theories. Yet, as long as secure chronological pegs are missing, it is possible to maintain different arrangements of archaeological strata of early Iron Age Palestine. Hence, the LC is possible, though it is not superior to the HC. Before it is proven, no far-fetched historical conclusions should be based upon it.
Unfortunately, BEN-TOR’s suggestion to cease this debate until a solution is found (2001, 301), was not heeded by FINKELSTEIN. Much like a book of fiction, a theory presented to the academic world must be explored and evaluated by scholars other than those who wrote it. If it has to be constantly nourished and kept alive by frequent changes and revisions from its author, it does not deserve a place of its own. Scholars can mention in a note which chronology they use, and follow it consistently. But it is time to move towards more fruitful avenues of research, away from a debate about 50 years, which is fuelled by ego and by such a doubtful theoretical basis. There is a whole world to study, as our own limited chronology tickles away.

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