The Conquest of Ai: A story of Biases, Evidence, and Inferences

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Abstract

This treatise covers the history, now more than 170 years long, of researches and debates concerning the biblical city of Ai. This archetypical chapter in the evolution of biblical archaeology and historiography was never presented in full. I use the historical data as a case study to explore a number of epistemological issues, such as the creation and revision of scientific knowledge, the formation and change of consensus, the Kuhnian model of paradigm shift, several models of discrimination between hypotheses about the past, the interplay between scientists' values and their epistemic beliefs, and the truth-bearing of historiographic reconstruction. I show, in particular, that when scientists share common epistemic values they can modify their beliefs even when such changes go against deeply entrenched biases and preconceptions. Considerations of coherence usually constrain such modifications, and when sufficient data is available the result can be a profound change of beliefs. Several episodes of the "Ai debates" demonstrate, however, that similar change of beliefs will not occur when non-epistemic values take precedence.

Introduction

In the early 1930s, a young and inexperienced archaeologist named Judith Krause astounded her colleagues when she announced that her excavations at a mound named Et-Tell, generally believed to be the location of the Biblical Ai, showed that the site was inhabited only during the Early Bronze Age and the Iron Age. Besides contradicting prior results of leading experts, this outcome implied that the story of the conquest of Ai, detailed in chapters 7 and 8 of the Book of Joshua, is legendary and lacks any historical substance.

The announcement drew immediate and widespread responses from scholars in the relevant disciplines. As a short examination of their publications can quickly reveal, at stake was more than scientific theories and personal reputations: Most of the archaeologists who worked in Palestine at the time were graduates of Christian denominational institutions, many of them ordained priests. The minority, including the young excavator herself, were Jewish and Zionists, who regarded the biblical narratives as cherished national lore. It is hardly surprising, therefore, that expressions such as "disturbing," "perplexing," "deep embarrassment," and the like were repeatedly heard for more than forty years after the appearance of the excavation report.

Something that was *not* heard, however, was reservations about the archaeological and chronological determinations. Despite the researcher's young age and lowly status and notwithstanding the disruptive and subversive character of her conclusions, all considered them as veridical. Scholars who found the results disturbing offered all sorts of explanatory hypotheses to reconcile them with their general historiographic views, essentially modifying the latter.

I think that this acceptance, by itself, calls for an explanation. According to some current theories in the sociology and the philosophy of science, scientists' background, beliefs, and *Weltanschauung* play a decisive role in shaping their inferences. Archaeology, in particular, is known to be a strongly theory-laden discipline. What was it that made Marquet-Krause's[1] conclusions so definitely acceptable to the scholars of her time and beyond?

A large part of this work revolves around this question. In this context, I discuss the nature of archaeological knowledge (with particular attention to ceramic typology which was instrumental in the chronological determination mentioned above), the risk of circularity and its possible mitigation through independence of evidence, and the interplay between scientists' values and their epistemic beliefs. The history of "the Ai conundrum" perfectly illustrates Tucker's (2004: 42) observation that as long as scientists' epistemic values are hierarchically superior to their other values,

[1] She married the orientalist Yves Marquet in 1934.

empirical evidence can induce modifications of scientific opinions. A counterexample is also available in the form of latter-day attempts to salvage the biblical Ai story through a substantial revision of the current consensus.

The lengthy and rich history enables me to illuminate a few topics in epistemology. I discuss the adequacy of the Kuhnian paradigm-shift concept for the changes that occurred in the scientific opinions about the biblical Ai story, the relative merits of several conceptual frameworks for the discrimination between historiographical hypothesis, the crucial role of coherence considerations in the shaping of an epistemic stance, the validity of inference from absence in archaeology, and the interplay between scientists' values and their epistemic stance. In a final section, I defend the idea that such cases of surprising, complicated and yet resilient discoveries about the distant past demonstrate the power of scientific research to discover mind-independent truths.

Inspired by Chang's (2004) "The invention of temperature," this treatise is divided into two parts. The first one is historiographic: It describes the sequence of historical episodes, from the early attempts to identify the location of the biblical Ai in the 1830s to recent articles that reopened this subject in the 21st century. I attempted to strike a balance between the need to provide the pertinent information and the wish to limit the size of an anyway lengthy essay. Background subjects which are pertinent to the discussion but are not directly related to Et-Tell and Ai, such as the evolution of ceramic typology in 19th-century Palestine and the date-of the-Exodus debate, are described in three "interludes" and one "digression" which are interspersed between the chronological sections.[2]

The second division begins with a concise summary of the history and goes on to discuss the epistemological issues mentioned above. The sections in this part are interrelated, but I tried to make each one self-standing. Internal cross-references are provided in the text.

[2] For a broader presentation of the history of biblical archaeology see Moorey 1991; Finkelstein and Silberman 2002; Dever 2003; Davis 2004.

The concluding section of this chapter restates its rationale and adds some general comments.

Division A: The history of a site and a legend

"Ai is simply an embarrassment to every view of the conquest that takes the biblical and archeological evidence seriously."

Callaway 1968

The Bible mentions Ai several times: as a place (Gen. 12:8, 13:3), east of Bethel, near which Abraham built an altar and sacrificed to God; as a fortified Canaanite city (Josh. 7,8), whose conquest by Joshua and the Israelite is described in vivid details; and as a city to which te Israelites returned (Ez. 2:28, Neh. 7:32, 11:31) in the post-exilic period.[3] The geographical, archaeological, and historical correlates of Ai have been the subjects of intense interest and scholarly debates for nearly two centuries.

This division presents the research activities and opinions related to Ai in modern times. It follows a chronological order, with some deviations from the strict timeline where I thought that grouping things by a topic would make the exposition clearer.

1. Where is Ai? Travelers with the Bible in hand, 1838-1900

The first to search for the location of Ai in modern times was the American Edward Robinson. Armed with a consummate knowledge of the Bible and other classical sources, and accompanied by Eli Smith, a Beirut-based missionary and translator of the Bible to Arabic, the professor from the Andover Theological Seminary traveled through Ottoman-ruled Palestine in 1838 (and again in 1852), with the express intention of locating biblical sites. Toward the opinions of past pilgrims (and Catholic traditions in general) on these matters he held nothing but contempt. His conclusions were derived from juxtaposing local geography and place-names with what he considered to be reliable texts. These included the Bible, of course, but also

[3] Some scholars, but not all, interpreted Isa. 10:28 mention of "Aiyath" and I Chr. 7:28 "Aiyah" as referring to Ai.

early geographical treatises such as Eusebius' *Onomasticon* and Jerome's *De situ et nominibus locorum Hebraeorum*, both from the fourth century A.D.

The latter sources indicate that Bethel should be "about twelve (Roman) miles from Alia (Jerusalem)" and "on the right of the road leading to Neapolis (Shechem)." Estimating the distances by horse ride and noting that "the Arab termination *în* for the Hebrew el is not an unusual change," Robinson concluded that "There is little room for question that both the name and the site of Beitin are identical with those of ancient Bethel" (Robinson 1856:127). It was more than a hundred and thirty years before someone (A12) disagreed with this conclusion.

But where was Ai? Here Eusebius and Jerome offered little help, saying only that it is "a small heap of stone" somewhere near Bethel. Gen. 12 specifies that it should be east of Bethel. Contemplating the account of battle in Josh. 8:17,[4] Robinson observed that these two cities

> were not so far distant from each other, but that the men of Bethel mingled in the pursuit of the Israelites, as they feigned to fly before the king of AI, … yet they were not so near, but that Joshua could place an ambush on the West (or Southwest) of Ai, without its being observed by the men of Bethel. (313)

Armed with these observations, Robinson attempted twice to locate the remains of ancient Ai. He was able to identify two suitable alternatives: "A site with ruins South of Dîer Dîwân; or upon a rocky Tell (hill) … overlooking a deep northern valley" (119; cf. also the maps in Figs. 1 & 2). In his second tour of the area, he decided that the latter would not do because "We expected to find here some remains of an ancient city, but there was nothing save a cistern and an immense heap of unwrought stones." For a similar reason, he rejected some other (unnamed) alternative in the vicinity and concluded, somewhat hesitantly, that "After all our search, we could come to no better result than to assign as the probable site of Ai with ruins just south of Dîer Dîwân… having near by [sic]… another smaller Wady, in which the

[4] "And there was not a man left in Ai or Bethel, that went not out after Israel…"

ambuscade of the Israelites might easily have been concealed." He did not name this "site with ruins," however, and that caused some misunderstandings later.

Over the following half-century, various scholars-travelers attempted to locate the site of Ai with greater certainty. Like Robinson, all applied the logic of juxtaposing the ancient texts with local geography, topography, and extant toponyms. Shared by all was a firm belief in the literal veracity of the biblical account. This led them to look for valleys that could hide a force of five thousand men (tacitly disregarding Josh. 8:4 which speaks of 30,000), the "Shevarim" of Josh 7:4, the heap of stones that was piled over the body of the king of Ai, the hill between Bethel and Ai upon which Abraham placed his altar or the remains of Jeroboam's idolatrous temple.

This methodology, it turned out, produce a handful of Ai-alternatives (Fig 2): The Dutchman Charles William Meredith van de Velde (1854: 279) rejected Robinson's (still unnamed) preferred choice for being on the South-Southeast, rather than strictly East, of Bethel. Following a suggestion by the British Consul James Finn, he opted for Robinson's first alternative: The cistern there was, for him, a sufficient evidence of an ancient city. He was also impressed by the resemblance between the site's name (he reported that the locals call it Tell-el-Hajar – "the mound of stones") and the language in Josh. 8:28 "So Joshua burned Ai and made it a permanent heap of ruins, a desolate place to this day."

The establishment of the British Palestine Exploration Fund in 1865 led to a renewed interest in the Biblical geography of Palestine. Charles Wilson's article, "On the site of Ai and the position of the altar which Abraham built between Bethel and Ai", appeared in the first volume of the Fund's Quarterly Statement (Wilson 1869). It summarizes several days of fieldwork that "consisted in personally visiting every hill-top and almost every acre," but first set out to clarify a question of toponymy: Van de Welde's "Tell el Hajar" also bears the name "Et-Tell." From a certain Reverend Williams, Wilson learned that Josh. 8:28, when consulted in the Hebrew origin, actually uses the words "*tell olam*" to describe the state in which Joshua left Ai. And if this was not enough to clinch the identification, "the topography of Ai is as

minutely described as that of any place in the Bible… These features are all found in connection with Et-Tell, and with no other place in the neighborhood of Bethel."

Not everybody concurred. The French explorer Victor Guérin published in the same year a detailed report on his journeys in Judea (Guérin 1869). After discussing several alternatives in the vicinity of Beitin / Bethel, he rejected Van de Welde's suggestion since "*le Khirbet Tell el-Hadjar n'offres le ruines d'une-ville veritable : ce sont les restes d'un simple village*" which would not accommodate even a tenth of the presumed population of ancient Ai. For the same reason he disapproved of Khirbet Hyeh (Hayian?), which was suggested by a certain Dr. Kraft. Khirbet El-Khudrya, on the other hand, "*presente tous les characters d'une cite antique rasse.*" This site, he believed, was also Robinson's choice.[5]

The next round of Ai searches came with the Survey of Western Palestine, conducted under the auspices of the Palestine Exploration Fund by officers of the Roya Engineers Corps. In his report for the year 1873, Charles Conder (1874) sided with Wilson, both about the true name of Et-Tell/El-Hajar (which he checked "with a dozen separate witnesses") and its perfect conformity, topographically speaking, to the details in Josh. 7-8.

Yet later, Conder changed his mind. In his book "Tent work in Palestine – a record of discovery and adventure" (1887), as well as in his summary report to the Palestine Exploration Fund (1881), he rejected Et-Tell both for the old argument that it "does not show traces of having at any time been covered by buildings" (Conder 1887: 109) and also for being "too close to the pass, it has moreover no valley fitting for an ambush to the west of it." Khirbet Haiyan (which Guérin summarily rejected) seemed to him now an ideal choice: It is two miles to the west of Bethel and "between the two sites is the open ravine …where unseen, yet close at hand, the ambush may have Iain concealed" (1881:37). Additional support for this suggestion he derived

[5] In this he was probably wrong, because Robinson's reference to the "site with ruins" seems to be somewhere in the southwest of Beitin, while Kh. Khudrya lies to the southeast of it. In fact, Albright (1924:141) surmised that Guérin mistook Kh. Haiyan for Kh. Khudrya.

from Josephus, citing the Greek version of Josephus' *Kadmoniot* which uses the name Aina for Ai.

Horatio Kitchener, Conder's junior partner to the Survey of Western Palestine (and of later Khartoum and WWI fame), offered yet one more Ai-alternative: A site named Khirbet el-Hai "one mile east of Mukhamas." The topography of the place, he argued, fits even better the description in Josh. 8. That the site is rather far from Bethel might be, he conjectured, an argument in its favor. It would explain why the Israelites, having sacked Ai, did not proceed right away to conquer Bethel (The conquest of Bethel is reported in Jud. 1 as having taken place after Joshua's death).

To sum up: The efforts of about a dozen scholars-explorers during the nineteenth century produced *four* alternatives for the site of ancient Ai: Et-Tell/Tell el-Hajar, Khirbet Khudrya, Khirbet Haiyan and Khirbet el- Hai.[6] Arguments for each suggestion relied mainly on biblical geography and topography and to a lesser degree on toponymy and biblical language.

2. Interlude: Flinders Petrie and the birth of Palestinian (aka biblical) archaeology[7]

In 1890 Flinders Petrie, who had already gained a reputation in Egyptian archaeology, came to excavate in Palestine on the invitation of the Palestine Exploration Fund. Assuming (erroneously) that a mound named Tell el-Hesi was the site of the biblical Lachish, he and this assistant Frederic Jones Bliss excavated there in 1890, 1891 and 1892. Petrie was arguably the first to realize that such a mound consists of the successive vertical accumulation of living settlements, thus pioneering

[6] There is considerable variation in the transliteration of the names. Henceforth, I will use the versions common to most of the later publications.

[7] Sections A2 (here), A5 and A7 discuss inferential strategies used by Flinders Petrie, William Foxwell Albright and Judith Marquet-Krause, respectively, for estimating the age of sites and artifacts. These brief interludes focus on aspects that were relevant for the chronological determination of t-Tell and especially the diachronic evolution of the relevant capabilities. As such, they should be understood as partial "snapshots" from the history of archaeological typology. For comprehensive discussions of the methodology and philosophical aspects of archaeological typology see, e.g., Shepard (1956), Adams & Adams (1991), Wylie (2002, especially Ch. 2), Trigger (2006), and Chapman & Wylie (2016).

Middle-Eastern stratigraphy. The problem was how to determine the chronology of the site and the layers therein:

> The first difficulty that we meet is that are no coins and no inscriptions to serve to date any of the levels. How then can we read history in a place where there is not a single written document? How can we settle here what the date of anything is, if not a single date or name remains? (Petrie 1892: 5).

Petrie's strategy had four elements, three of them implicit and the fourth and crucial one implicit:[8]

- Looking for systematic and gradual chronological evolution in form, production technique, and style. This is the basic background assumption of typology, which Petrie perfected, using the methodology of *seriation*, during his work with artifacts from prehistoric tombs in Egypt, applied here in a different context:

  > We could recognize without doubt also the three great divisions: (1) the massive walls below (2) the deserted and barbarous state of the site (3) and the thinner walls above with very different pottery. (8)

- Making some gross local assumptions:

  > When I came to compare these dates with the range of pottery in the tell, it indicated that the accumulations were about five feet in a century … We are led to put the earliest foundation of the place to the beginning of the seventeenth century B.C. (7)

- Using previously dated artifacts from other countries to chronologically "anchor" the series of artifacts:

  > But how are we to proceed in where we know nothing as yet of the age of its products? It is like an inscription in an unknown language: … We have to wait for a bilingual tablet…. In the same way, we must wait till we find objects from other countries of known age, intermixed with those as yet unclassified.

[8] Sir William Matthew Flinders Petrie was a prolific writer. The fullest exposition of his methodology and philosophy can be found in "The aim and methods of archaeology" (1904) and "Seventy years in archaeology" (1932). I prefer to cite from his 1892 monograph because it is directly pertinent to my topic. See also the previous footnote about my aims and scope here.

Both existing and missing artifacts served as chronological "anchors":

> I found nothing as late as Alexander … Hence it is certain that we must place the whole history of the Tell before the fourth century B.C.
>
> A jar handle stamped with "The Palace of Ra-aa-x`heperu," namely, Amenhotep II, takes us back to the middle of the fifteenth century. (9)

- Using a historiographic framework for interpretation of the material remains, usually (in the case of Palestine) based on biblical texts:

> [The divisions above] correspond to the Amorite Age, The Judges, and the Jewish monarchy. (8)
>
> Thus there is a total of probably just under 40 years from the Exodus to the last Egyptian raid on Palestine… Tell el Hesy – the Lachish of the Amorites – was therefore capable of but little defence when the Israelites fell on it. (16)

The outcome of Petrie's work was twofold: A historiographic framework (the "Ages" mentioned above) and a typology – a classificatory system for Palestinian pottery that was expanded and published by his pupil, Frederick Bliss. The two (as he maintained several times in the short essay cited above) support each other because they cohere ("agree well") mutually and with other discoveries from the ancient Levant.

Petrie considered the typology his more significant achievement: "If I do nothing else, I shall at least have established a scale of pottery which will enable any future explorer to date all the tells and khirbets" (Davis 2004: 29). Indeed, his historiographical framework and the identification of Tell el-Hesi with ancient Lachish did not survive the test of time. But his methodological innovations — the stratigraphic approach to a *tell*, the research strategy of typology creation and his typological systematics — served as cornerstones for everything that was done later.

3. Early archaeological explorations 1900-1932: Sellin, Albright, and Garstang

In modern terminology, we would say that the answer to the question of Ai's location was strongly underdetermined by the combination of geographical,

topographical and toponymical (onomastic) data that was used throughout the nineteenth century. Notwithstanding the confident words of each writer in turn, the issue remained unresolved (A1).

It was Ernst Sellin who first pointed to a potential source of evidence that could solve the conundrum. Standing, literarily "with the book of Joshua in hand" near Deir-Diwan, the young theology professor from Vienna identified "with certainty the remains of two ancient cities" in Et-Tell and Khirbet Haiyan and was confident that "we have to discover ancient Ai in the one and ancient Beth-Aven (Josh 7:2) in the second" (Sellin 1900: 2-4). However, "Determining which is which *can hardly be achieved without excavations*" (My italics).

Sellin criticized Robinson's "uncharacteristically cursory observation" in Et-Tell, noting that the above-surface indication of ancient settlement should not be hewn stones but sherds, of which he saw a lot. This criticism may have been somewhat unfair, but it shows how much had been learned since the days of the first scholars-travelers.

Sellin later turned into archaeology himself,[9] but for some reason did not try to excavate either Et-Tell or Khirbet Haiyan. The region of Deir-Diwan was first explored archaeologically only more than twenty years later, when William Foxwell Albright, the new director of the American Schools of Oriental Studies, toured the area accompanied by students and colleagues, sampling sherds as they went.

Albright's conclusions concerning the location of Ai were summarized in a nine-page appendix (Albright 1924 Appendix V) to an extensive article about his excavations at Tell el-Fûl. In the type of intellectual *tour de force* that characterized much of his works, he discussed the scriptural texts (Hebrew origin and the Septuagint), the etymology of Arab place-names, the writing of previous scholars, and the local geography and concluded that "Beyond all doubt is the fact that et-Tell is the only

[9] He conducted (alone and with others) excavations in Ta'anach, Jericho, and Shechem.

tell [10] in the whole neighborhood, besides being by far the most suitable location, so far as the topographical indications of the Bible go." He conjectured Tell Haiyan to be the site of another Ai – the monarchic one mentioned as Ayat or Aya in Isa. 10:28 and I Chr.7:28 as well as the post-exilic Ai from Ezr. 2:28 and Neh. 7:32. This Israelite town, maintained Albright, cannot occupy the same place as the Canaanite Ai because "All the cries [sic] of Ai must be harmonized with the statement in Josh. 8:28 that Joshua destroyed Ai, and made it a mound (tell) for ever [sic], which can only mean that Ai remained a ruin from the conquest to the date of composition of Joshua, some centuries later."

The novelty in Albright's approach lay in his use of sherds collected on the surface as chronological evidence: "out of large quantities of potsherds examined [from Et-Tell] all but one or two percent were Canaanite… The few non-Canaanite sherds were… Arabic … Apparently no archaeologist has previously examined the site with a trained eye for potsherds. This examination will surely remove the last doubts as to the identity of Et-Tell with the biblical Ai."

So far, so good. But Albright's analysis of the sherds also led him to some unexpected conclusions. The relevant paragraphs are worth citing nearly in full, both because they are highly pertinent to what follows and because they portray Albright differently than what is often said of him (italics are mine):

> Having established the identity of et-Tell with Ai anew, let us turn our attention for a moment to some historical consequences to be deduced from the archaeological examination of the *tell.* As observed above, all the hundreds of potsherds inspected, with some insignificant Arab exceptions, were Canaanite. But, *almost to the consternation of the writer,* they proved to be, not Late Canaanite, but Middle Canaanite (Middle Bronze). All the sherds were hand-modeled, hardly a single wheel-made piece appearing … During the following Late Canaanite period, nearly all vases are wheel-made…

[10] That is, the only artificial mound.

> If Ai was destroyed during the invasion of Palestine by Israel, under Joshua's leadership, which we may date to about 1230 B.C. … one would have a right to expect Late Bronze pottery on the summit of the mound. …. But *all this stage, with its Phoenician and Cypriote sherds, its characteristic metal bowl rims, etc., is entirely lacking*. Instead it belongs unmistakably to the preceding Middle Bronze period (roughly 2000-1600 B. C.),... *apparently, therefore, we are forced to conclude that Ai was destroyed centuries before the invasion of Israel under Joshua,* between the seventeenth and the fifteenth centuries, presumably in the sixteenth.
>
> Let no one think that this conclusion is forced and premature; *the writer has, in company with members of the American School, combed the surface of the tell on more than one occasion, examining thousands of sherds* from all parts of the summit. (Albright 1924:146-7)

So, to reconcile his conclusions from the analysis of Et-Tell pottery with his favored chronology for the Joshua conquest (see A4 below), Albright hypothesized early, pre-Joshua, waves of Israelite conquests in Canaan. This theory he was to abandon later.

Garstang's sounding: Six years after Albright's field survey at Et-Tell, a sounding excavation was conducted there by John Garstang, the former director of the Palestinian Department of Antiquities (and a professor of archaeology at the University of Liverpool). A short editorial note in the bulletin of the British Palestine Exploration Fund (Jan. 1929) reports that the excavation "disclosed the walls of Ai, and it was reported that the city dated from the Early Bronze Age, and was totally destroyed in the Late Bronze Age." The note informed that "Professor Garstang was making a study of other sites mentioned in the books of Joshua and Judges … the main subject of his expedition is to endeavour to date the conquest of Canaan by Joshua."

Garstang's work in Et-Tell was brief. From Albright (1929a, 1929b) we learn that he stayed there only a few days and concentrated his soundings outside the city wall. "The shreds so far examined… carry us down to a period later than the end of Tell

Beit Mirsim D, but earlier than C, i.e., to about the fifteenth century B.C." which reinforced Albright's own dating. Except for a minor difference in terminology[11], then, the independent results of Albright and Garstang—unquestionably, the two leading figures in the Palestinian archaeology of the time—agreed.

Interestingly, Garstang never communicated the result of his work in Et-Tell in a professional publication, and later scholars who refer to them invariably cited his popular book "Joshua, Judge" (Garstang 1931). The book is a plea for the "early Exodus hypothesis" (see A4 below) —which Garstang held (as an increasingly isolated view) to his last days—presenting Garstang's excavations at Ai, Hazor, and (chiefly) Jericho as providing tangible support to this hypothesis (Garstang 1931:54).[12] The discussion of Ai (pp. 149-159) follows the nineteenth-century tradition of juxtaposing the biblical narrative with the local topography. A short appendix paragraph (pp. 355-6) contains the archaeological information. The city wall is identified as belonging to the Middle Bronze Age by its masonry style and by "a sherd of M.B.A 1 technique picked out of the bonding." As for the ceramics collected elsewhere on the site, "M.B.A wares were most abundant, there was found a considerable proportion of L.B.A 1 including … a Cypriote wish-bone handle, but nothing of Mykenaean [sic] date or character, nor any local fabrics of a date later than 1400 B.C."

To sum up, the consensus view in the early 1930s, based on the results of two prominent archaeologists, was that ancient Ai is buried under the mound of Et-Tell and that it was destroyed in the fifteen century B.C. This accorded well with one hypothesis regarding the chronology of early Israel (see below), and challenged the other. Due to the centrality of the Ai episode in the story of the conquest of Canaan by the Israelites, it was felt [13] that a full-scale excavation could help determine both

[11] The fifteenth century B.C is referred to as Middle Bronze in Albright 1934, and as Late Bronze here. This discrepancy in archaeological nomenclature is immaterial to our subject.

[12] All these conclusions were later revised by subsequent excavations.

[13] According to Vincent (1937:231) the idea was first proposed by Garstang.

the time and the character of the Israelites' conquest of Canaan and, by implication, the date of the Exodus and a large part of the biblical chronology as well.

4. Digression: When did the Exodus happen?

Starting with Albright's analysis (1924) cited above, estimating the time of the Israelite's exit from Egypt and their arrival in Canaan became central to all the deliberations on Ai. A short digression on this topic is, therefore, due here.

The task of fixing the historical dates of the biblical accounts has occupied orientalists, archaeologists, and biblical scholars since antiquity. It gained fresh impetus from the archaeological discoveries in the Levant during the late nineteenth century. Of particular interest were the timing of the Exodus and the Conquest of Canaan, on which there were two schools of thought: The "late" hypothesis placed the conquest in the final decades of the thirteenth century B.C., with the Exodus preceding it by about one generation. The "early" hypothesis posited that both events occurred during the fifteenth century B.C., the conquest taking place at its very end.

Initially, these two hypotheses were based almost exclusively on information derived from studies in ancient Egypt. Since no record of anything similar to the biblical story was found, both schools inferred from what they considered to be indirect evidence. The "late" hypothesis (Albright 1921:63) (a) pointed to the accordance of the name of the "storage cities" Pithom and Ramesses (Ex. 1:11) with sites from the reign of Ramesses II and III; (b) used inscriptions from Ramesses III mortuary temple as a basis for estimation of the appearance of the Philistines in Palestine[14]; and (c) considered the Merneptah's Stella (ca. 1210 B.C.) as providing *a terminus ad quem* for the conquest, with the Exodus about 40 years earlier. The "early" hypothesis (Garstang 1931:53-6) relied (a) on the similarities between names of Canaanite cities mentioned in the Book of Joshua and places named in the "Execration texts" from the Egyptian Middle Kingdom; (b) on reports (in the Tell – el Amarna letters) of

[14] Since the Philistines are mentioned in the book of Judges but not in Joshua, they should have arrived shortly after the Israelite conquest.

unrest in Canaan during the fourteenth century B.C. Canaan, identifying the "Habiru" mentioned therein with the Hebrew tribes; and (c and perhaps most importantly) was inspired by the figure of 480 years mentioned in I Kings 6:1 as the span between the Exodus and the construction of Solomon's temple which (as all agreed) must have taken place toward the end of the 10th century B.C.[15]

It is apparent from the above that Albright's conclusions from Et-Tell (that it is the site of biblical Ai and that it was destroyed in the sixteenth or fifteenth century B.C.) challenged his view that Joshua's conquest happened several hundred years later. His solution to this problem was based on the Wellhausenian conceptualization of the Bible which he held at the time (Albright 1921):

> ... the account of the conquest of Palestine in the Old Testament is highly schematized, and contains the record of events spread out in reality over centuries. We know that there were several Hebrew invasions: one under Abram ... in the seventeenth century; another of the Bene Yosef somewhat later, continuing down to the Amarnah Age; ... and finally the great invasions of Israel under Joshua, and Judah under Caleb, falling roughly about 1230 B.C. To which of these are we to ascribe the destruction of Jericho and Ai?

Albright concluded that Jericho, Ai, and Bethel were all captured and destroyed during the Bene-Yosef invasion, which "presumably occurred during the anarchic period between the break-up of the Hyksos Empire and the establishment of the Egyptian, that is between 1600 and 1550 B. C., say about three and a half centuries before Joshua."

Albright's arguments for this hypothesis should not concern us here, for he was to rescind it later (Albright 1934: 10, A9). His belief that the "main" conquest of Canaan by the Israelites took place during the last decades of the thirteenth century B.C.,

[15] The "early Exodus" hypothesis was more popular at the beginning of the twentieth century. It receded to a minority view during the following years, to large extent because of Albright's works. Neither of these hypotheses, nor any other one seeking to place the Exodus and the Conquest of Canaan in a realistic frame, is now consensual among scholars (cf. Wallach 2018, 2019a and citations therein).

however, was solidified by his later seminal excavations in Tell Beit Mirsim and at Bethel (Albright 1929a, 1934).

5. Interlude: Albright's inferential strategy

Albright's main research instrument was ceramic typology (he was later faulted for not paying adequate attention to stratigraphy, cf. Greenberg 1987; Dever 1993). It was the combination of his meticulous attention to minute details of sherds with his gargantuan knowledge of languages, history, archaeology and geography of the ancient Levant that made him the most influential figure in biblical archaeology for a couple of generations. His excavations at Tell el-Full (Albright 1924), Tell Beit-Mirsim (1929a) and at Bethel (1934) were, by all accounts, the major steps in biblical archaeology and ceramic typology of the time (Herr 2002). Basically, he followed Petrie's strategy of combining outside references, local (mainly sequential) considerations and historiographic hypotheses for both classification and interpretation of the pottery. Unlike Petrie, however, he was not starting from an almost-blank page but could draw on earlier results, both his own and others'. The following quotations give a taste of his methodology (italics mine):

- Drawing on parallels from other works:

  > The results of Phythian-Adams at Ashkelon, where the Philistine pottery appeared just above a burned level, which separated it from the stratum containing Cypro-Phoenician and Aegean sherds, have been fully confirmed elsewhere. … At Tell Beit Mirsim our results have been identical with the observations of Mackenzie… (1929a: 9)

- Using late results to refine prior ones:

  > Recapitulating the results, we find that the pottery from Gibeah I and II belongs with [sic] the end of Bliss's Pre-Israelite and the beginning of Macalister's Fourth Semitic; *the more exact dating possible for the first and second periods at Gibeah enables us to assign more exact dates … and thus marks a distinct step forward in the knowledge of Early Iron Age pottery* of the first phase. (1924: 16)

- Using pottery classification to create or support historiographic hypotheses:

> It is not difficult to fix the date of the close of period B. The pottery from the latter part of the period is identical with that of Megiddo IV, aside from certain local characteristics. The recent work of Guy and his brilliant discovery of the stable area, prove that this stratum is Solomonic. Tel el-Ful II, the period of Saul, [14] is distinctly earlier. The thorough destruction of the city cannot be dated in the reign of Solomon himself, but may be ascribed with absolute security to Shishak… (1934: 18).

- Attempting to weave diverse inferential strands into a coherent and converging conclusion:

> There are four principal lines of evidence for the date, and all agree to pointing us to the second half of the twelfth century B.C. (1924:48)

> With the general evidence of the pottery agrees that of the scarabs, since the two scarabs of Amenophis II from a tomb belonging to the latest Canaanite occupation of Jericho only point to cir. 1400 as a maximum date, while the allow considerably lower dates… (1939: 20)

The use of historiographic hypotheses to determine the chronology of pottery sequences, themselves created to establish historiographic hypotheses, raises, of course, a risk of circularity. The fact that Albright "to the consternation of the writer" deduced from sherds gathered on the mound of Et-Tell that the ancient town there could not have been destroyed at a period expected from his historiographic theory shows, however, that circularity is not inevitable. I shall return to the question of circularity in the next division (B2).

6. The Et-Tell excavations, 1932-1935

Influenced by the notion that the mound of Et-Tell may contain the clue to biblical chronology, Baron Edmund de Rothschild, a long-term supporter of both the humanities, the sciences,[16] and Zionism, offered to finance an excavation there

[16] He was, among other things, a member of the *Académie de Beaux-Arts* and a founder and benefactor of *Institut Henri Poincaré* for mathematics.

under the auspices of the Louvre Museum. Following a recommendation by the museum's curator of Near Eastern antiquities, Professor Ren*é* Dussaud, he chose a young archaeologist named Judith Krause to conduct the excavations.

A few biographical details are due here (H. Krause in Marquet-Krause through Dussaud 1949 pp. 5-6; Barag 1997), not only because of the centrality of Krause (later Marquet-Krause) and her excavations to our story. but also because they are relevant to the topic of the relationship between scholars' backgrounds and their beliefs, discussed in the next division (sections B5, B6). Judith Krause was born in Palestine in 1906 to parents that were deeply involved in the Zionist activities of the time. Her mother was a member of the Hankin family which played a central role in the *Yishuv*, and her father was (among other things) the director of the Sejera pioneers' training farm and later of the Mikve-Israel agricultural school. Markedly talented and hard-working, she finished the *Herzliya Hebrew Gymnasium* in Tel-Aviv with a good knowledge of French, English, Hebrew, and Arabic and went to study in Paris. There she drifted from ancient languages to archaeology and wrote a thesis on ancient buildings in the Middle East.

Krause was not only very young (25-26), and a sole female in the men's community of biblical archaeologist, when she was assigned to one of the most coveted opportunities in the Palestinian archaeology of the time. More significantly, she had almost no active experience in field archaeology. Her first step was, therefore, to enroll herself in Garstang's excavations in Jericho for one season in 1932.

In the same year, she made a short prospective tour to the site of Et-Tell. In a later (unpublished) reports, she wrote that already then she noticed "a strange amalgam of very archaic sherds and Israelite sherds, [of] at least one millennium later" (Marquet-Krause 1934: 8). The question of how could she, in what must have been a visit of several hours, detect what two very knowledgeable veterans could not, shall be discussed in the second division (B1).

The excavations proper started in September 1933, with a workforce of fifty men from the local population of Deir Diwan. The somewhat-older Jewish archaeologist

Shmuel Yeivin, "already familiar with the archaeology of the terrain," served as Krause's co-director, and both her parents helped in administrative tasks. The work had to be halted after six weeks because of "political agitation in the Muslim centers of Palestine" (1934: 9).

The findings from this short season, however, were significant. Strong stone walls, built on the bedrock and supported by massive earthwork, enclosed the ruins of an ancient city, with a necropolis outside it. An imposing building, constructed with unhewn stones, was excavated on the mound's summit, and Krause identified it as the city's palace and suspected that part of it enclosed a sanctuary. That city was destroyed and burned to the ground. Above a thick layer of debris and ashes stood simple walls of a later, unfortified settlement.

What made these findings not just significant but extraordinary was the chronology of the structures: Krause dated the city, with its walls and the necropolis, to the Early Bronze period. Hesitantly, she adds that "some [of the sherds) seem to be attributable to the Middle Bronze." As to the later unwalled settlement, she dated it with certainty to the Iron Age 1, with two successive phases of construction but without any stratification. (Marquet-Krause 1934; Marquet-Krause and Yeivin 1934; Marquet-Krause and Yeivin 1935).

There is no doubt that the young excavator realized the implications of her findings: If the walled city was destroyed in the Early Bronze Age (which ended 2,200 B.C.) and was never rebuilt, it could not have been conquered by Joshua and the Israelite tribes, irrespective of whether they entered Palestine in the fifteenth century (as assumed by the "early" hypothesis) or in the thirteenth century B.C. (as posited by the "late" one) (A4). Nor would Albright's conjecture of "prior early conquests" (A3) solve the problem because these were also assumed to happen during the Middle Bronze or the early Late Bronze period. Besides, the identification of an early Iron Age settlement—that is, within the period presumed to be covered by the Book of Judges—conflicted with the notion of Ai remaining "a heap of ruins, a desolate place to this day" (Josh. 8:28).

What was clear to the young archaeologist was also clear to others: Her co-director,Shmuel Yeivin, referred to the result as "surprising" (Yeivin 1937).[17] Her teacher Dussaud later wrote: "The consequences of this (inhabitation) hiatus, precisely at a time when one expects to find the most loaded story, were so serious – since they put the historicity of chapters VII and VIII of Joshua in doubt - that I suggested to her to publish nothing from the first season and to concentrate next year on a verification of this so unexpected result" (Marquet-Krause through Dussaud 1949:10). Marquet-Krause thanked Albright and pére Louis-Hughes Vincent from the Ėcole Biblique for "helping, in many discussions, to clarify many problems related to the excavation and its consequences" (Marquet-Krause and Yeivin 1934:30; cf. also Albright 1934:3 and Vincent 1937: 256), which means that they had an almost real-time access to the findings and must have been aware of their implications.[18]

Notwithstanding Dussaud's advice, Krause did publish a preliminary report on her first excavation season in two forms: One in Hebrew, written as a report to the editor of the Bulletin of the Jewish Palestine Exploration Society *Yediot* (Marquet-Krause and Yeivin 1934), and one in English, in the Quarterly of the Department of Antiques in Palestine (Marquet-Krause and Yeivin 1935). In both reports, the city is tentatively dated to the Early Bronze Age (though the existence of some sherds belonging to the Middle Bronze is mentioned as a possibility, to be explored later), and the unwalled settlement built above it to the early Iron Age. A longer report in French, which was not published at the time (Marquet-Krause 1934) and appeared only in the posthumous compendium of 1949 was more forthcoming about the

[17] Oddly, writing forty years later Yeivin said that "The present writer, however, knows that there were among the large quantity of sherds recovered from the last burnt phase of the occupation… [some] which should really be designated as 'Transitional period between EC and MC' … There was also … a single large sherd of buffish-green, medium sized, carinated bowl … which he considered at the time LC I, …." (Yeivin 1971: 51). Coming after Callaway's independent excavations (A10) that verified Marquet-Krause's chronological inference, this remark seems particularly strange and, for all I know, has never received any scholarly attention.

[18] Curiously, there is no mention of Garstang having visited the site or taking a look at the findings, though he was himself excavating in Jericho at the time.

discrepancy between her conclusions and those of "the illustrious researcher professor Garstang" but allows for the possibility of a revision by further analysis. Neither of these short reports alludes to the historiographic implications, but one may safely assume that they were apparent to most of those who read them, which must have heightened the expectations toward the next season.

The second season at Et-Tell, now directed solely by Marquet-Krause (newly married to the orientalist Yves Krause), began in June 1934 and lasted five months. The 32-pages preliminary report (Marquet-Krause 1935a) naturally adds many details. The analysis of the sanctuary, in which she recognized three stages, and of findings in tombs around the city contributed significantly to the understanding of the third millennium B.C in the Southern Levant.[19] In what concerns us here, the report is marked especially by its decisive conclusion (translation and italics mine):

> *The epoch of Middle- and Late Bronze are inexistent*. The sherds of Iron Age I are mixed with those of the Late Bronze … On the arrival of Joshua this city was, therefore, destroyed a long time ago. And this, undoubtedly, is the reason why the Israelites called it "Ha'Ai" – the ruin. We do not know what name it could have had in the 3rd millennium. However, the general description of this place that one finds in the Bible corresponds perfectly to the general situation of the Tell and of the ravine, which would allow an ambush like the one narrated in the book of Joshua. *This leads us to believe that chapter VII and VIII of Joshua, that could be assumed to be historical, are part of a legend, as is the account of the capture of Jericho, but a legend that is based on a real place."* (pp. 340-342)

These short and unambiguous sentences were, unfortunately, also the final ones Marquet-Krause got to say about the matter. She returned in November 1935 for a third excavations season with an increased workforce of 160 men but had to stop short when she became gravely ill. She returned to France for treatment and died a

[19] Albright's pupil Ernest Wright relied on Marquet-Krause's findings (together with archeological discoveries from Beth-Shan and other sites) in his definitive treatise on the typology, chronology and sub-division of the Early Bronze Age (Wright 1936, 1937).

few months later. Her truncated report for the third season (Marquet-Krause 1936) appeared only in the memorial compendium,[20] which included the notes and drawings prepared by the excavator and was published by Dussaud (Marquet-Krause through Dussaud 1949) in the hope that "they could be fruitful again in the hand of specialists of Palestinian archaeology." As we shall see (A10), this hope was indeed fulfilled, even if only in a small way.

7. Interlude: Marquet-Krause's inferential technique

Judith Marquet-Krause did not live to publish a complete professional account of her work at Et-Tell, but her reasoning is evident in the 32-pages *Rapport Sommaire* (Marquet-Krause 1935) published after the second season of excavation. Stratigraphy played some role in delineating sub-periods within the Early Bronze Age, especially in the sanctuary, and trivially in the sense that the Iron Age constructions were built above the ash layer of the Bronze Age. Apart from that, she relied solely on ceramic typology, and her main line of inference involved comparing her finds to pottery types that were excavated and dated elsewhere.

- As in the case of Petrie (A2) and Albright (A5), chronological "anchors" founded on external (Egyptian) sources were important:

  > Many cultic objects were distributed … picked up mainly from the second and the third Egyptian dynasty. (333)
  >
  > The flat-base dishes with flared walls … are exact replicas from the Thinite period (2nd dynasty) found in Sakkara. (333).

- These anchors, however, were no longer as essential as in the earlier decades. Most of the parallels cited by Marquet-Krause referred to local findings, including yet-unpublished ones from excavations that were running parallel to hers. Her base of comparison, therefore, was broader and more refined than that of her predecessors:

[20] This compendium had a troubled history: It was meant to be published in 1940. When the world war broke, the final proofs disappeared and the printing plates were melted for ammunition by the German occupation force. An early proof, however, miraculously survived, and formed the basis for the 1949 publication.

> A small votive bed... recently Mr. Rowe found a similar one in Gezer, within a tomb from the Early Bronze. (321)
>
> The ensemble is perfectly close to that of level IV in Megiddo, that was put [by the excavators there] at the beginning of the 3rd millennium, which accords with our independently constructed dating. (336)
>
> The ceramics from this epoch ... is represented, in particular, by big jars with pointed base... and a circular protrusion around their neck, and a quantity of cooking pots with two annular handles ... Thanks to the amiability of professor Albright we could compare these sherds with those from Bethel, that belong to Iron 1. (340).
>
> All the ceramics from the necropolis reached us in a rare state of conservation... One finds there ... many analogies with those from other Palestinian sites from the Early Bronze like Ophel, Baisan [Beit-Shean], Gezer, Megiddo, and Jericho... (343).

It was possible, for example, to diagnose that certain Egyptian-style artifacts found in one level were typical ("une réplique exact") to a much earlier period (1935:335, 1949:31). Instead of serving as a chronological "anchor," therefore, these unique findings were categorized as intrusive! (But See (B3)).

- Marquet-Krause was able to derive definitive conclusions not only from the presence but also from the absence of typological evidence.

  > The époques of Middle and Late *sont inexistentes*... This allows us to conclude that 800 years have passed between the violent destruction, by an unknown enemy, of the royal city near 2000 [B.C.] and its short resurrection around 1200. Upon the arrival of Joshua this city was, therefore, long destroyed. ...

The legitimacy of inferring from absence is the subject of (B5) (Cf. also Wallach 2019b). For the time being, let me note that the problems associated with such

inference did not give Marquet-Krause any pause. Neither did it concern her contemporaries, as the next section shows.

- A second level of inference then used her own findings to refine prior knowledge: The sequential levels of the sanctuary supported a sub-division of the Early Bronze age and enabled "to discern the evolution of the ceramic industry" along the third millennium B.C.

Her last, unfinished and unpublished article (Marquet-Kruse 1936, Marquet-Krause through Dussaud 1949:30) discussed the evolution of ledge handles, thus refining the typology of an important *fossile directeur* that was identified by Petrie forty years earlier. One imagines that much more would have come up had she been able to continue her work.

8. "A cause of some perplexity"

This section is about the initial reactions to Marquet-Krause's findings and her interpretation of these findings. To bring home the importance and urgency assigned to the issue at the time, I would like to begin with some remarks written in December 1935. The writer was no other than Judith Marquet-Krause herself. In a letter to Shmuel Klein—then the dean of Erez-Israel studies at the Hebrew University of Jerusalem, the chairman of the Jewish Palestine Exploration Society, and the editor of its periodical *Yediot*—she refers to an article (Market-Krause 1935b) that she had sent for publication there (my italics)[21]

> …As to the findings – rest assured that the conclusions were reached only after extensive debate. I have submitted, objectively and with a clear conscience, the results of my work over the course of three years, without the involvement of my personal emotions or of my attitudes or connections (Jewish national attitudes and connections) to the Bible.
>
> …. These conclusions, which I have detailed in my article, are drawn from the results of the excavations conducted to date. *As God is my witness, I hope to*

[21] Quoted (in Hebrew; English translation by D. Landman) in Elitzur 2014, itself a subject of (A12).

> *find evidence that corroborates the Biblical narrative in the upcoming excavations*.
>
> …However, if you decide that this position should not be publicized for religious or national reasons, I will take your view into consideration and will not object to concluding my article with a broad summary of the findings.

Klein replied briefly, expressing his belief that a proof for "a settlement that was there before the period of the conquest" will be found in the next excavation seasons. There is no sign that he made use of the excavator's extraordinary offer to modify her article prior to publication. But the fact that Marquet-Krause was motivated to write such a letter, as well as the editor's response, demonstrate how disturbing her results and the implications thereof were to people of [Jewish-] Zionist persuasions, herself included, at the time.

And not only to them: The "notes and news" section of the Quarterly Statement of the (British) Palestine Exploration Fund from April 1936, citing Dussaud's (1935, see A9 below) analysis of the excavation, reported that:

> The results of the recent excavations at Ai will cause some perplexity to those who pin their faith too simply to the spade. The eminent French archaeologist, M Dussaud , writes … "Mme. Marquet concludes that the site was desolate when the Israelites began the conquest of Canaan … These excavations … lead to the conclusion that the narratives describing the capture of Ai are no more historical than those which describe the crossing of the Jordan or the fall of Jericho."

This relatively nonchalant report was a cause for an uproar, as evident from the minutes of the annual general meeting of the Fund three months later (PEFQS July 1936: 123-131). A certain Mr. Edwards spoke to express

> .. on behalf of several of the members, our disapproval of the character of some of the notes that have appeared in the April *Quarterly Statement…* This Fund was started by Christian people and very largely for the purpose of

> demonstrating to the unbelieving world the truth of the story as recorded in the Bible. … On page 54 of the April *Quarterly Statement* we are told that some excavations made by some French people at Ai will cause some perplexity to those who pin their faith simply to the spade … I beg to protest very strongly against the tone of that article and hope it will not be repeated.

The editor of the Quarterly Statement, invited by the chairman to reply, opened by expressing deep sympathy with the protest "the more so that I was myself brought up in the atmosphere which he still so admirably represents" and informed the meeting that he offered his resignation if this was felt necessary to relieve the Fund of any embarrassment. As to the disturbing paragraph, it was only a quote of an argument made by a well-known French archaeologist, Professor Dussaud, printed to draw comments from knowledgeable people like Cannon Phythian-Adams[22] and Professor Garstang, and that the former has already responded.

Garstang himself was present at the meeting and was invited to comment, but what he had to say was disjointed: The whole thing, he suggested, is a misunderstanding, and he sympathizes with both Mr. Edwards and the editor; as to the excavation of Et-Tell itself, he heard "hearsay" that it uncovered only a small part of the mound. His dating for the site is supported by Albright.[23] There is some doubt whether the site is indeed the place of ancient Ai (though he himself believed it is.) His experience shows that once a site has been burned, it is liable to denudation… "So long as there remains a doubt, the evidence of the excavation of Ai is incomplete and it cannot be adduced in an argument either as to the conquest of Palestine or the date of the exodus."

The chairman (Sir Charles Marston) then closed the meeting with a few dismissive words about the "cursory excavation of what is alleged to be the site of Ai" and lauding Garstang's works and historiographic interpretations.

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[22] Like many other mentioned in these pages, Phythian-Adams was both an ordained reverend and an archaeologist. Among other things, he directed the excavations of ancient Ashkelon.

[23] Actually Albright by that time had already retracted his own estimate (see below A9), a fact that Garstang should have been aware of.

The "solution" of Phythian-Adams (1936), mentioned above, was to sacrifice the historicity of the Ai story, conjecturing it to be a monarchic-time addition created in the context of the adversity between the kingdoms of Judea and Israel. At the same time, he defended in strong words the general factuality of the conquest scenario, the "early" hypothesis for its date (A4), and Garstang's work in Jericho. Except for a short rebuttal by Vincent (1937, see below), Phythian-Adams' suggestions were mostly ignored. The reverberations of the "Ai debate," however, continued.

9. The scholarly debate, 1934 - 1960

The first comment in print on the Et-Tell excavations (Albright 1934) appeared just days after Marquet-Krause finished the second season and before she published her report. The occasion was Albright's article on his excavations at Bethel, where the most important finding, in his view, was a destruction layer dated to the thirteenth century B.C. (that is, to the end of the Late Bronze Age) followed by a simple Iron Age settlement. For him (and, through him, for at least two generations of archaeologist) these findings were pivotal:

> We are compelled to identify it with the Israelite conquest. In reaching this obvious and inescapable conclusion, the writer abandons a position which he has held for eleven years, and adopts the low date of the Israelite conquest of central Palestine. In connection with this change of position, he also abandons Garstang's view of the date of the Israelite capture of Jericho. (p. 10).

How does this square with the findings from Et-Tell? Having followed Marquet-Krause's excavations closely (p. 3), Albright endorsed her results as well as her interpretations.[24] To the challenge posed by Marquet-Krause results to the biblical conquest narrative Albright had a creative solution:

[24] About his own erroneous dating of the site (Albright 1924: 146) he had the following to say: "Both the writer and Professor Garstang were victims of a curious situation at et-Tell (Ai) … The 'Middle Bronze' which we found there on the surface and in a trench outside the city wall was Early Bronze ware of a type which was then assigned to MB I. The 'early Late Bronze' ware which we found was nearly all composed of cooking pot rims of a type which begins in the fifteenth century and continues

> We may also observe that the vicissitudes of Ai and Bethel cannot be separated; the two towns are so close together that only one could have any importance… in a given period. The name *Ha-'ai* means simply "the ruin" *(par excellence)* in Hebrew. Bethel fell into the hands of the Israelites, who burned it to the ground, somewhere in the thirteenth century. In tradition, since Ai was the precursor of Bethel, and was also destroyed by a foe who burned it to the ground, some eight centuries before, the former replaced the latter. (1934:11. See also Albright 1935:15).

Essentially, Albright suggested that the Ai story in the book of Joshua is etiological—that is, it was created to explain the origin of prominent ruins. This part of his conjecture is not different from Marquet-Krause's "a legend based on a real site" and was shared by many. Albright's idea that the "true" story referred to Bethel, however, is open to many questions[25] and was not embraced even by scholars like Vincent (1937: 260, see 9.2 below) and Yadin (1982), who defended the general historicity of the conquest narrative.

Dussaud was the first out point to the ad-hoc character of Albright's "solution" (Dussaud 1935:351 n2): Why apply this logic, he asked, only to the story of Ai Josh. 7-8 and not to the story of the conquest of Jericho in chapter 6, the legendary character of which is so apparent and could not be supported archaeologically? As an alternative, he surmised that the story of the conquest of Ai was created to

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for some six centuries with very little change. By a strange coincidence, the pieces we saw belonged to the simplest form of the type in question, a form which might in itself very well date from the fifteenth century, but actually belongs to the twelfth! This experience is again a warning against over-confidence in dating from isolated surface finds."

It should be remembered that Albright (1924:147) mentioned thousands of sherds collected and analyzed (A3), so "isolated" here cannot be taken too literarily. But a lot changed between 1924 and 1933, as discussed (B1) and (B2).

[25] Among other things, the Bible (Jud. 1) has a very different story about the capture of Bethel. One notes also that the very proximity between the sites was formerly used by Albright (1924:148, (A3)) as an argument for their synchronous conquest.

provide a ritual legitimation to the possession of the place. In a later article (Dussaud 1937) he combined Marquet-Krause's results from Et-Tel with Albright's results from Bethel and a lot of textual analysis to hypothesize about the extinct name of the third-millennium city, before the Israelites named it "the ruin." It was, he surmised, originally called Beth-Hadad in honor of a Canaanite god, derogatively changed to Beth-Aven by biblical scribes, and later etiologically named as "Ha'ai that is by Beth-Aven."

9.1 Noth's challenge: A seminal contribution to the debate was made by the German biblical scholar Marin Noth. Although Dussaud, Albright, Garstang, and Phythian-Adams differed in their interpretations of the archaeological results from Ai and in their reconstruction of the conquest of Canaan by the Israelites, they (and many others) did not doubt that such a conquest did take place. Noth, following his teacher Albrecht Alt, advanced a radically different opinion that was later called "the Peaceful Immigration hypothesis." This account interpreted the books of Joshua and Judges as tribal traditions, independently and locally created and amalgamated to a national lore by later editors. Most of the "conquest" chapters in Joshua, for example, are confined to the allotment of the tribe of Benjamin.

This interpretation was deduced from analysis and critique of the biblical texts. Noth's writings, however, show a profound and up-to-date acquaintance with the archaeological publications. The results from Et-Tell played an important part in a series of articles (Noth 1935, 1938a, 1938b) that he published following the excavation there.

The contradiction between the seemingly-realistic biblical story in Josh. 7-8 and the archaeological results, asserted Noth, disappears when one recognizes the story as an etiological one. And the same holds for most of the conquest stories, especially those that include the phrase "until this day."[26] Not only that, but one can deduce from the text who were the original creators of this story: The detailed topographical

[26] But not, for example, the story in Jud. 1 about the conquest of Bethel which does not contain such a phrase, a point that Noth counts against Albright's conjecture that the story was transferred between the two sites (1935:29f).

knowledge that this narrative reflects could only come from the local (presumably Benjaminite) population who settled in the place as part of a local, tribal *Landnahme* and named it Ha'ai - "the ruin." The archaeological results, in *ein überaus lehrreiches Beispiel* (1938a:14) for independent confirmation, testify both to the very early destruction of the city and the existence of a local, Israelite population on the place later.

9.2 A multitude of conquest scenarios: Noth's analysis presented a challenge, to which everybody who discussed the matter afterward felt obliged to react.

The first systematic response came from Father Vincent (1937) the doyen of Biblical archeologists at the time (and the one who introduced Albright to archaeology). In an article dedicated to the memory of the late Marquet-Krause, which presented her results in a much more detailed form than she herself had the opportunity to, Vincent rebuked the participants of the 1936 annual meeting of the Palestine Exploration Fund (A8) for their dismissive attitude to the excavator and her work. He also rejected the suggestions of Albright and Phythian-Adams as artificial (because they attempted to salvage the biblical narrative while at the same time modifying it deliberately) and offered a solution of his own, to be mentioned shortly. But it was Noth's suggestion that concerned him the most since it led, "on the basis of a literary critique alone," to a negation *pure et simple* of the historicity of the biblical texts. Whatever historicity Noth was willing to allow (like the possibility of local, tribal campaigns) was, in Vincent's opinion, not worth retaining.

The more so, because there exists, in Vincent's view, a simple and entirely convincing explanation for what happened at Et-Tell/Ai. Since these ruins, especially those of the walls and the ramparts, are so impressive even today, it is "believable in itself and in perfect concordance with the known local and temporal conditions" (p. 263) to assume that some Canaanite group, probably from adjacent Bethel, used it as improvised fortifications against the approaching Israelite invasion. The attackers, having just come from the desert and being inexperienced in the art of siege, might have initially considered the place as easy prey, but after a first painful defeat came to regard it as a mighty city to be conquered by stratagem. That a supernatural

element (Achan and the stolen spoil, Joshua's divination, etc.) was added to this entirely realistic episode is attributable to the "essentially religious character of the Bible."

In a later series of articles, Albright (1935; 1939) set out to defend the historicity of the conquest episodes against "the nihilistic attitude of Professor Noth" and at the same time establish their timing in the thirteenth century B.C. against "the apodictic statements of Professor Garstang, Sir Charles Marston and others, all standing for considerable higher [i.e., earlier] dates." A description of Albright's detailed argumentation, centered on a discussion of the archaeological excavations in Megiddo, Beth-Shan and (especially) Lachish,[27] as well as his own excavations in Tell Beit Mirsim and Bethel, will carry us far beyond our subject here. In what concerns Ai, he recognized that "the case of Ai has undoubtedly been responsible for a marked tendency in certain quarters to depreciate the historical content of the narratives of Joshua" (1939: 15) and reasserted his 1934 hypothesis, namely that a real thirteenth-century Bethel conquest was transformed to a narrative about a conquest of Ai. And "what was more natural than that this tradition, current for many generations among the Israelite inhabitants of Bethel, should have been attached to the impressive Canaanite ruins of Et-Tell, whose destruction actually preceded the foundation of Bethel?"

In the next quarter-century, Albright's views about the historicity of Joshua's conquest and its timing became "the received view" for most archaeologists and many biblical scholars. It was held, in particular, that excavations in the Tel-Beith-Mirsim, Bethel, and Lachish (plus Yadin's excavations in Hazor in the 1950s) supported and confirmed these views. Albright's "Bethel–Ai replacement hypothesis," on the other hand, was generally considered speculative or just untenable (Yeivin 1937; Callaway 1968; Yadin 1982). The case of Ai/ Et-Tell became an "anomaly" in the Kuhnian sense, a single "outlier" (joined, according to some, by

[27] Albright's argument relied heavily on the findings of the British excavations of Lachish, but at the same time insisted that their dates for the destruction of the site are too high (early). I return to this point in (B2), which discusses the problem of circularity.

another one from Jericho) to a generally successful theory. An anomaly to be explained away, usually as an etiological story created sometime in the Iron Age.

Before continuing the next scenes in "the Ai story" let me summarize the main similarities and dissimilarities in the views mentioned in this section, as a preparation for the next one and for issues that shall be discussed in division B:

1) All accepted that Et-Tell is the only site that fits the biblical descriptions of Ai and its conquest scenario.
2) All accepted Marquet-Krause's inference that the site was only inhabited in the Early Bronze Age and again for a short period in the early Iron Age.
3) All—*including* the Alt/Noth school—held that the books of Joshua and Judges, and in particular the conquest chapters (Jos 1-9 and Jud. 1), contain *some* historical content, and that light can be shed on this content by textual analysis, geographical considerations and archaeological discoveries.[28]
4) And last but not least, all the scholars cited in this section, *including* "conservatives" such as Garstang and Albright and ordained priests such as Vincent and Phythian-Adams (and, later, Callaway and De-Vaux), – held that the biblical texts are not *literarily* accurate, but are the outcome of alterations, omissions and additions made by human scribes, and that their content reflects the knowledge, perceptions, and motivations of these scribes. This meta-assumption allowed a wide degree of freedom by selecting textual evidence for one's view, denying the evidential relevance of other passages, and interpreting a text in a manner different from its literal meaning. Specifically, all but Vincent[29] agreed that the excavation results from Et-Tel-Ai could not be reconciled with the biblical narrative in Jos. 7-8, and opted to consider this chapter as an etiological story, in one version or another.

[28] The Alt / Noth school, of course, offered a different historiographical interpretation of the conquest chapters, namely as episodes in a long settlement process rather than a rapid conquest campaign. Prior to the works of Yohanan Aharoni in 1950s, scholars of this school also tended to assign lesser import to archaeological discoveries vis-à-vis the textual analysis. But they did not maintain a "nihilistic" attitude to either the archaeological results or the biblical narrative.

[29] And even Vincent, as noted above, posited a conquest story different from the biblical narrative.

The first attempt to challenge this set of beliefs came in the 1960s (Grintz 1961, 1971), and it was directed toward points (1) (the identification of Ai with Et-Tell) with, as its background, reservations about point (4) (the non-literal and selective interpretation of the biblical texts). Because Grintz's position is related to works that were published in later years, I defer its discussion to section A12 below. Point (3) (that the relevant biblical chapters point to some historical realities, which can be at least partially deciphered) was strongly challenged since the last quarter of the twentieth century, (A11).

The next section describes a systematic attempt to test both point (1) (Ai=Et-tell) and (2) (Marque-Krause's results) archaeologically.

10. The spade returns to Et-Tell, 1964-1972

Joseph Callaway, a faculty member of the Southern Baptist Theological Seminary, excavated Et-Tell intermittently between 1964 and 1972 with the express purpose "to carry forward the work of Mme. Marquet-Krause."

The 1964 excavation (Callaway et al. 1965) verified Marquet-Krause's chronological scheme: A fortified city during the Early Bronze Age (he made a small correction to its chronology) and an Iron Age village, with nothing in the intervening period. The excavated area was expanded beyond what Marquet-Krause dug, and this by itself put to rest Garstang's conjecture (A8) that the missing "Joshua's city" is hidden somewhere under the mound.

In the Iron Age village Callaway, like Marquet-Krause before him, discerned two habitation phases, which he dated tentatively to 1200-1050 and 1050-1000 B.C, the second being "a little more than a camping phase." In neither phase was there any evidence of belligerencies; rather, "occupation seems to have been interrupted by periodic abandonment, not violent destruction." Callaway also noted that the Iron-Age settlement contained many cisterns and speculated that "an extended drought could have forced the abandonment of the village."

Besides the work at Et-Tell, Callaway also conducted in 1964 an exploratory excavation at Khirbet Haiyan, suggested by Robinson as the site of the biblical Ai (Callaway and Nicol 1966). A sounding down to the bedrock found evidence to Moslem, Byzantine and probably Roman occupation, but nothing earlier. Therefore, he crossed out Khirbet Haiyan as "not likely to yield evidence of a biblical site dating to the post-exilic period or earlier."

Callaway returned to excavate at Et-Tell and its vicinity in 1966, 1968, and again in 1968-1972 (Callaway 1968, 1969, 1976; Callaway et al. 1969). His motivation, openly stated, was not only to expand on Marquet-Krause discoveries but also to seek some reconciliation between these discoveries and the biblical narrative: "Ai is simply an embarrassment to every view of the conquest that takes the biblical and archaeological evidence seriously… I am persuaded that there is too much historical evidence to call the conquest of Ai legend or etiology**. …** Noth's view that the capture of Ai is etiological legend is extreme, and I cannot accept it…" (1968).

In the following years Callaway, in parallel to his excavations at Et-Tell (and at Khirbet Raddana, which shall not be discussed here), checked and eliminated all the other Ai-alternatives (A1). Khirbet Khudrya was found to be the remains of a Byzantine monastery, Khirbet Hay - Mamlukian, and "there is no Late Bronze evidence in the region east of Bethel that I can find." (1968: 315)

At the site of Et-Tell itself, Callaway's excavations did not produce anything substantially different from what was found before. But he did come up with an entirely novel theory (1976:30): Ai was indeed conquered by the Israelites, but not in the thirteenth or the fifteenth century, but in the late twelfth century B.C! What was conquered was not a mighty Canaanite city, but a small Iron-Age village, populated by earlier invaders, possibly Hivites, that settled on the debris of the Early Bronze citadel at the end of the Late Bronze Age.

In short, Callaway suggested that the conquest mentioned in the Bible occured between the two Iron-Age phases at Et-Tell. Importantly, he saw this idea not as

a localized solution but a general one, pointing to what he considered as similar findings from other sites in Palestine: "The emerging picture of the conquest that I see in the archaeological evidence is like the Phase I settlement at Ai... This, I admit, is a less glorious picture of the conquest than Albright's Late Bronze dating implies or the biblical traditioners insist. But to me, it is more realistic and credible, and it actually has more support from the biblical traditions."

Callaway's historiographic reasoning here is somewhat circular: Because (already in that time) several sites that should have been conquered by Joshua showed Iron-Age occupation but no traces of a Late Bronze Age one, and because he assumed that *some* Israelite invasion must have taken place, he concluded that disruptions in these Iron-Age settlements testify to this invasion.

As for archaeological evidence for the hypothesis from the Et-Tell site, Callaway presented two, one of which he seemed to retract later: First, contrary to his 1965 article, his 1968 one reported (p. 320) signs of burning in one of the buildings of the first Iron Age phase. He did not repeat this claim in any of his following publications and completely rescinded it in an article he wrote for NEAEHL (Callaway 1993). Secondly, and more central to his argument, he noted that the settlers of the first Iron Age possessed knowledge in masonry (such as chiseling stone pillars) and agriculture (building cisterns) unexpected in "people newly arrived from the Transjordan desert" (1969:59). The second occupation phase, on the other hand, seemed to him less developed, with above-ground granaries replacing built storage facilities (1976:30) so that "If there is evidence of an Israelite population at Ai with a nomadic background reflected in the biblical traditions and affirmed by the fellow-travelers and continuers of both Alt and Albright" this should be it.

Callaway was certainly right to point out that both "early Israel" hypotheses (the Conquest and the Immigration) current at the time presupposed a movement of nomadic tribes into western Palestine, and also in his observation that the material culture of the Iron Age village at Et-Tell shows signs of a sedentary,

agriculturalist background.[30] Apparently, he was not prepared to consider any hypothesis that *did not* include such a movement.[31]

Crucial to Callaway's hypothesis is the dating of the Iron Age phases. Like Marquet-Krause (1935:340) before him, he dated both to the *early* Iron Age. He supported this estimation by comparing the pottery types that he found to those from other sites (1968: 317; Callaway et al. 1969:7-9) and by pointing out that "only one piece of pottery that is possibly burnished has been found in two seasons of work, and it is not clearly burnished. Burnished pottery is typically present in the latter part of Iron Age I at other sites…" (Callaway et al. 1969:9). He opined that "This last characteristic tends to push back the Iron Age I occupation at Ai to the earliest possible date, conceivably before some of the last Late Bronze cities fell" (1968:7).

Callaway's "Hivite" hypothesis failed to convince anybody and was criticized by quite a few. De Vaux (1969: 273-4) was quick to point out that it suffers from three problems: (1) a fragile archaeological support (there is no evidence for a violent conquest of Iron-Age Et-Tell); (2) a speculative assumption of a "Hivite" migration into Late-Bronze Palestine, to which there is no support whatsoever; and (3) even if Callaway's scenario is accepted, it does not salvage the historicity of Josh. 7-8, which tells a very different story. Zevit (1983:32), who relied heavily on Callaway's reports to reaffirm that Et-Tell is indeed the site of ancient Ai, rejected his hypothesis for similar reasons, adding that the ceramics from *both* Iron-Age phases looked "Israelite", that is, similar to those found in any other hill site in Iron Age I.

An even sharper criticism, directed at the crucial point of the date of the Iron-Age village, was made by Finkelstein (2007). Because Callaway never authored a full publication of the remains from the Iron Age at the site (only of the Early Bronze

[30] This is supported, for example, by an analysis of the results from his excavations at Khirbet Raddana, in the vicinity of Et-Tell (Lederman1999).

[31] Such as George Mendenhall's "Revolt hypothesis" (cf. e.g., Mendenhall 1962), of which he must have been aware, since he cites Mendenhall in support of his "Hivite" conjecture (1968:368).

ones), Finkelstein turned to Marquet-Krause's sketches and plates that appeared in the 1947 posthumous compendium (A6). By comparing these to sherds and vessels from other sites such as Shiloh, Megiddo, Aphek, Giloh and Izbet-Sarta (pp. 108-110), he concluded that the Iron-Age settlement at Et-Tell existed distinctly later than stated by Callaway, beginning at the eleventh and terminating in the tenth century. Most of the sites that Finkelstein uses for comparison were excavated and published after Callaway left Et-Tell, but in Finkelstein's opinion Callaway could and should have come to much the same conclusions based on assemblages from Megiddo and other sites, which were available to him, and his interpretation was motivated by "a highly conservative reading of the biblical texts."

Callaway himself seemed to vacillate about his theory. He withdrew it totally in 1985, raised it again, though in a hypothetical tone, in 1992 (p. 130), and did not mention it at all in his encyclopedia article about Ai (1993). By that time both the knowledge base and the hypotheses landscape have considerably changed.

11. The subsidence of the Ai debate, 1970-2014

Callaway's attempt to reconcile the biblical narrative with the "embarrassment" of Et-Tell was quickly becoming irrelevant in the eyes of most scholars of the relevant disciplines.

Starting with Yohanan Aharoni in the mid-1950s,[32] some archaeologists and biblical scholars adopted the Peaceful Immigration hypothesis advanced by Albrecht Alt and Martin Noth (A 9.1). In this account, material evidence for the establishment of the Israelites in the Land of Canaan was to be found not in the destruction layers of Canaanite cities but rather in the simple Iron-Age villages in the hilly regions. Viewed in this light the situation at Ai/Et-Tell was not an anomaly, but the rule.

[32] I say more about the "emergence of Israel" hypotheses and their interplay with the archaeological discoveries in Wallach 2018 and Wallach 2019. For more details see, e.g., Mazazr 1990; Finkelstein 1998; Morrey 2004; Dever 2011.

Even archaeologists who defended some form of a conquest hypothesis (e.g., Yadin 1982) acquiesced to the Ai story being an etiological legend. This seemed inevitable since, as Zevit (1983) observed, any candidate for ancient Ai should produce evidence of a Bronze Age city and (assuming identity with pre- and post-exilic cities with similar names) an Iron-Age settlement, and in the vicinity of Bethel only Et-Tell qualifies. The excellent fit between the geography and topography of Et-Tell and the narrative details supports, rather than weakens, its identification as etiological story, told first "by and for people intimately familiar with the realia and topography of the area" (Zevit 1983: 32) and incorporated much later, through a process of theological interpretation, into the national saga.

During the 1970s, as additional discrepancies between the biblical narrative and the archaeological finds were piling up, the Peaceful Immigration hypothesis became a mainstream consensus.

Another shift of opinion happened during the 1980s and the 1990s. Results from more and more excavations and wide-areas surveys made it apparent that the material culture of the simple Iron Age settlements, in Et-Tell as well as elsewhere in western Palestine, was local, without any sign of foreign origin. This and other issues put a strain on the Peaceful Immigration hypothesis, and most scholars in the relevant disciplines now hold that the inhabitants of these settlements, which later coalesced to the population of the kingdoms of Israel and Judea, was autochthonic. Under this hypothesis, too, the ruins at Et-Tell present no special challenge and a search for archeological evidence for the dates of the Exodus and the Conquest, which motivated the excavations at Et-Tell, is pointless. Unsurprisingly, "the problem of Ai" is no longer part of the mainstream scholarly discourse. It continues, however, to occupy some groups of dedicated scholars, as discussed in the next section.

12. Where is Ay (again)? The uncompromising savants

Several scholars rejected the mainstream opinions described above, offering radically different hypotheses about the biblical Ai and calling into question widely

held background assumptions. This section outlines two of the more prominent versions of this thinking.

The hypotheses discussed below differ in several respects, but they have two things in common: Both embrace an "early" hypothesis for the time of the Exodus and the conquest of Canaan by the Israelite (A4), and both reject the identification of Et-Tell with the biblical Ai. In these respects, therefore, they re-open issues that were assumed to have been satisfactorily answered.

The first hypothesis was formulated by Professor (from Tel-Aviv University) Joshua Meir Grintz (1947, 1961) and repeated by Yoel Elitzur (1980, 2014). The reasoning in all these articles is identical (except for one small but significant difference, mentioned below), so I shall discuss them using Grintz 1961, which is the only one published in English.

After an elegant review of the prior deliberations about the location of Biblical Ai (A1, A3) Grintz criticises and rejects all the arguments that led people to identify it with Et-Tell: (1) the geographical and topographical correlation with the biblical descriptions is "possible but not binding" – other locations in the vicinity that were suggested as alternatives have a similar or even better correlation; (2) the toponymic/onomastic argument is simply false: He points to several locations in modern Palestine that bear the Arabic name "Et-Tell" in (208) and to several different places named "Ai" in the Bible (211). Besides, "It is … abundantly clear that the word Ai does not serve as the designation of a ruin… it means pile, a heap of stones…."[33]; and (3) as for the archaeological results "The contradiction is really a complete one, being between the excavations and the Bible as a whole."

The last point is the crucial one: Grintz postulated biblical Ai to be not only the place of Abrahams's altar (Gen 12:8) and Joshua's battle (Josh. 7-8), but also of pre-exilic

[33] Zevit (1983) accepted Grintz position against the onomastic identification "Ai = Et-Tell", adding some arguments of his own. He rejected, however. Grintz' geographical analysis and insisted on the identification of Et-Tell with the biblical Ai as the only possible one.

places with the similar names: Aiyah (1 Chr 7:20) and Aiyath (Ish. 10:20), and the post-exilic Ai (Ez. 2: 28, Neh. 11:31). Other scholars (though not all of them) have made similar identifications. Grintz, however, combined this list of biblical references with the early-exodus chronological framework, which he endorsed, to derive a surprising result. If indeed the Patriarchs lived in the eighteenth and the conquest of Canaan occurred in the fourteenth or fifteenth century B.C. then it turns out that "upon the destruction of Et-Tell, Ai was settled... and upon the destruction of Ai, Et-Tell was settled" (207). The archaeological evidence from Et-Tell, therefore, rather than challenging the biblical narrative, proves that Et-Tell *cannot be* the site of biblical Ai!

Similar considerations enabled Grintz to suggest that Et-Tell is, in fact, the location of biblical Beth-Aven. As for the location of ancient Ai, both Grintz (216) and Elitzur suggested that it must be somewhere close to Et-Tell (given that Ai is "near Bet-Aven," Josh 7:2). But here there is a difference: Grintz pointed to Khirbet Haiyan as an interesting, though yet unconfirmed possibility that should be explored by excavation. Elitzur (2014:29), speaking after Callaway's excavations which ruled out the place as well as all previously suggested Ai-alternatives, said more obliquely that "it is likely that the ruins of Ai can simply be found in a section of et-Tell not yet excavated, perhaps buried beneath part of the adjacent town of Deir Dibwan." Recognizing that the site had been surveyed before, he hastens to add: "It is critical to stress that the mere lack of archaeological evidence for this claim is no proof of its illegitimacy. Many important sites appear completely innocuous … before they are excavated."

A different and more radical hypothesis was offered by David Livingston (1970, 1989, 1994, 2003) and John Bimson (Bimson and Livingston 1987).

The Livingston - Bimson proposal consists of (1) rejecting the identification of Bethel with Beitin, proposing the town El-Bireh instead as the site of biblical Bethel; (2) Identifying Khirbet Nisya, a small site about one mile southeast of El-Bireh, as the likely place for biblical Ai (see map in Fig.2); (3) adopting an early (fifteenth century B.C.) date for the Exodus and the time of entry of Israel into Canaan; (4) suggesting

that the date for the end of the period designate by archaeoogists as the Middle Bronze II should be moved by over a century, from around 1550 B.C. to around 1420 B.C. (The last idea, arguably the most audacious one, is only alluded to in Livingston 1970 [p. 44] but is fully developed in Bimson and Livingston [1987: 8-9]).

Discussing these ideas in full, and the replies to them (e.g., in Halpern 1987) would take me far beyond the scope of this work. What is important to understand is that the elements of the hypothesis are intertwined. The fifteenth-century Exodus scenario, combined with the suggestion to lower the date of Middle Bronze II by a century, enables Bimson and Livingston to show that Canaanite cities mentioned in the book of Joshua (e.g., Arad and Horma) were inhabited before the conquest and destroyed after it. As for Bethel, remember that it was the destruction layer in Beitin that sealed for Albright (and many that followed him) a thirteenth-century B.C. time-frame for the conquest (A9). But if it can be demonstrated that Beitin is not, in fact, the site of biblical Bethel, this argument is called into question.

Livingston notes that the challenge posed by Ai to their preferred historiography is particularly grave: "There is textual evidence for the burning of only three cities by Joshua: Jericho (Josh. 6:24), Ai (Josh. 8:28), and Hazor (Josh. 11:13)." Jericho and Hazor have evidence for a Middle-Bronze conquest (Hazor also for a Late-Bronze one), so "The only exception seems to be Ai. … But we believe we have a solution to this anomaly. …. In our view, the site of Ai has been misidentified." (Bimson and Livingston 1987:10).

To move Ai, while adhering to the biblical descriptions, one needs to move Bethel. Livingston offers a potpourri of arguments why ancient Bethel could not have been in Beitin: The dimensions of the site do not fit for what should have been a border town between the tribes of Ephraim and Benjamin; a place that is mentioned so many times in the Bible should have been "a living town," but Arab Beitin is "a dead town today"; the failure to find a vestige of Jeroboam's idolatry temple is "evidence from silence" against Beitin; the linguistic etymology Beitin = Beitil = Bethel is not valid, etc. Livingston's main argument, however, concerns the interpretation of the relevant passages in the fourth-century A.D books by Eusebius and Jerome (A1).

Both report that Bethel is located about twelve Roman miles from Ailia (Jerusalem), on the route to Neapolis (Shechem). By Livingston's reckoning, Beitin does not fit this specification (it is too far) while El-Bireh does—*provided* one measures the distance from the Muristan square inside the Old City of Jerusalem and not, as commonly thought, from the pillar that stood inside the present-day Damascus Gate. Assuming the identification of El-Bireh with ancient Bethel, Bimson and Livingston offer arguments why Khirbet Nisya is, topographically speaking, an ideal candidate for the site of ancient Ai (Livingston 2003. cf. also the map in Fig.2).

These arguments were discussed and rejected, one by one, by Rainey (1971, 1980, 1988; cf. also Rainey 2006). He showed, for example, that when Eusebius mentioned distances to locales along the Jerusalem-Neapolis road, he usually reported them to the turnoffs from the main road (and therefore his description fits Beitin), and that these distances were measured from the city center (making a fit to El-Bireh impossible). Livingston's other arguments are similarly misleading. In short: "If Bethel is not Beitin, then there is no historical geography in the Bible." (Rainy 1988). Himself a graduate of a theological seminary like Livingston and Bimson, Rainy sympathized with their desire to find "a solution to the problem of Ai" (1971:188), but he would have none of what, in his view, "could hardly be reckoned as a serious scholarship" (1980:251). Livingston and Bimson, however, remained unconvinced.

Livingston conducted about a dozen excavations seasons at Khirbet Nisya (excavating at the densely built El-Bireh is out of the question). He reported the results in a book (Livingston 2003. For all I know, they were not published in a peer-reviewed archaeological journal). The main result is that

> Excavations at Khirbet Nisya have shown that the site was occupied during the biblical periods when Ai was in existence. Periods of significant occupation, determined by ceramics, artifacts, and architectural evidence are: Middle Bronze II, Late Bronze I, some Late Bronze lIB, Iron Age I and II, Persian, Hellenistic, Early Roman, Byzantine and Early Islamic (Livingston 2003: 213).

No traces of architecture from the Middle and Late Bronze Age or the early Iron Age were found, a fact that Livingston attributed to obliteration by later human activities.

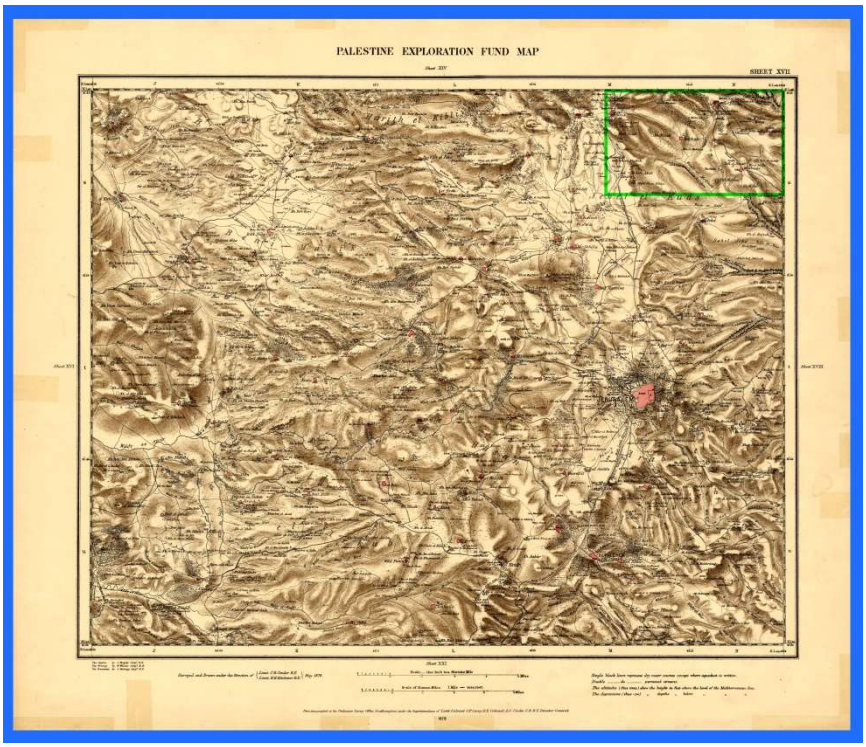

Fig. 1 Maps of the PEF Western Palestine Survey, by C. R. Conder and H. H. Kitchener, 1879.

Above: Map No. 14 (Beitin); Below: Map No. 17 (Jerusalem). Green rectangles mark the area included in the enlarged map below (Fig. 2).

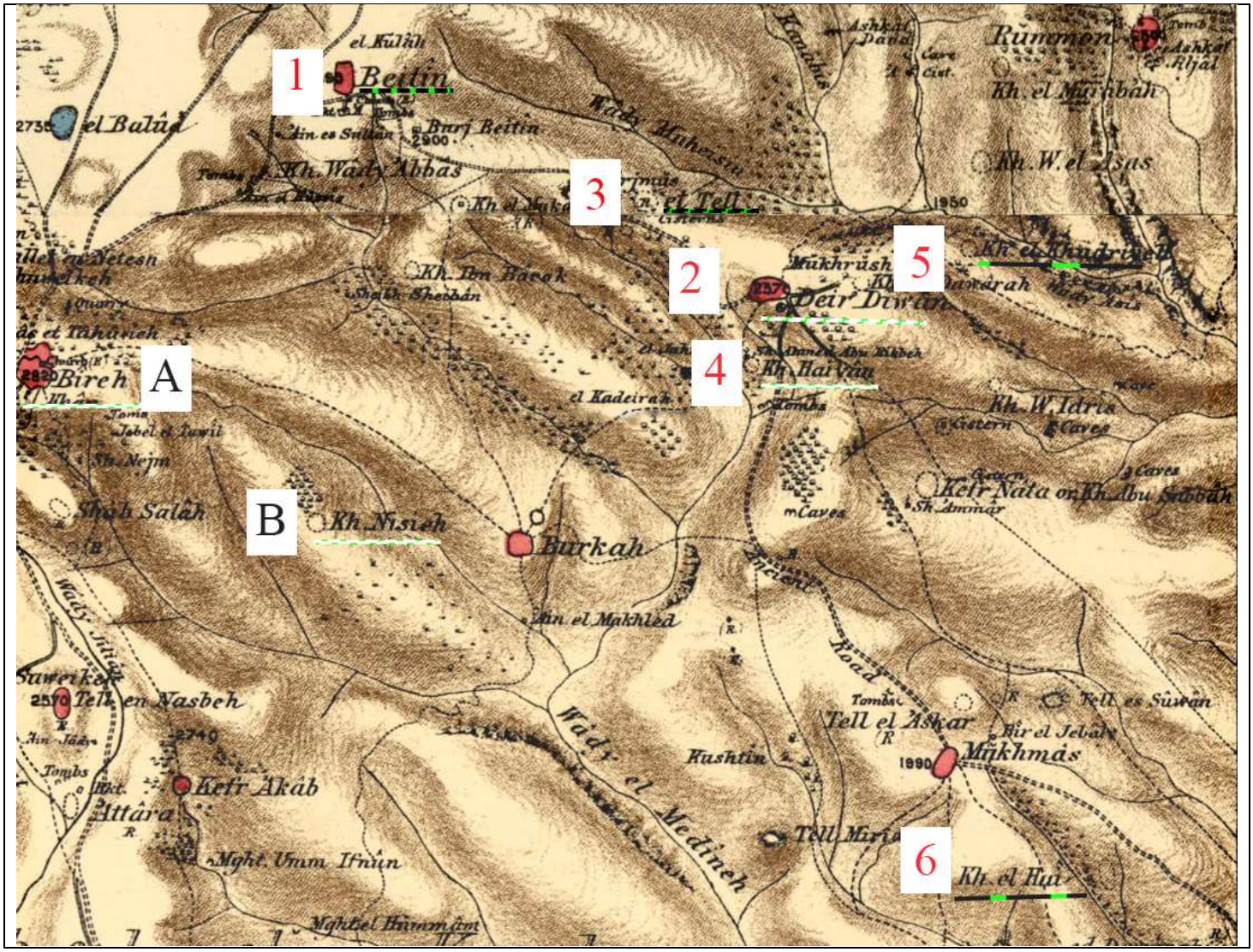

Fig. 2: All the Ai – alternatives. An enlarged section combined from maps No. 14 and 17

1 – Beitin (Bethel), 2 – Deir Diwan, 3 – Et-Tell ("Tell el-Hajar), 4 – Khirbet Haiyan, 5- Khirbet Kydrya, 6- Khirbet el-Hai (Sections A1, A3, and A10).

A – El-Bireh, B- Khirbet Nysia (Section A12)

Division B: Epistemological considerations

Neither these data nor the evidential claims based on them constitute a self-warranting empirical foundation, and yet they can powerfully challenge and constrain the reconstructive and explanatory claims we project onto the cultural past. This is the paradox of material evidence.

Chapman and Wylie 2016

Coherence alone is not a symptom of truth, but maintaining coherence in light of more evidence, independent evidence, is.

Kosso 2001

Well, do historical scientists aim for truth? Sometimes—but they provide much more than a bunch of approximately true claims. They also promote understanding of the processes that shape our world, explanations of past histories, lessons for our own present and future.

Currie 2018

A recap, three notations, and the general theme:

In this part, I discuss several epistemological issues that are raised by the episodes described in the former one.

First, a short reminder; the protracted story in the previous pages can be summarized as follows:

- The story of Ai in Josh. 7-8 was long regarded as central to Canaan's conquest by the Israelite tribes. Searches of many scholars resulted in four alternatives for the site of the biblical episodes (A1). Since the early 1920s, the mound of Et-Tell was generally believed to be the site of this event and it was expected that its excavation would resolve the "time of Exodus" dispute (A4).

- From the analysis of the sherds and vessels found in her 1933-1935 excavation, Marquet-Krause concluded that the site was inhabited during the Early Bronze Age and the early Iron Age, but not during the intervening centuries (A6). I shall refer to this conclusion as **the *Local Chronological Inference*** or the **LCI.**

- This result was surprising, especially since it contradicted the opinion of two distinguished archaeologists who had examined the site before. Marquet-Krause's conclusion that the Ai story must, therefore, be legendary—the ***Local Historiographic Inference*** **(LHI)**—was revolutionary, and, for many of her colleagues also deeply disturbing on both theoretical and ideological grounds (A8).

- Nevertheless, both the chronological result and the inferred conclusion were quickly and unanimously accepted (A9). Scholars who were committed to the conquest hypothesis modified their accounts, usually describing the Ai story as an exception to the generally correct, even if not fully accurate, biblical report.

- Callaway's excavations in the 1960s (A10) verified Marquet-Krause's chronological scheme, and also eliminated all "Ai alternatives" suggested before (A1), making the inference from the LCI to the LHI very robust.

- Total abandonment of the conquest hypothesis came much later, concomitant with further discoveries that conflicted with it (A11). A widespread consensus now considers the book of Joshua wholly or mostly legendary, with the Ai story an example of the rule rather than an exception. I shall call this view **the *Global Historiographic Inference*,** or the **GHI**.

- A minority of scholars rejects the current consensus (A12), offering alternatives to both the identification of Et-Tell with biblical Ai (thus rejecting the **LHI** while accepting the **LCI**) and to the commonly accepted account of the Bronze and the Iron Ages in the Levant (rejecting, in particular, the **GHI)**.

Because the three acronyms **LCI**, **LHI** and **GHI** are going to appear repeatedly in the following discussions, it is worth spelling them out again:

- The **LCI** (local chronological inference): The statement that the mound of Et-Tell was uninhabited during the Middle and Late Bronze Age (in modern chronological schemes, approximately from 2200 B.C. until at least 1300 B.C). This is, obviously, an essentially archaeological inference.
- The **LHI** (local historical inference): The statement that the biblical Ai story is legendary. Identification of Et-Tell as the only suitable location for this story (plus any estimation ever offered for its possible time) in conjunction with the **LCI** entails the **LHI**.
- The **GHI** (global historical inference): The statement that the content of the book Joshua is mostly or wholly legendary.

This part of the treatise is structured as follows: The first three sections discuss what can support hypotheses about the past; the first two concentrate on the **LCI** and the third considers historiographic hypotheses in general. Section four compares the acceptance of the GHI to the Kuhnian conception of scientific revolutions; section five is about inference from absence. Sections six and seven examine how the epistemic opinions of scientists can be influenced by their personal and ideological values. The eighth and last section is an argument for the truth of the **LCI**.

That a complicated and (initially) unexpected inference such as the **LCI** can be (and, given all that we know, *must be*) true, in the sense of *correspondence* truth, is, I submit, a principal outcome of this work. Briefly stated, the truth-claim is based (B8) upon (1) the notion that *something* must be true about the occupancy of the mound at any time of the past and (2) the observation that this inference is the only coherent interpretation of a vast and very diverse amount of data.

Another principal point (B7) is that it is the relative prioritization of values that determines scientists' receptiveness to disruptive results and inferences. This case study provides polarized examples of scientists' values and their influences on their epistemic stances.

In light of these two points, the current work can be taken as a plea against relativistic construals of knowledge, in archaeology, historiography, and elsewhere.

1. Typology as knowledge [34]

Marquet-Krause's dramatic chronological conclusion—the LCI—was based entirely on typology: the classification of pottery into types and the assignment of these types to specific periods. The nature of the site dictated that stratigraphy, the other common basis of archaeological reasoning, played no role (except for the trivial observation that the Iron Age structures were above the deep ash layer).[35] Other methods of chronological determination, like radiocarbon dating, were not even imagined at the time.

The concept of typology, as M. Salmon (1993: 327) observed, is pivotal to every philosophical discussion of archaeology. Archaeologists and philosophers argued a lot, in what became known as "The Typology Debate," over the epistemic and ontological status of archaeological types and classificatory systems, and in particular whether they are "real and discovered" or "artificial and invented" (Dunnell 1986; Adams and Adams 1991: 33, 239; Wylie 2002: 42-56).

None of these issues bothered the scholars who created and used the Middle Eastern ceramic typology during the 1920s and the 1930s. Working in what Adams and Adams call "the classificatory era," their approach to the matter of classification was entirely practical: creating a framework that will support a temporal, spatial and, in some cases, ethnical or societal assignment of sites and layers.[36]

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[34] The argument in this section applies, *mutatis mutandis*, to other sources of archaeological knowledge. I focus on typology because it played a crucial role in this case. In this section and the next I confine myself, furthermore, to what enabled typology to support the LCI. These sections, therefore, should by no means be taken as a treatise on the philosophy of typology or anything similar. See also footnote #7 in (A2).

[35] Stratigraphy was involved in the excavator's suggestion for the sub-division of the Early-Bronze city's lifespan, which does not concern us here.

[36] To a large extent, the same holds today: Conceptual debates about the essence of archaeology etc. that occupied the thinking of many scholars in Europe and (especially) in the United States, had and continue to have only minor influence in the context of biblical and Middle-Eastern archaeology. Discussing why it is so is beyond the scope of this work, but one can point out that "theories that led where practice could not follow" (Adams and Adams 311) were usually eschewed there.

How could archaeological typology, pragmatically understood, support such a far-reaching chronological and historiographic inference?

"Pragmatic" by no means equals "trivial." People who are not archaeologists are usually baffled by the intricacies of classifying and sorting sherds and other artifacts. Finkelstein's critique of Callaway (A10) and Albright's withdrawal of his conclusions that were derived from "examining thousands of sherds" at Et-Tell (A9) show that the task can be tricky even for seasoned archaeologists. As Shepard (1956: 315-6) warned:

> In any event, I would emphasize that the condition of the average pottery sample and particularly the incompleteness of stylistic data make of the pottery type a tentative, hypothetical class to be re-examined, corrected, and amplified from time to time as evidence accumulates; a class that may spill or combined with another, redefined, or discarded. It is a category in the process of formulation instead of a fixed standard of reference.

The ambiguity of types is not the only problem of archaeological typography. Add to this the problems of interpersonal differences in recognizing and describing patterns (Harding 1999: 193), of post-depositional processes that can separate between artifacts and their original contexts (Orton and Hughes 2013: 222), and in particular the theory-ladenness of every premise and the danger of circularity, namely of assigning ceramic types to a certain period and then using those types as a diagnostic marker for that period (*ibid*, 226; see also the next section B2). It would appear that any inference based solely or mainly on archaeological typology should be regarded with reservations.

And yet, Marquet-Krause's **LCI** was not met with reservations. Its robustness is demonstrated by the fact that it was accepted by many scholars of diverse backgrounds over many decades. Among these scholars, many found its historiographic implications deeply disturbing and would have personal, institutional, theoretical, and ideological motivation to refute it, but none saw it expedient to reject it. Notably, even scholars who rejected both the **LHI** and the **GLI** (A12) accepted the **LCI** and the reasoning behind it.

This is not a unique, one-of-a-kind case, but just one example of what Chapman and Wylie (2016: 4) call "the paradox of material evidence":

> Neither these data nor the evidential claims based on them constitute a self-warranting empirical foundation, and yet they can powerfully challenge and constrain the reconstructive and explanatory claims we project onto the cultural past. This is the paradox of material evidence: that 'traces don't speak.' Material evidence is inescapably an interpretive construct; what it 'says' is contingent on the provisional scaffolding we bring to bear. And yet it has a striking capacity to function as a 'network of resistances to theoretical appropriation' that routinely destabilizes settled assumptions, redirects inquiry and expands interpretive horizons in directions no one had anticipated.

An even greater paradox, I argue, arises when challenging interpretations that are based on such a questionable foundation are consensually accepted as veridical.

Tucker (2004, Ch. 1) maintains that the existence of such a consensus indicates that the inference is knowledge-based:

> …heterogeneous uncoerced and large consensus on historiographic beliefs is a likely indicator of knowledge [since]… if a consensus on beliefs satisfies… [these] three conditions, the gap between the likelihood of the consensus given the knowledge hypothesis and its likelihoods given competing hypotheses increases significantly.

A necessary qualification: Note that this construal does not imply that the consensus in question is immutable or infallible. Even an uncoerced, extensive and heterogeneous scientific consensus is not an imprimatur of truth. But it does indicate—makes it very likely—that the consensually shared opinion is informed in a rational manner by the available empirical facts, since it is implausible that such a consensus would have accidentally materialized otherwise. It is in this sense—as an indication of rationality rather than as an assertion of a necessary truth—that I shall use the term "knowledge" from now on. (The truth-aptness of a particular inference is discussed in B8 below.)

The relevant knowledge, in our case, is that of determining the age of material artifacts. The possibility of creating a reasonably stable consensus on this matter is, as explained above, far from self-evident. Indeed, heated debates often occur that are resolved only after many decades, if at all. But in many cases, a consensus on typological classification and its chronological implications does emerge. Somewhat paradoxically, I submit, such consensuses are enabled and maintained exactly by the "dirty" and fragmentary nature of material evidence.

The extended course of events described in the first division can be used to highlight several crucial aspects of such processes:

Knowledge creation: The interludes in sections A2, A5, and A7 discussed aspects of the creation and development of the Palestinian ceramic typology, from the nearly blank state at the end of the nineteenth century to a corpus of knowledge stable enough that Callaway's conclusions in the 1960s reproduced Market-Krause's from the 1930s. These aspects included: making coarse initial assumptions, looking for chronological anchors, heavy reliance on parallels from other sites, creating hypotheses, and, most importantly, constant development and refinement of both the typology and the chronological schemes with an aim of achieving a coherent spatial-temporal framework.

Similar strategies characterize other processes of knowledge creation. In his description and analysis of the development of thermometry, Chang (2004) says that science uses "a method of epistemic iterations" in which

> …we start by adopting an existing system of knowledge, with some respect for it but without any firm assurance that it is correct; on the basis of that initially affirmed system we launch inquiries that result in the refinement and even correction of the original system. (6)
>
> [Such] epistemic iteration is most likely a process of creative evolution; in each step, the later stage is based on the earlier stage, but cannot be deduced from it in any straightforward sense. Each link is based on the principle of respect, and the imperative of progress and the whole chain exhibits innovative progress within a continuous tradition. (46)

Speaking specifically about material evidence in archaeology, Chapman and Wylie (2016) use Toulmin's metaphor of *scaffolding* to describe a similar concept.[37] Scaffolding involves opportunistically applying various resources: "ladening theory, background knowledge (tacit and explicit), technical skills, social networks, institutional infrastructure and vigilant reflexive critique" both for making the archaeological observations possible and for deriving evidential claims from them. The scaffolding process, too, is iterative: "Archaeologists are engaged in a dynamic process of continuously building, extending, and refining provisional foundations."

There were, as we saw, quite a few *faux pas*. Many of the initial assumptions—e.g., Petrie's Amorite/Israelite chronology or the (widely diverging) estimations of the date of Jericho's walls by various archaeologists—were later revised, sometimes profoundly. Whole typological frameworks (for example, the dating of Philistine pottery) were revised several times. Nevertheless, the historical account in division A provides ample illustration of the manner in which successive iterations, scaffolding and re-scaffolding, create, revise, and expand a body of knowledge. The iterative nature of the process is, I believe, evident enough. What is important to note is that it also had a property of *convergence*, in the sense that as knowledge accumulated it also constrained, more and more, the gamut of possible interpretations.

For example, when Albright (A3) together with colleagues from the American School combed the surface of Et-Tell, knowledge gathered from excavations in Ashkelon and other sites enabled him to conclude "almost to his consternation" that the lack of certain types of sherds, such as Phoenician and Cypriote ware, is a strong evidence-from-absence against a Late Bronze occupation on the site. He failed, however, to discern between Middle-Bronze and Early-Bronze ware. When Marquet-Krause came there ten years later (A6), inputs that were added by other excavations created an interwoven web of data "thick" enough to constrain further the possible interpretations of the site's chronology. She was thus able to conclude that the site did not reach even the beginning of the Middle Bronze Age, effectively increasing the resolution of *relative* dating from ca. five to one hundred years.

[37] Currie (2018: Ch. 10) applies a similar concept of "scaffolding" to the generation of historical knowledge in general.

Stability of knowledge: As Currie (2017) notes, it is not clear if Chapman and Wylie's scaffolding concept allows for a "completed product" in archaeology. All knowledge, indeed, is tentative and fallible.

It is a fact, though, that the **LCI** remained unchanged despite the vast increase in the amount of data and the substantial development of inferential tools in the eighty years since it was first published. Nor did the watershed changes in the general understanding of the Bronze-Age transition in Palestine—from the Conquest to the Peaceful Immigration to the Autochthonic scenario (Wallach 2018, 2019a) —involve a revision of the LCI and the classification scheme that underlies it.[38] This suggests that under some circumstances the repeated scaffolding process can result in knowledge that is very stable.

Such stability is, of course, not guaranteed. Not only is knowledge (in the sense used here) not immutable, but it is also liable to deteriorate when the process of iterations and refinement cease. A vivid (and distressing) example is given by Albright's critique (also echoed by Moorey 1991:61) of Petrie's later works in Palestine:

> Petrie's absolute dates, being based on a scarab of Tuthmosis II and a calculation of the relative rate of deposit of debris on the mound, are worthless before 900 B. C. He carefully disregards all archaeological work done in Palestine since his own first excavations at Teil el-Hesi in 1890. (1929a: 9, 2n)

Limitations of knowledge creation: Marquet-Krause could point to significant and sometimes surprising aspects of Et-Tell chronology. She did not attempt, however, to say anything about the cognitive and spiritual world of the people who lived in the city and worshipped in its sanctuaries. Excavating the city's necropolis, she noted that one of the skeletons held a small decorated amphora in its palm "which might have a significance that eludes us" (1935:343). Had she tried to opine on these matters (or had anybody used her data for this purpose) the outcome would have

[38] This resonates with Chang's (2004: 52) observation that mid–level regularities are often robust enough to be decoupled from changes in grand theories.

been much less stable, much more underdetermined than her chronological scheme. The ability to infer from material remains to cognitive stances is very limited.

But the obstacles to our knowledge of the past are not confined to questions of human beliefs and cognitions; depending upon the evidentiary status, even more mundane inferences may be unwarranted. We do not know (the suggestions of Dussaud (A9) and Grintz (A12) notwithstanding) the name of the ancient city and who destroyed it, and probably never will. Adams et al. (1979) show that a close connection between dated pottery types and historical development cannot be taken for granted. In fact, more than a century of biblical archaeology failed to provide a clear answer to the question of the "origin of Israel." Even more inherently, all archaeological dating methods, whether by typology or by modern methods such as radiometry, have a finite resolution which means that some intriguing questions cannot be answered definitively.

It does not follow that such subjects are not amenable to a historiographic investigation that is informed by archaeological evidence. However, when the material evidence or our ability to interpret it is limited, archaeological reasoning is more akin to what Chapman and Wylie, following Bradley, call "ways of seeing" (2016: 42). Whether such a pluralistic landscape of hermeneutically supported views can also be interpreted as an indication of knowledge is a question that is beyond the scope of this work. (cf. also Tucker 2004 Ch. 6 about the limits of historiographic knowledge.)

2. The risk of circularity

All observations are theory-laden, and all interpretation is liable to be biased by prior assumptions. An archeologist's interpretations of material evidence are likely to be influenced by her theoretical assumptions, including her preferred historiographic framework. This theory-ladenness can lead to circularity when the interpretation of archaeological findings depends on hypotheses that the same traces are said to support.

For an example of archaeological interpretation engendering circularity, consider Albright's (1935: 13) discussion of results from British excavations at Tell ed-Duweir (ancient Lachish):

> There can be no doubt about the competence of Mr. Starkey and Mr. Harding in matters of pottery, but one may be permitted to doubt whether the absolute dates are entirely correct, since the writer has had occasion before to note a divergence of from thirty to fifty years in the average chronology of the thirteenth and the twelveth centuries in our publications … A date in the second half of the reign of Ramesses II seems, therefore, more probable than one in the first half.

Albright's reliance on his own chronological scheme to criticize Starkey's conflicting one is clearly problematic. But much more was at stake here: Ramesses II "The Great" reigned 66 years. The famous "Israel stele" of his successor Merneptah was understood as providing a crucial chronological anchor for the early years of the Israelite conquest of Canaan (A4). A much earlier date for the last material remains from Canaanite Lachish (as suggested by Starkey) would, therefore, challenge a scenario in which Albright (and many others) strongly believed at the time.

The risk of circularity is not peculiar to archaeology. Discussing the physics of calorimetry, Chang (2004: 59-60) observed that:

> Whenever we have a method of measurement that rests on an empirical law, we have the same kind of problem in testing and justifying that law. This circularity is probably the most crippling form of the theory-ladenness of observation.

Whether because of it's hands-on, readily accessible nature or for other reasons the problem of circularity (and of theory-ladenness in general) is often mentioned in connection with archaeological results and their interpretation:

> If the data stand as evidence only under interpretation, could they not be interpreted in any number of different ways, and thus support a myriad of alternative reconstructive and explanatory hypotheses? Even more worrisome, does this contextualism not entail that inferences concerning the

> past are unavoidably circular, that archaeologists will necessarily find in the record just, or only, what their conceptual framework prepares them to recognize as evidence? (Wylie 1993: 22)

Some archeologists have concluded that circularity in archaeological interpretation is inevitable and incurable:

> We must reject any naive distinction between the object conceived as concrete hard fact and theories or ideas about it conceived as abstract. Theory works on empirical objects which are theorized, brought into the account, through the subject-object reflexive relationship … There is literally *nothing independent of theory* or propositions to test against. Any test could only result in a tautology. (Shanks and Tilley 1987:111).

Chang's solution to the problem of circularity is, surprisingly, to accept it and try to "tighten" it through successive epistemic iterations

> The only productive way of dealing with that circularity is to accept it and admit that justification in empirical science has to be coherentist. Within such coherentism, epistemic iteration provides an effective method of scientific progress, resulting in the enrichment and self-correction of the initially affirmed system. This mode of scientific progress embraces both conservatism and pluralism at once. (2004: 239)

Chapman and Wylie (2017: 28) advocate a more nuanced and balanced attitude: Not every instance of theory-ladenness must lead to circularity, and not every shade of circularity invalidates the archaeological inference:

> The second intuition implicit in practice is that the theory-dependence of archaeological evidence does not in any necessary or comprehensive sense entail vicious circularity. The background knowledge, assumptions, and expectations that play a role in interpreting data as evidence – the 'ladening' theory – only pre-determine what archaeologists will find or recognize as evidence if there is a hyper-integration of inferential warrants and test hypotheses. That is, a compromising circularity only follows if the middle-range theory that archaeologists rely on to interpret data as evidence is the

> same as, or presupposes, the hypothesis this evidence is used to support or to test. This is, of course, always a risk, but it is not a given.

The key to avoiding vicious circularity is, then, to assure independence between the information used as evidence and the theory or hypothesis that information is said to be evidencing.[39] Inferences based on archaeological data can be made reasonably non-circular (and also correctable, or "tightable" *sensu* Chang) if they are connected to diverse and reasonably independent sources of data such as different sites and strata, foreign chronology, radiometric dating, etc.

A similar idea, in somewhat more technical terms, is outlined by Kosso (1989:183):

> It is a likely fact of life that some part of the theory-of-x will be used to account for the observation of some aspect of a particular x. But a degree of independence is preserved if this is not a crucial part of the theory, the part which explicitly discusses that aspect of x which has putatively been observed, the part which could, therefore, benefit (by confirmation) from the observation. In relying on the relationship between TX1and TX2 and the relationship between TX2 and {T}, this characterization of independence of an account potentially admits of degrees.

Independence, then, comes in degrees and, *ipso facto*, the same (counterintuitively, perhaps) holds for circularity. The more contingent upon the theory being tested an interpretation of evidence is, the more vicious is the circularity of the reasoning. Albright's objection to Starkey's dating of Lachish stratum VI, cited above, is tainted with a non-negligible degree of circularity because it relied crucially on a historiographic hypothesis that this dating challenged.

Chang, however, dismisses both the possibility and the necessity of independence:

> What is certain is that there is no guarantee that observations enabled by a particular theory will always validate that theory… Therefore there is no clear reason to wish for theory-neutral observations or seek what Peter

[39] Wylie (2002: 192) distinguished between two kinds of independence: That of background assumption from test hypotheses ("vertical") and that between the assumptions used in the interpretation of different sources of evidence (horizontal).

> Kosso (1988, 1989) calls ‘‘independence’’ between the theory of the instrument and the theory to be tested by the observation produced by the instrument. Even in the case of positive test-outcomes, the comfort provided by independence is illusory. Duhem’s physiologist relying on the laws of physics can be comforted only as far as those laws of physics are reliable.[40] (2004: 95)

I think Chang is wrong here. For one thing, he ignores Kosso's observation (which Chapman and Wylie implicitly share) that independence is a matter of degree.[41] Consider, for example, the important topic of providing "anchors"[42] to the archaeologists' chronology. At the period discussed here, absolute chronologies were derived from artifacts of Egyptian origin such as cartouches, hieroglyphic or hieratic inscriptions referring to Pharaohs. Interludes A2 and A5 in the previous division show how crucial these were for stabilizing the "scaffolding" of the Palestinian typology and through it the security of inferences like the Market-Krause's **LCI** and **LHI**.

Chang would perhaps say that these "anchors" and inferences based on them were as good as was the chronology of ancient Egypt at the time. He could add that this chronology was established by methodologies of stratigraphy, typology, seriation, etc., which were similar to those used by archaeologists in Palestine and thus, potentially, subject to common errors. However, what matters is that the determination of Egyptian chronology did not rely on theories about the archaeology and history of Palestine. At the very least this makes such determination *less circular* than otherwise. Inferences that depended not on absolute but on relative Egyptian

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[40] Duhem said that theories in physics can only be tested holistically, among other things because test instruments are designed according to physical theories, but physicians can rely on similar instruments since such theories are not part of their reasoning.

[41] This difference of opinion between Kosso and Chang may be related to their different concept of truth: Kosso speaks in terms of *truth* of descriptions of the past, so that "a true description is one that matched the determinate state of affair" and "coherence alone is not a symptom of truth, but maintaining coherence in light of more evidence, independent evidence, is" (1989: 182). Chang (2004: 228) thinks that truth is inappropriate as an epistemic goal, or (2018: 33) is willing to endorse a notion of truth that "is a matter of degree and circumstances". See (B8) for my own view about the relation between truth and coherence.

[42] "External evidence" in Adams and Adams' terminology (1991: 122).

chronology (i.e., the order of Pharaohs and lengths of their reign) were *even less circular* since this relative chronology was also supported by textual and epigraphic evidence, whose interpretation normally does not depend upon archaeological methods.

Second, recalcitrant evidence has the power to constrain the space of possible interpretations, sometimes to the point of making historiographic hypotheses completely untenable and thus breaking a chain of circularity. To return to the example mentioned in the beginning of this section, when later excavations at Lachish exposed a cartouche containing the name of Ramesses III (Ussishkin 1987:13) below the destruction debris of the last Canaanite city, no option remained to preserve a thirteenth-century B.C. conquest scenario (in either Albright's or Starkey's version) for the site.[43]

3. Appraising historiographic hypotheses: Consilience, Coherence or "Smoking Guns"?

Sciences of the past, including archaeology, seek to account for present circumstances by postulating past events, processes, and state-of-affairs. Often, several plausible hypotheses are offered to explain a given assemblage of traces. If, as in our case here, a "winner" is selected by the relevant scientific community, how is this selection made? Which "game rules" influence it?

Cleland (2002, 2011) suggested that discrimination between competing hypotheses in the historical sciences can be achieved using "smoking gun" evidence. A smoking gun is a (previously unexpected) trace that unambiguously discriminates one hypothesis about the past because this hypothesis, but not others, provides an explanation of the total body of evidence including the newly-discovered trace(s).[44]

[43] Thirty years separate the death of Ramesses II from the beginning of Ramesses III thirty-one-year reign.

[44] Cleland (probably in response to criticism by Forber & Griffith below) insisted that smoking gun evidence does not play the role of an *experimentum crucis*, conclusively determining between competing hypotheses. Rather, "a smoking gun for a hypothesis is a capstone piece of evidence; it can only be judged as a smoking gun when combined with the rest of the evidence available" (Cleland 2013: 4; cf. also Currie 2018: 235)

As its name implies, the discovery of a smoking gun has sociological and psychological dimensions: "the more improbable an association among a collection of traces seems, the more psychologically appealing the claim that it is the product of a common cause." (2011:19). The discovery of smoking-gun evidence is not guaranteed, but Cleland maintained that it might be a fairly common affair, because in the world as we know it, a past cause will typically have extensive and diverse localized extant effects, while a present event or state-of-affairs normally have more than a single cause. This "asymmetry of overdetermination" (after Lewis 1979) warrants that "One can never rule out the possibility of finding a smoking gun, and this is a consequence of an objective fact about nature" (2002: 492). Jeffares (2008) further elaborated this idea by arguing that historical reasoning, including reasoning from smoking gun evidence, relies on the availability of regularities that "are testable and stable enough to work in the relevant circumstances."

A smoking gun at Et-Tell? Judging by the unexpectedness of their results and the effect they had, the results of Marquet-Krause excavations can be viewed as a typical case of a smoking gun. Can we, therefore, label it as yet another example (besides the proverbial Cretaceous-Tertiary extinction and a few others) of Cleland's thesis?

I think we should not, for two reasons: First, the excavation's results themselves were based upon archaeological (specifically, typological) reasoning which, as discussed above (B1, B2), was achieved through a painstaking iterative process with no possibility for smoking-gun shortcuts. Secondly, these results had proven insufficient in themselves to discredit the Conquest hypothesis in the eyes of most scholars (A9). Something more was needed (A11) for the **GHI** to be accepted.

Cases of a true "smoking gun"—formerly unexpected pieces of evidence that (in combination with other data) speak decidedly for one hypothesis among several—are rare in archaeology (and probably in other historical sciences as well). Albright's employment of potsherds' analysis to decide between several "Ai alternatives" (A3) had such an effect, but only because the hypotheses he debunked were at the time several generations old. The discovery of unmistakable evidence of Rameses III in Lachish, mentioned in the previous paragraph, or of an inscription referring to the "house of David" (Biran and Nave 1995) can also be a candidate. Scratching one's

head for examples brings to mind discoveries related to the global dispersion of hominins, such as the evidence for the Denisovans or a pre-Clovis human presence in America. The majority of archeological inferences, however, cannot appeal to such singular and conclusive supports.

Forber and Griffith (2011) discounted both the plausibility and the importance of smoking gun evidence as posited by Cleland and Jeffares. Firstly, because time exposes the traces of past events to attrition and obliteration, "metaphysical overdetermination is compatible with epistemic underdetermination" (after Turner 2005), and therefore we may never find extant traces that will have the power discriminate between competing hypotheses.

Even more fundamentally, since the interpretation of empirical evidence depends on a host of auxiliary and background assumptions, even an argument that is based upon observation of telltale and surprising traces may still be underdetermined. A supporter of the Conquest hypothesis, for example, could accommodate the **LHI** while maintaining that an Israelite conquest of Canaan did take place by bracketing off the content of Josh. 7-8 as an etiological story (Albright), a grossly exaggerated report (Vincent) or just an exceptional legendary narrative (Yadin).

Forber and Griffith maintain that strong epistemic support for historical claims is derived from the consilience of multiple independent lines of evidence concerning essential properties of the causal history proposed, rather than from improbable "smoking guns". The degree of consilience, as analyzed by them, depends on two factors. The first is the extent to which the various lines of inference constrain the assumed causal history and its various aspects (can we safely discard other explanations?) and is not very different from Cleland's "smoking gun" criterion. The second is the degree of independence between the auxiliary assumptions that are required to derive each inferential path from extant traces to past events (do we really have independent evidence or do they all depend on the same assumptions and may fail with them?). Both Cleland and Forber and Griffith use the asteroid-driven mass extinction at the Cretaceous-Paleogene boundary to illustrate their argument.

Currie (2016, 2018)[45] noted that the difference between the approaches of Cleland and Forber & Griffith is not profound, since both are concerned with the relation between common-cause hypotheses about the past and extant traces, and because "smoking gun" arguments are often themselves based on ensembles of (consilient) evidence. Using case studies in paleontology for illustration, he argues for another (non-exclusive) methodology for generating and adjudicating hypotheses in the historical sciences: Testing for coherency between hypotheses about the past. For example, a hypothesis that gigantic sauropods were hot-blooded coheres with a hypothesis about their feeding strategies (maintaining endothermic metabolism requires consuming a lot of food), and both draw support from evidence about their dentition. A hypothesis that the glaciation in the last "snowball earth" episode was incomplete coheres with hypotheses that the later "Cambrian explosion" resulted from separated local refugia in this incomplete ice cover. Scientists exploit these dependencies by constructing interdependent explanations with coupled confirmation, which serve as "coherency tests."[46]

Commenting on Currie, Wylie argued (in Chapman and Wylie 2016: 66 n14) that "this practice of reasoning from explanatory coherence, a practice that Currie sees as a distinctive (non-trace-centric) strategy, depends upon an appeal to the convergence of distinct lines of evidence which is a standard feature of trace-centric accounts."

Archaeological reasoning: consilience-cum-coherence: I think Wylie's remark cited above captures much of what archeologists do when they attempt to offer an interpretation of theirs and other's results, or a reconstruction of the past based upon such interpretations. The overused "jigsaw-puzzle" metaphor for archaeological research, though not totally adequate, reflects this concept by invoking the ideas of "fitting together" elements of an unknown big picture and approaching this task from various directions.

[45] In his last book, which appeared when this work was coming to a close, Adrian Currie (2018) presents an expanded version of this theory and applies it to historical reconstruction in general. As far as I can tell, my approach and conclusions here generally accords with his.

[46] Currie (2016: 16) insists that this position does not oblige him to a coherentist epistemology. I share this view (see B8).

And so it has been since the dawn of the discipline. Consider, for example, how Trigger's *History of Archaeological Thought* (2006) describes the very first steps in the development of archaeological typology (my italics):

> … all the characteristics of individual objects and of the objects found together in closed finds *displayed coherent patterns* with respect to material, style, decoration, and contexts of discovery … Thomsen's[47] crude but effective technique of occurrence sorting produced classificatory units that appeared unlikely to have coexisted and, therefore, most probably represented a chronological sequence. (p. 126)
>
> Because of the vastly greater amount of data available and Montelius'[48] more detailed artifact classifications, it was possible for him not only to identify shorter periods but… to order these periods chronologically. For such a sequence to be persuasive, materials, techniques of manufacture, shape, and decoration had to *covary in a coherent pattern*. (p. 226)

Usually (our case study is atypical in this respect) archaeological reasoning combines typology with stratigraphy. Often, other sources of information such as epigraphic, numismatic, remote sensing, and dates derived from dendrochronology or (increasingly in modern times) radiometric data are involved, and their combined result need be not only consilient—converging to the same conclusions—but also coherent.

But what is coherence, anyway? It turns out to be a difficult concept to qualify, much less to quantify. Mere consistency is obviously not enough: If you believe, for example, that (a) Joshua won the battle of Jericho and (b) The rain in Spain stays mainly in the plain, this set of beliefs is consistent, but hardly coherent. The Cambridge dictionary defines coherence as "the situation when the parts of something fit together in a natural or reasonable way" (which again brings to mind the jigsaw-puzzle metaphor). The Oxford dictionary stresses "the quality of forming a

[47] Christian Jürgensen Thomsen (Denmark, 1788-1825) was one of the first antiquarians to use typology in order to determine the age of past artifacts.

[48] Oskar Montelius (Sweden, 1843-1921) was arguably the first to use archaeological typography in a systematic and comprehensive manner.

unified whole." Generally speaking, we expect that elements in a coherent set will be *connected* in some significant way. Scholars who tried to characterize the required connectedness stressed a casual (Carrol 2001: 37), explanatory (Kosso 2001: 75-6), or inferential (Kuukkanen 2015: Ch. 7) relationship, or merely the continuity of entities in space and time (Hull 1975). Depending on the particular context, each of these connecting schemes can serve as the "glue" of coherence. (cf. Wallach 2019a.)

Here is an example of coherence considerations shaping archaeological interpretation in a small way: Upon discovering a collection of stone and alabaster vessels in a context of Early Bronze Age III, Marquet-Krause (1935: 332-3, 1949:31 [A6], [A7]) noted that they were *une réplique exact* of Egyptian artifacts from the time of the second dynasty, that is, Early Bronze II—a difference of more than two hundred years. Ruth Amiran (1970) had a better idea: These artifacts were not replicas, but a heritage, retained through generations because of their rarity and (possibly) sanctified status. This interpretation is more coherent because it posits something that people normally do, rather than a rare preservation of style and technique over hundreds of years, and is in harmony with a more general historiographic hypothesis—namely that the links between Egypt and Canaan were interrupted after the Early Bronze II period—that was perhaps not known to Marquet-Krause. A chemical or physical analysis of the material from which these vessels were made (a much later development) could add a consilient evidence of their provenance.

More important than the exact definition of coherence is the requirement that it should be maintained over an extended time and against the challenge of additional data from multiple sources. In fact, one-time coherence is of no epistemic value at all:

> it is only in… the case in which the belief system converges on and eventually presents a relatively stable long-run picture of the world, thus achieving coherence over time … that the coherence of the system provides any strong reason for thinking that the component beliefs are thereby likely to be true. (BonJour 1985: 170).

> Coherence is a necessary requirement of justification, but by itself it is not enough to separate good fiction from the truth. Add to it the need to encounter a steady supply of evidence, to acknowledge at least some of it as reliable and relevant, and to fit it into the existing coherent system, and it is less and less likely that the network of fiction will maintain its coherence. (Kosso 2001:182)

Even though Petrie (A2) and Albright (A5) supported their analysis with diverse inferential lines that led to coherent conclusions (in their language, ones that "agree well"), many of these conclusions did not stand the test of time. In contrast, Joseph Callaway's excavations at Et-Tell reached, maybe against his hopes (A10), the same conclusions as did Marquet-Krause more than thirty years earlier. *It is this stability against a deluge of artifacts, stratigraphic data, and repeated epistemic iterations* (B1) that makes the **LCI** practically unassailable and the **LHI** practically unavoidable.

The GHI and beyond: Coherence and historiographic narratives: Complex historiographic hypotheses (often presented in narrative form) are always underdetermined by the available evidence—there is no way to "prove" the **LHI**—but the need for coherence of narratives can force modifications in them and even lead to the abandonment of a widely believed and strongly evocative hypotheses. And the more *diverse and consilient* the evidence, the tighter will these constraints be.

In Wallach 2019a I examines the role of coherence in determining the evolution and the fate of historiographic narratives, again stressing the importance of dynamic maintenance of coherence over the long term.

4. Kuhnian considerations

When the consensus opinion began to regard the books of Joshua and Judges as ahistorical (A11), it became fashionable to describe this change of consensus as a Kuhnian paradigmatic shift (Herzog 2000, Broshi 2008). Viewed in this light, all the attempts to resolve "the embarrassment of Ai" constituted just one more example of how defenders of a paradigm "devise numerous articulations and *ad hoc* modifications in their theory in order to eliminate the apparent conflict" when faced with an anomaly (Kuhn 1970:78).

The change was indeed profound. It altered not only the understanding of the Bronze-Iron transition in Palestine, but also the span of possible scenarios for the emergence of the territorial monarchies of Judea and Israel, for the origin of the Jahvist religion, and much more. In a sense, it transformed the study of the period between the thirteenth and the tenth centuries in Palestine to something akin to the study of prehistory, because the biblical texts about these periods are now viewed as legendary, and other written sources do not exist.

Nevertheless, I claim that the Kuhnian concept of "paradigm shift" or "scientific revolution" does not capture this change of consensus. It deviates from what happened in three significant aspects:

- *There was no incommensurability*: In the case of the Ai excavations and elsewhere, the theoretical concepts—ages and periods, strata and phases, pottery types and inhabitation periods—were similarly construed by the scholars involved. So were also the historiographic questions that were addressed: the origins of the Philistines and the Israelites and the time of their appearance in Palestine, the organization and patterns of living in various sites, etc. Arguments and disagreements abounded, of course, and sometimes basic elements of the working terminology were argued upon and altered.[49] But there was no situation of scholars talking past each other or failing to comprehend what their predecessors had meant. Had it been otherwise, neither the shock created by Marquet-Krause results nor the rapid acceptance of the **LCI** and the **LHI** would have been possible. Nor could Callaway eliminate the alternatives to the Ai site offered in the preceding century, or Finkelstein argue against Callaway's hypothesis (A10) on the basis of Marquet-Krause's drawings and notes.

This situation may be common. Putnam (1981: 117) maintains that as long as people share the same *concept* of an entity, they can communicate even if their *conceptions* of it are different. Buchwald and Smith (2001) offer a pragmatic test: If empirical

[49] For example, the chronological division of the Iron Age, which was inspired by Albright and used (by Marquet-Krause and others) until the 1960s was revised by Aharoni and Amiran (1958) and again later.

evidence derived with the help of one hypothesis is part of the justification for another one (as were, for example, classical computations used to compare the measured value of Mercury's perihelion to relativistic predictions), then the two hypotheses are, *ipso facto*, commensurable. The iterative, scaffolding-on-scaffolding aspect of archaeological knowledge (B1) makes this beautifully apparent.

- *The process of consensus change was lengthy and gradual* rather than a gestalt-like transformation as described by Kuhn (1970: 77-91). Initially, a series of hypotheses (Albright 1934; Phythian-Adams 1936; Vincent 1937; Dussaud 1937; Yadin 1965) modified the conquest scenario while retaining its core assumptions. These hypotheses could, indeed, point to archaeological results that appeared to support them (A11). Only later, as more and more findings that could not fit well into the scenario were piling up from excavations and wide-area surveys, was the Conquest hypothesis abandoned and replaced with the peaceful immigration one. Similarly, the recognition that the material culture of the "Israelite" settlements is of local, indigenous nature and cannot support either a conquest nor an immigration scenario evolved gradually, passing through several interim hypotheses. (Amiran 1963: 233-344; Fritz 1987; Finkelstein and Na'aman 1994; Finkelstein 1998; Dever 2003).

    - *Undeniably, progress has been made*: For Kuhn, scientific progress is something that happens only within the normal, puzzle-solving phases. In scientific revolutions and paradigm shifts, however, progress is illusory: "Revolutions close with a total victory for one of the two opposing camps. Will that group ever say that the result of its victory has been something less than progress?" (*ibid*)

Concerning the **LCI**, Even the "revisionist" (A12 and B7) accept that the accuracy and consistency of chronological determination today is superior to what had been in Marquet-Krause's time and certainly before that. Most other scholars will agree that current historiographic understanding of the Bronze / Iron transition is substantially superior to that of Albright's (not to speak of Robinson's) days.

It is easy to conclude from the above that even though this change of consensus was quite profound, it was not a "real paradigm shift."

Adams and Adams, however, think that the problem is deeper, and relates to all sciences of the past. Commenting on Kuhn's idea of the sovereignty of paradigms over observations, they say (1991:321):

> If Kuhn were right about cosmological paradigms there should be no such thing as *the* history of the world, and consequently no unique historical or prehistoric record for the archaeologist to reconstruct. But again the typological example makes this doubtful… the provenience-dates that are indicated by the potsherd data are not artifacts of the typology; they are extrinsic to it.

This argument of Adams & Adams was criticized by Wylie (1992: 490) as too light and insufficient to repel the claim of post-processualist archaeologists (echoing Kuhn) that data and observations are themselves not autonomous of expectations. I think, however, that this critique misses the point: It is exactly the ability to (sometimes) derive coherent and stable results out of admittedly loaded observations that challenges the relativistic worldview. I shall return to this topic in the last section (B8).

Finally, a historical side-note: Planck's maxim (cited in Kuhn 1970:151) that "a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it" may, if you wish, be applied to the first change of consensus (from the Conquest to the Immigration hypothesis) but not to the second one (from the Conquest to the Autochthonic one). The latter occured within a single generation and largely by the same scholars.

5. Inference from absence

"Absence of evidence is not evidence of absence," wrote Carl Sagan[50] (Sagan and Druyan 1997: 213) creating one of the best-known maxims used in scientific and philosophical argumentation. The aphorism is recent, but the concept of "we have not seen is no proof" is venerable (e.g., Lock 1690).

And yet, in the archeological researches at Et-Tell, inference from absence was nonchalantly made and incontestably accepted, not once but three times: once by Albright to conclude that the site was not inhabited during the Late Bronze Age (A3), then by Marquet-Krause do derive the LCI (and from it, the LHI, A6), and finally by Callaway to affirm Marquet-Krause's results and to eliminate all "Ai alternatives" (A10).

In Wallach 2019b I cite many examples that show that the use of inference from absence (IfA) is common and widespread in archaeology. Archaeologists often accept arguments based on the absence of evidence as comparable to arguments from evidence: fallible and defeasible, of course, but having a respectable plausibility. Quite a few TPA and TPQ (date before or after which something happened or existed) determinations are based on such inferences.

Not only that, but archeologist are sometimes prepared to infer from paucity, treating a small number of certain artifacts at a particular locality as lack thereof. This is why Garstang's claim that he found a single Cypriote wish-bone handle at Et-Tell (A3) and Yeivin belated memory of having seen some Late Bronze vessels (A6 n16) did not receive much attention: Even if true, they could well have resulted from random post-depositional activities and therefore are inconsequential for determining the time of occupation of the site.

This situation is quite unique: Inference from absence in the sciences is rare, and another group of examples cited in Ch. 4 demonstrates that when raised, it is usually considered inconsequential or outright false. In the historical sciences in particular, the lack of evidence is cited (Turner 2007, see also B8) as an argument for pessimism

[50] Quoting the British astronomer Martin Rees.

about our ability to know the past. Probabilistic models by Sober (2009) and McGrew (2014) regarding IfA in Palaeontology and Historiography, respectively, demonstrate that even though IfA is not literally false (because, logically, if evidence supports a hypothesis then its absence supports the hypothesis' negation), it is usually very weak. IfA in archaeology, to the best of my knowledge, has never been analyzed.[51]

The discrepancy between archaeology and other sciences can be resolved once we note that absence of evidence *is* evidence of absence, and potentially a very strong one *if the evidence is strongly expected*. The stronger our credence that evidence for something exists and can be found, the greater our surprise when (after a well-conducted search) it is not found and our justification to draw conclusions from its absence. If the dog always barks at strangers and was not heard barking, then the intruder must have been a non-stranger.

In an orderly archaeological excavation, the likelihood of finding traces in a site that was sedentarily inhabited for an appreciable period is very high. That is because human beings, since early prehistory, are avid producers and discarders of artifacts wherever they are. These artifacts are usually distinguishable from their natural environments, and many of them have good survivability over the time scale of interest to archaeologists (think ceramic sherds). A prolonged sedentary inhabitation should, therefore, result in many identifiable traces in the site. And because excavation is a (locally) very intensive search, the likelihood of finding none (or just a few) of these traces, assuming the site was inhabited in the relevant period, is negligible.

This simple consideration (See chapter 4 for a more detailed and rigorous treatment, including a probabilistic model) warrants the widespread use of IfA in archaeology. One notes that this state of affairs is intimately connected to archaeology being a science that researches the *human past*, and it is difficult to create in other disciplines.

[51] Currie (2018, Ch. 5) is more optimistic about our ability to overcome the problems posed by the "gappiness" and "faintess" of the traces record, but he does not discuss archaeology per se.

Before closing this short exposition, several caveats need to be mentioned:

- First, excavations of sedentary sites are, of course, an important part of archaeological research. However, other methods of archaeological research exist in which the above argumentation does not apply or is weaker. In particular, an over-the-surface field survey can miss evidence, which might have contributed to Albright's mistake when he combed the surface of Et-Tell with his students (A3, A9 #n29). In other circumstances (such as the search for settlements of nomads) IfA can be entirely misleading.
- Second, excavations are *local*. Strictly speaking, inference from an excavation—either from evidence or from absence thereof – is similarly local. Generalizing from a partial excavation to the whole site is an additional inference, which needs to be justified on its own, taking the context into account. Garstang's conjecture (A8) that the biblical city might lie outside the area of Marquet-Krause's excavation was not vindicated, but only Callaway's excavation (A10) put it to rest by expanding the excavated site.
- Third, even under the best circumstances, inference from absence is just one kind of empirical, inductive inference, and as such fallible and defeasible. But unlike in most other scientific circumstances, the onus of arguing for and against an archaeological IfA is more symmetrically divided. For example, Garstang's (A8) and Livingstone's (A12) suggestions that the absence of traces is due to their complete post-depositional removal by human or natural activities require pointing to circumstances (possible, but rather rare) that have enabled such a removal (which they did not do).

6. Why was "the great embarrassment" allowed?

Current theories in the sociology of science (Barnes and Bloor 1982; Bloor 1991) ascribe to social and ideological factors a major role (and in some versions, an exclusive role) in shaping scientific opinions. From this perspective, one would expect, at the very least, that the **LCI** and certainly the **LHI** would be met with deep suspicion and trenchant criticism. As already noted (A8), these inferences challenged not only the dominant historiographic hypotheses but also deeply entrenched ideologies and emotions. For Jewish scholars, from Marquet-Krause and Yeivin to

Klein and Yadin, the story of Ai was part of the national lore that inspired a modern revival. Many non-Jewish archaeologists, from Pythian-Adams, Vincent and Albright to Callaway, were devout believers, mostly graduates of denominational institutions and often ordained priests. Numerous references cited above show how disturbing were these inferences to most of them.

However, Market-Krause's results—both the **LCI** and the **LHI**—were quickly and unanimously[52] accepted as veridical (A8, A9). How can this be reconciled with a point of view which maintains that "Scientific theories, methods, and acceptable results are social conventions" (Bloor 1991:43)?

My answer, admittedly old-fashion, is that scholars' commitment to epistemic values denied them the option of rejecting or disregarding a result that was entirely plausible when evaluated by these norms.

As Tucker (2004: 42) states:

> Legitimate historiography is marked by the precedence of critical cognitive values over other values, not by the absence of other values that generate different historiographic interpretations. Indeed, the presence of values in historiographic interpretation is inevitable (Berlin, 1969). As long as the hierarchical precedence of cognitive to other values is preserved, legitimate historiography can accommodate myriad different and conflicting values and ensuing interpretations… Once we understand the hierarchy that gives precedence to consensus-generating cognitive values over other values that divide the historiographic community, it becomes clear that value-laden historiographic interpretation is inevitable, but hierarchically inferior to its scientific core according to cognitive values.

Axiological considerations, therefore, can explain how people accept scientific inferences that go against their prior beliefs and interests. Tucker does not offer a list of appropriate cognitive values, and neither will I. But high on any such a list, I

[52] The passionate protest of a certain Mr. Edward and the evasive reaction of Garstang during the 1936 meeting of the Palestine Exploration Fund (A8) are exceptions that underline the rule.

think, would be the prioritizing of coherence in crafting and assessing explanations (B3). The **LCI**, with its centuries-long gap between the postulated biblical event and archaeological chronology, could not be negated without losing coherence with the thick topological and chronological knowledge base, not only of Palestine but of the whole Levant. And once the biblical geographical and topographical information was combined with the requirement that any eligible Ai site should present relevant material remains (B5), the **LHI** followed from the **LCI**, as Albright immediately understood and Callaway later verified.

As long as the case of Et-Tell appeared to be a more-or-less isolated one, it was still rationally possible to reject the **GHI** by modifying the conquest hypothesis. This, however, became less and less plausible as contravening evidence accumulated (A11). When, in the 1970s, this hypothesis came to be viewed as entirely discredited, the relevant epistemic community was still composed mainly of religious Christians and Zionist jews.

I do not mean to portray archaeology, or science in general, idealistically. Scientists' biases, convictions, and non-epistemic values influence their judgments, sometimes strongly so, and the case discussed here is no exception. The complete refusal of the archaeological community (until the early 1950s) to consider the Alt-Noth "nihilist" theory as a possible explanation (A9) is an example, as is Albright's rejection of Starkey's dating of Canaanite Lachish (B2).

I do claim, however, that as long as scholars give precedence to epistemic values such as coherence and explanatory scope they are bound, at the very least, to modify their hypotheses and constrain the gamut of possible modifications. As additional data became available, the same set of values made rejection of the **GHI** untenable to most scholars, regardless of their religious or political sentiments.

For what happens when epistemic values are secondary, see the next section.

7. Knowledge and values

A shared body of knowledge is a necessary, but not a sufficient, condition for scientific consensus. Grintz and Elitzur, Bimston and Livingston—all well-versed in the historical texts and the modern scientific literature—eject the current consensus

opinion, both in its early and local form (the **LHI**) and in its later global form (the **GHI**).[53]

These scholars offer alternative hypotheses under which both the **LHI** and the **GHI** are unwarranted. Both hypotheses (A12) include a revised chronology for the ancient Levant and novel suggestions for the location of biblical Ai. In other aspects they differ,[54] but the similarities are substantial enough that I allow myself to group them under one term: the revisionist thinkers, or simply the revisionists.

Critical evaluation of established theories is essential to scientific discourse, and in this respect such voices should be welcomed. If a widespread and heterogeneous consensus is an indication of knowledge (Tucker 2004), then challenging an established consensus and putting it to the test may pave the way to another and better-established consensus and, arguably, to better knowledge. There is a caveat, though: A challenge can ameliorate knowledge only if epistemic values are hierarchically superior to other values. Judging what qualifies as "better" knowledge is problematic enough when values are shared, but it is hopeless when they are not.

I posit that the fact that the revisionist scholars maintain an array of hypotheses that diverge from the current consensus is wholly explicable by the fact that they hold other values—to wit, adherence to the literal or quasi-literal veridicality of the biblical record - as superior to epistemic values.

This hierarchy is explicit in their writings. Elitzur simply says that "According to the common position among Bible scholars, the Exodus and Conquest actually took place nearly 200 years later … However, this position is inconsistent with the Biblical text" (2014, n4). Livingston asserts that "Whatever the outcome, we maintain that the Bible is truth, and that archaeology is too fragmentary to piece together the whole truth. When archaeology and the Bible seem not to say the same thing, our

[53] One thing that they *did not* challenge or attempt to revised is the **LCI** (that Et-Tell was inhabited during the Middle and Late Bronze age), and the methodology of dating by ceramic typology, on which it was based. Livingston (2003) also makes extensive use of conventional ceramic typology.

[54] One difference is that unlike Grinz and Elitzur, who merely say that ancient Ai "must be somewhere there", Bimson and Livingston offer a concrete alternative and support it with additional auxiliary hypotheses about, e.g., the correct interpretation of Eusebius' text and the Byzantine practices for measurement of intercity distances.

approach is to seek to reconcile them and not to quit until we have." Recalcitrant data, in other words, can never challenge the hypothesis, only the other way around.

That the revisionist hypotheses are loaded with inconsistencies was amply demonstrated by Halpern (1987), Bietak (1988), Rainy (1980, 1988), and others.[55] Rather than repeating their arguments, let me add a few of my own:

- Concerning the Grintz/Elitzur hypothesis, one notes that if we accept (as Grintz convincingly demonstrated) that the toponym "Ai" was not unique but was used elsewhere in ancient Palestine, there is not much support for assuming that the locations of biblical places with similar names (the monarchic " Aiyah " and " Aiyath") or even the same name in later periods (the post-exilic "Ai) were identical with Joshua's Ai. But if they were not, then Grintz' argument from chronology is a non sequitur.
- Concerning Livingston's report of his Khirbet-Nysia excavations, one notes that even if these results are accurate, there is nothing in them to compel identification of Khirbet Nysia with ancient Ai. The claim that the site is a possible Ai-candidate depends crucially on the identification of El-Bireh with ancient Bethel which is, under the most graceful interpretation, very speculative.
- Both Elitzur (2014: 29-30) and Livingston (1970: 27) rely on Josh. 10:2 [56] to claim that the biblical Ai should have been smaller than Gibeon of the same era. Since the assumed site of Gibeon (at current Al-Jib, an identification which they accept) is a small one,[57] this should prove that Et-Tell does not qualify. This argument is an obvious *Petitio Principii*, a perfect example of *vicious* circularity (B2), in a context where the reliability of the biblical source

[55] These articles were written as rebuttals of Bimson and Livingston, but the first two discuss the chronology of the Levant and are therefore a rebuttal of the Grintz/Elitzur version as well. Zevit (1983) criticizes the Grintz/Elitzur rejection of Et-Tell as the site of Ai.

[56] "He and his people were much alarmed, because Gibeon was a great city, as one of the royal cities, and because it was greater than Ai, and all the men thereof were mighty."

[57] In fact it was negligible during the Middle Bronze Age and virtually non-existent in the Late Bronze (Pritchard 1993).

> is itself in question. It is also inconsistent with the biblical texts themselves (Josh. 8:25 mentions 12,000 inhabitants in Ai)

The preponderance of the non-epistemic value of biblical veracity in the revisionist's stance is also apparent in their light-handed dismissal of inference from absence, even when backed by repeated conscientious searches in a confined locality (cf. (B5). Aware that Callaway's excavations exhausted all geographically plausible alternatives for the location of biblical Ai, Elitzur writes: "It is likely that the ruins of Ai can simply be found in a section of Et-Tell that has not yet been excavated, perhaps buried beneath part of the adjacent town of Deir Dibwan. It is critical to stress that the mere lack of archaeological evidence for this claim is no proof of its illegitimacy." Livingston downplays his failure to discover any evidence for construction, fortifications or a destruction layer (2003: 29-30) at his proposed Ai site of Khirbet Nysia by postulating a total obliteration by "later building and agricultural activities" (2003: 15). Building and agricultural activities are indeed part of the history of many archaeological sites in Palestine, but only under special circumstances—which Livingston does not cite—can they result in *complete* removal of all relevant traces.

Advocating relativism about knowledge, Barnes and Bloor (1982) assert that

> Our equivalence postulate is that all beliefs are on a par with one another with respect to the causes of their credibility… For the relativist there is no sense in the idea that some standards or beliefs are really rational as distinct from merely locally accepted as such.

Similar opinions were expressed by several archaeologists:

> Individuals, interest groups, and societies all have different perspectives on the past. There is and can be no monolithic, undifferentiated PAST. Rather, there are multiple and competing pasts made in accordance with ethnic, cultural and gender political expectations (Shanks and Tilley 1987: 11).

In the case discussed here, *all scholars involved* held much the same ethnic, cultural (and, who knows, maybe also gender political) expectations, but most of them

nevertheless allowed their perspectives of the past to be checked and modified by empirical results, while some did not.

The comparison between the two approaches shows the poverty of relativistic thinking. *Not all beliefs are similarly explained.* The difference in the way the revisionists and other scholars handle the same information can only be explained by attributing *different* causes to beliefs: prioritizing religious belief on the one side and commitment—partial and mitigated to be sure, but nevertheless hierarchically superior—to cognitive and epistemic values on the other.

Viewed in this light, the humble mound of Et-Tell / Ai serves as a foil to the relativistic tenet no less than to the content of two biblical chapters.

8. Ground truths

Throughout the second division of this treatise, I referred to typology (and, by extension, to archaeology in general) as knowledge-generating. But knowledge assumes truth; even critics of the TJB construal of knowledge accept that false propositions cannot be known (Ichikawa and Steup 2018).

The idea that science can discover truths has received a lot of bad press. A watershed was Kuhn's critique, epitomized in (1970: 170-6):

> We may, to be more precise, have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth. … Perhaps there is some other way of salvaging the notion of 'truth' for application to whole theories, but this one will not do. There is, I think, no theory-independent way to reconstruct phrases like 'really there'; the notion of a match between the ontology of a theory and its "real" counterpart in nature now seems to me illusive in principle.

A separate worry is epistemic: Even assuming metaphysical truth, can we get to know it? Chang (2004: 46) contends that:

> There are very few actual cases in which we could be confident that we are approaching ''the truth'' by epistemic iteration. Other objectives are easier to achieve, and the degree of their achievement is easier to assess. [58]

Sciences that seek to reconstruct the past are subject to a particular type of pessimism about their ability to point to truths. The separation of the researchers from their subject-matter (since only traces are available) and the impossibility of repeatable experiments are often cited as impediments to the truth-conduciveness of these sciences. Where the *human* past is concerned, other difficulties arise. The most salient ones are the inaccessibility of the agents' inner world on the one hand, and the always-present burden of the researchers' culture and values on the other. Considering these issues, many thinkers came to conclude that the concept of truth is irrelevant for reconstructions of the human past.

I think this is wrong, and the case of Ai/Et-Tell can serve as a rebuttal. Specifically, I am going to claim that in many (though by no means all) questions about the human past the choice between hypotheses can be not only rational but also amenable to a truth-bearing judgment.

My argument focuses on the **LCI** (the inference that the mound of Et-Tell was inhabited during the Late Bronze Age and again during the Early Iron Age, but not during the intermediate period). For my contention, as stated above, the **LCI** suffices because it was non-obvious, contradicted commonly held beliefs, had important implications, and is generally and stably accepted since its first pronouncement in the mid-1930s (A8, B1). It has the advantage of being simple enough to allow an excluded-middle type of argument: either there was a sedentary inhabitation on the site during a particular period, or there was not. The fine-grained details—just what kind of settlement, for how long—matter a lot for the historical reconstruction, but not for the essence of the inference or for the impact that it created, and I shall ignore them here.

[58] In a more recent article Chang (2018: 33) proposes "a pragmatist coherence theory of truth, according to which a statement is true if (belief in) it is needed in a coherent epistemic activity… a matter of degree and circumstances." As far as I can see, this construal of truth retains his former reservations.

As the former discussion should have made plain, there was and is not any coherent or even just consistent (B3) way to negate the **LCI**—to argue, for example that a town stood at the mound during the fifteenth century B.C.—while at the same time retaining the accepted chronology of the ancient Levant. And this chronology, in a much finer resolution than necessary for the **LCI**, is corroborated by literarily millions of pieces of evidence – inscriptions and papyri, sites and strata, sherds and other artifacts – from all over the Middle East as well as from the Aegean and Nilotic realms. Modifying this corpus of data to an extent sufficient to challenge the **LCI** is implausible, and denying its validity altogether calls for an extreme Cartesian-Demon type of skepticism against *any* human knowledge. Unless one adopts such a skeptic stance, one is bound to consider the **LCI** as a true statement about the past.

Contrary to Kuhn's assertion cited at the beginning of this section, then, I claim that the Late-Bronze Age city at Et-Tell *was really there*, and the same is true for the Iron Age settlement and the intermediate gap of sedentary habitation.

It is important to stress that even though coherence plays a central role in my argument, it is not predicated on a coherence theory of truth, or even on a coherentist theory of justification (which has gone out of favor in recent decades, BonJour 1999; Schubert and Olsson 2013; Olsson 2017). Rather, it rests on the commonsensical premises that (a) a patently incoherent set of beliefs cannot be truth-conducive (a claim that both fundamentalist and coherentist usually accept) and (b) *something* must be true about the occupancy of the mound at any time of the past. The truth of the **LCI** then follows from the assertion, defended in length in the previous sections, that the prolonged and iterative process of archaeological research resulted in a coherent and robust system of chronological determination (certainly to the extent of temporal resolution required in this case) of which the **LCI** is the only plausible result. It is, in a sense, an eliminative argument rather than a coherentist one.

If this is correct, then the ability to derive such an unexpected true result through a scientific investigation speaks also against Chang's deep epistemic pessimism.

It is telling that even those who were strongly motivated to object to the **LHI** or the **GHI**, from Garstang and Phythian-Adams in the 1930s to Elitzur and Livingston in the

2000s (A12, B7), have found the **LCI** unassailable. In this respect, the case of Et-Tell/Ai and in particular the **LCI** is a fine example of Laudan and Leplin's (1991) thesis that empirical equivalence and contrastive[59] underdetermination between theories are not inevitable, because it may be practically impossible to formulate a plausible alternative to a hypothesis that has broad epistemic support.

Looking at the other inferences discussed above, one observes that the **LHI** follows from the **LCI** given few simple and widely accepted auxiliary premises (B0): that all alternatives to the location of the biblical Ai offered so far suffer from grave problems of coherence and consistency, while the **LHI** itself coheres well with other evidence-based inferences about the Bronze-Iron transition in Palestine. It is extremely likely, therefore, to be true. The case of the **GHI** is much more complicated: Perhaps an argument similar to the one given here can support a claim for the *falsity* of the Conquest and the Immigration hypotheses. It does not follow from this, however, that the currently believed Autochthonic scenario is true, because other as-yet-unforeseen alternatives may be possible.

As emphasized above (cf. the last three paragraph of B1), not all questions about the human past are amenable to truth-bearing resolution. Many, maybe most, are not. But inasmuch as *some* are, this can be construed as in-principle support for the ability of science to discover substantive mind-independent truths, surprisingly drawn from what is arguably the most special of the special sciences.

Conclusion

This treatise is comprised of two divisions. Each one, I believe, has merit in its own right, but they are meant to be mutually supporting.

The first covers the history of a scientific topic: the whereabouts, identity, and fate of a biblical city. If this part is somewhat lengthy (it is certainly longer than what I

[59] As distinct from holist (Duhem-Quine) underdetermination, which arises from the impossibility to test a hypothesis in isolation from other ones. Laudan and Leplin do not make this distinction between types of indetermination, but it is convincingly made in Stanford (2017).

envisioned when I began this work!) it is because I tried to "give a voice" to the many scholars that were involved and to their beliefs and deliberations.

In the second division, the historical scenario from the first one is used as a template for the analysis of several issues related to the epistemology of the past. In sections B1 and B2, I showed that the iterative process that underlines archeological research can lead to stable and well-substantiated knowledge while minimizing the risk of vicious circularity. Section B3 stresses the central role of the consilience of evidence *combined with* the coherence of explanation in evaluating hypotheses about the past. Section B4 highlights the inadequacy of the Kuhnian framework for the description and understanding of substantial changes in our beliefs about the past. Section B5 clarifies the unique role of inference from absence in archaeology. In sections B6 and B7 I build on the convoluted history of The "Ai-debates" to demonstrate that, contrary to prevailing relativistic theories of knowledge, scientists can and do arrive at conclusions contrary to their former entrenched beliefs and ideologies, but only if their values prioritizes epistemic values over other ones. In section B8 I argue that evidence-based inferences about the past can (sometimes) point to mind-independent truths.

These sections are meant to be self-contained, but there are also mutually linked. They draw a lot, needless to say, from many works by numerous scholars about the epistemology of archaeology and the sciences of the past generally. For my interpretations, observations, and conclusions I am alone, of course, responsible.

Presentism and Whiggism: Studies that use historical episodes as a resource for evaluation of general issues encounter several critiques that deserve to be mentioned here:

Presentism is the study of the past from the vantage point of the present, something that is judged negatively by historiographers (see for example Hunt, 2002). Nevertheless, many scholars of history and philosophy of science have come to see that it is merely an inevitable part of their work (Chang, 2009), and that "taken seriously, such a ban on presentism would appear to preclude not merely the

narration of scientific progress, but all treatment of transitions in the sciences" (Jardine, 2003).

Jardine (2000) and Tosh (2003) argued that a present-centered, retrospective analysis of past scientific works is inevitable and, when properly done, legitimate. In any case, the scientific categories that were relevant here, such as "site," "layer," and, especially, the conceptual framework of archaeological eras (Bronze and Iron Ages and their sub-divisions) were retained, slightly refined but not fundamentally modified, throughout and beyond the period discussed here. Therefore, the question of presentism or anachronism is of little import here.

"*Whiggism*"[60] and "*triumphalism*" are terms that are sometimes used interchangeably to denote (and denounce) looking at the past as inevitably leading to the present beliefs, accentuating the role of the "winners." Chang (2009), however, defines them as conceptual antipodes: Whiggism involves judging past scientific theories according to our modern standards, while triumphalism continues to celebrate what was once victorious in the past. I avoid both traps by following the dynamics of scientific opinions through time, referring at each point to the knowledge available *then*. The largest share of this treatise focuses on the eventual "losers" and shows why their epistemic stances were rational given the evidence they had.

I posit, therefore, that the approach taken here is sound and valid, and the analysis and conclusions in this case study deserve to be judged and evaluated on their own merits.

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Bibliography

[60] Originally coined in the context of British political historiography (Butterfield 1931).

Adams, W. Y., and Adams, E. W. (1991). *Archaeological typology and practical reality: a dialectical approach to artifact classification and sorting*. Cambridge University Press.

Adams, W. Y., Abel, L. J., Arnold, D. E., Chittick, N., Davis, W. M., De Maret, P., ... and Simmons, M. P. (1979). On the Argument from Ceramics to History: A Challenge Based on Evidence From Medieval Nubia [and Comments and Reply]. *Current Anthropology*, *20*(4), 727-744.

Aharoni, Y., and Amiran, R. (1958). A new scheme for the sub-division of the Iron Age in Palestine. *Israel Exploration Journal*, *8*(3), 171-184.

Albright, W. F. (1921). A revision of early Hebrew chronology. *The Journal of the Palestine oriental society*, 1, 4-79.

- (1924). Excavations and results at Tell el-Ful (Gibeah of Saul). *The Annual of the American Schools of Oriental Research*, 4, 1-160.
- (1929a). The American Excavations at Tell Beit Mirsim. *Zeitschrift für die alttestamentliche Wissenschaft*, 47(1), 1-17.
- . (1929b). Progress in Palestinian Archaeology during the year 1928. *Bulletin of the American Schools of Oriental Research*, 1-10.
- (1934). The Kyle memorial excavation at Bethel. *Bulletin of the American Schools of Oriental Research*, (56), 2-1.
- (1935). Archaeology and the Date of the Hebrew Conquest of Palestine. *Bulletin of the American Schools of Oriental Research*, (58), 10-18.
- (1939). The Israelite conquest of Canaan in the light of archaeology. *Bulletin of the American Schools of Oriental Research*, (74), 11-23.
- (1956). Albrecht Alt. *Journal of Biblical Literature*, 74(3), 169-173.

Amiran, R (1963). *The ancient pottery of the holy land: From its beginning in the Neolithic period to the end of the Iron Age*. The Bialik Institute and the Israel Exploration Society. (In Hebrew).

- (1970) The Egyptian Alabaster Vessels from Ai, *Israel Exploration Journal*, (20), 170-179.

Barag, D. (1993). The first "Zabré" archaeologist. *Kadmoniot,*112(2), 118-9. (In Hebrew)

Barnes, B., and Bloor, D. (1982). Relativism, rationalism and the sociology of knowledge. In Hollis, M., and Lukes, S. (eds). *Rationality and relativism*. pp. 31-47. The MIT Press.

Bietak, M. (1988). Contra Bimson, Late Bronze Age cannot begin as late as 1400 B.C. *Biblical Archaeological Review*, 14(4), 54-5.

Bimson, J. J., and Livingston, D. (1987). Redating the Exodus. *Biblical Archaeology Review*, *13*(5), 40-53.

Biran, A. and Nave, J. (1995). The Tel Dan Inscription: A New Fragment. *Israel Exploration Journal*, 45(1), 1-18.

Bloor, D. (1991). *Knowledge and social imagery*. University of Chicago Press.

BonJour, L. (1985). *The structure of empirical knowledge*. Cambridge, Mass: Harvard University Press.

- (1999), "The Dialectic of Foundationalism and Coherentism," in J. Greco and E. Sosa (eds.) *The Blackwell Guide to Epistemology*. Malden, MA: Blackwell.

Broshi, M. (2008). The Israeli archaeology: Real biases and imagined biases. In: M. Feige and Z. Shiloni (eds.) *An ax to grind with: archaeology and nationality in Eretz-Israel*. Ben-Gurion University Press. pp. 19-42 (In Hebrew).

Buchwald, J. Z. and Smith, G. E. (2001). Incommensurability and the discontinuity of evidence. *Perspectives on Science*, 9(4), 463-498.

Butterfield, H. (1965). *The Whig interpretation of history*. (London, 1931). *Google Scholar*, 12.

Callaway, J. A. (1968). New Evidence on the Conquest of Ai. *Journal of Biblical Literature*, 312-320.

- (1969). The significance of the Iron-Age village at Ai (Et-Tell). *Proceedings of the World Congress of Jewish Studies*, 1, 56-61.
- (1976). Excavating Ai (et-Tell): 1964-1972. *The Biblical Archaeologist*, 18-30.
- (1985). Was My Excavation of Ai Worthwhile? *Biblical Archaeology Review*, 11(2), 68-69.
- (1992). Ai. In: D. N. Freedman (Ed.) *The Anchor Bible Dictionary*, Doubleday, Vol 1, 125-130.
- (1993). Ai. In: E. Stern (ed.) *The new encyclopedia of archaeological excavations in the holy land*. The Israel Exploration Society and Carta. pp. 39-45.

Callaway, J. A., Power, W. J. A., Bull, R. J., and Schoonover, K. (1965). The 1964 'Ai (Et-Tell) Excavations. *Bulletin of the American Schools of Oriental Research*, 178, 13-40.

Callaway, J. A., and Nicol, M. B. (1966). A Sounding at Khirbet Ḥaiyân. *Bulletin of the American Schools of Oriental Research*. 183, 12-19.

Callaway, J. A., Harvey, D., Schoonover, K., Ward, J. M., Vine, K., and Livingston, G. H. (1969). The 1966 'Ai (Et-Tell) Excavations. *Bulletin of the American Schools of Oriental Research*, 196, 2-16.

Carroll, N. (2001). On the narrative connection. in S. Benjamin and W. van Peer (Eds.) N*ew perspectives on narrative perspective*. Albany: State University of New York Press. pp. 21-4.

Chang, H. (2004). *Inventing Temperature: Measurement and scientific progress*. Oxford University Press.

- (2009). We Have Never Been Whiggish (About Phlogiston) 1. *Centaurus*, 51(4), 239-264.

- (2018). Realism for realistic people. *Spontaneous generations: A journal for the history and philosophy of science*, 9(1), 31-34.

Chapman, R., and Wylie, A. (2016). *Evidential reasoning in archaeology*. Bloomsbury Publishing.

Cleland, C. E. (2002). Methodological and epistemic differences between historical science and experimental science. *Philosophy of Science*, *69*(3), 447-451.

- (2011). Prediction and explanation in the historical natural science. *British journal of the philosophy of science*, 0: 1-32.
- (2013). Common cause explanation and the search for a smoking gun. *Geological Society of America Special Papers*, *502*, 1-9.

Conder, C. R. (1874). The Survey of Palestine. *Palestine Exploration Quarterly*, *6*(2), 35-64.

- (1881). On some of the gains to biblical archaeology due to the new survey. *Palestine Exploration Quarterly*, 13(1), 34-56.
- (1887). *Tent work in Palestine: a record of discovery and adventure* (Vol. 2). Bentley.

Currie, A. (2016). Hot-Blooded Gluttons: Dependency, Coherence, and Method in the Historical Sciences. *The British Journal for the Philosophy of Science*, *68*(4), 929-952.

- (2017). *Review of Evidential reasoning in archaeology by Chapman and Wylie*. Doi: 10.1086/693878
- (2018). *Rock, Bone, and Ruin: An Optimist's Guide to the Historical Sciences*. The MIT Press. Kindle Edition.

Davis, T. W. (2004). *Shifting sands: the rise and fall of Biblical archaeology*. Oxford University Press.

De Vaux, R., (1969). Critique of "Die Landnahme der israeliten Stämme in der neuren wissenschaftlichen Discusion" par M. Weippert. *Revue Bibl*ique, 86, 272-276.

Dever, W. G. (1993). What remains of the house that Albright built? *The Biblical Archaeologist*, 56(1), 25-35.

- (2003). *Who were the early Israelites, and where did they come from?* Wm. B. Eerdmans Publishing.
- (2011). *Recent archaeological discoveries and biblical research*. University of Washington Press.

Dunnell, R. C. (1986). Methodological issues in Americanist artifact classification. *Advances in archaeological method and theory*, *9*, 149-207.

Dussaud, R. (1935). Note additionnell, *Syria*, 16(4) 346-352.

- (1937). Le nom ancien de la ville de'Ay en Palestine. *Revue de l'histoire des religions*, 125-141.

Elitzur, Y. (1980). The Arab village and the biblical geography. *Teva Va'aretz*, 22(6), 233-237. (in Hebrew).

- (2014). "Between Bethel and Ai" – Identifying Ai. *Makom Baparasha*. 19-30. Yediot Aharonot publication. (In Hebrew; English translation by D. Landman in: http://etzion.org.il/en/parashat-lekh-lekha-"between-bethel-and-ai"-–-identifying-ai).

Finkelstein, I. (1998). The rise of early Israel: archaeology and long-term history. In: S. Ahituv and E.D. Oren (Eds), *The Origin of Early Israel–Current Debate: Biblical, Historical and Archaeological Perspectives*, Ben-Gurion University of the Negev, pp. 7-39.

- (2007). Iron Age I Khirbet Et-Tell and Khirbet Raddana: Methodological Lessons. *Up to the Gates of Ekron": Essays on the Archaeology and History of the Eastern Mediterranean in Honor of Seymour Gitin*. *Jerusalem*, 107-113.

Finkelstein, I., and Na'aman, N. (Eds.). (1994). *From nomadism to monarchy: archaeological and historical aspects of early Israel*. Yad Izhak Ben-Zvi.

Finkelstein, I., and Silberman, N. A. (2002). *The Bible Unearthed: Archaeology's New Vision of Ancient Isreal and the Origin of Sacred Texts*. Simon and Schuster.

Forber, P., and Griffith, E. (2011). Historical reconstruction: Gaining epistemic access to the deep past. *Philosophy and Theory in Biology*, 3.

Fritz, V. (1987). Conquest or Settlement? The Early Iron Age in Palestine. *The Biblical Archaeologist*, *50*(2), 84-100.

Garstang, J. (1931). *The foundations of Bible history: Joshua, Judges*. Constable and Company Limited.

Greenberg, R. (1987). New Light on the Early Iron Age at Tell Beit Mirsim. *Bulletin of the American Schools of Oriental Research*, 55-80.

Grintz, J. M. (1947) "Ai which is with Beth-Aven." *Sinai*, 21, 219-228. (In Hebrew).

- (1961). "'Ai which is beside Beth-Aven" A re-examination of the identity of' Ai. *Biblica*, *42*(2), 201-216.

Guérin, V. (1869). *Description géographique, historique et archéologigue de la Palestine*; *Tome troisième: Judée*. L'Impremerie imperial.

Halpern, B. (1987). Radical Exodus Redating Fatally Flawed. *Biblical Archaeology Review*, *13*(6).

Harding, A. (1999). Establishing archaeological chronologies. In: Barker, G. (Ed.). *Companion encyclopedia of archaeology*. Routledge.

Herr, L. G. (2002). WF Albright and the history of pottery in Palestine. *Near Eastern Archaeology*, *65*(1), 51-55.

Herzog, Z. (2000): Archaeology, the Bible and the Israeli society. *Al-Atar* 7, 9-16. (In Hebrew)

Hull, D. L. (1975). Central subjects and historical narratives. *History and theory*, 14(3), 253-274.

Hunt, L. (2002). Against presentism. President's column in Perspectives: *The magazine for the American Historical Association*. Retrieved June, 16, 2008.

Ichikawa, J. J., and Steup, M. (2018). The Analysis of Knowledge." In E. N. Zalta (Ed.) *The Stanford Encyclopedia of Philosophy* (Summer 2018 Edition), URL = <https://plato.stanford.edu/archives/sum2018/entries/knowledge-analysis/>.

Jardine, N. (2000). Uses and abuses of anachronism in the history of the sciences. *History of science, 38*, 251-270.

- (2003). Whigs and stories: Herbert Butterfield and the historiography of science. *History of science*, *41*(2), 125-140.

Jeffares, B. (2008). Testing times: regularities in the historical sciences. *Studies in history and philosophy of science part C: Studies in history and philosophy of biological and biomedical sciences*, *39*(4), 469-475.

- (2010). Guessing the future of the past. *Biology and Philosophy*, *25*(1), 125-142.

Kitchener, H. H. (through W. F. Birch) (1878). Ai. *Palestine Exploration Quarterly*, 10(3), 132-133.

Kosso, P. (1989). Science and objectivity. *The Journal of philosophy*, *86*(5), 245-257.

- (2001). *Knowing the past: Philosophical issues of history and archaeology*. Humanity books.

Kuhn, T. S. (1970). *The Structure of Scientific Revolutions, 2nd. Ed*. University of Chicago Press.

Kuukkanen, J-M. (2015). *Postnarrativist philosophy of historiography*. Springer. Kindle edition.

Laudan, L., and Leplin, J. (1991). Empirical equivalence and underdetermination. *The journal of philosophy*, *88*(9), 449-472.

Lederman, Z., (1999). An early iron age village at Khirbet Raddana: the excavations of Joseph A. Callaway. Ph.D. thesis presented to Harvard University.

Lewis, D. (1979). Counterfactual dependence and time's arrow. *Noûs*, 455-476.

Livingston, D. P. (1970). *Location of Biblical Bethel and Ai Reconsidered*. Westminster Theological Seminary.

- (1989). Khirbet Nisya 1979-1986: a Report on Six Seasons of Excavation. *Dissertations*. Andrews University.
- (1994). Further Considerations on the Location of Bethel at el-Bireh. *Palestine exploration quarterly*, *126*(2), 154-159.
- (2003). *Khirbet Nisya: the search for biblical Ai 1979-2002: excavation of the site with related studies in biblical archaeology*. Associates for Biblical Research.

Marquet-Krause J. (1934). La première campagne de fouilles a Ay (1933). Rapport sommaire. (unpublished) In: Marquet-Krause, J., through Dussaud, R. (1949). *Les fouilles de'Ay (El-Tell), 1933-1935*. Librairie Orientaliste Paul Geuthner. pp. 7-12.

- (1935a). La deuxième campagne de fouilles a Ay (1934). Rapport sommaire. *Syria*, 16(4) 325-345.
- (1935b). The recent excavations at Ai (Et-Tell). *Yediot - Bulletin of the Jewish Palestine Exploration Society*, 3(3), 73-77. (In Hebrew).
- (1936). Troisième Campagne de fouilles a Ay (1935). Rapport sommaire. (unpublished). In: Marquet-Krause, J., through Dussaud, R. (1949). *Les fouilles de'Ay (El-Tell), 1933-1935*. Librairie Orientaliste Paul Geuthner. pp. 29-34.

Marquet-Krause, J., through Dussaud, R. (1949). *Les fouilles de'Ay (El-Tell), 1933-1935: la resurrection d'une grande cite biblique*. Librairie Orientaliste Paul Geuthner.

Marquet-Krause, J. and Yeivin, S. (1934). The excavations at Et-Tell. *Yediot - Bulletin of the Jewish Palestine Exploration Society*, 1(4), 28-30. (In Hebrew)

- (1935). Et-Tell. *The Quarterly of the Department of Antiquities in Palestine*, 4, 204-205. Oxford University Press.

Mazar, A. (1990). *Archaeology of the Land of the Bible*. The Anchor Bible Reference Library. New York: Doubleday.

McGrew, T. (2014). The argument from silence. *Acta Analytica*, 29(2), 215-228.

Moorey, P. R. S. (1991). *A century of biblical archaeology*. Westminster John Knox Press.

Noth, M. (1935). Bethel und Ai. *Palästinajahrbuch des Deutschen evangelischen Instutut fürAltertumwissenschaft des heiligen Landes zu Jerusalem*, 31(7), 7-29.

- (1938a). Grundsätzliches zur geschichtlichen Deutung archäologischer Befunde auf den Boden Palästinas. *Palästinajahrbuch des Deutschen evangelischen Instutut fürAltertumwissenschaft des heiligen Landes zu Jerusalem*, 34, 7-22.
- (1938b*). Das Buch Josua*. Verlag von J.C.B Mohr.

Olsson, E. (2017). Coherentist Theories of Epistemic Justification. In E. N. Zalta (ed.) *The Stanford Encyclopedia of Philosophy (Spring 2017 Edition)*. URL = <https://plato.stanford.edu/archives/spr2017/entries/justep-coherence/>.

Orton, C., and Hughes, M. (2013). *Pottery in archaeology*. Cambridge University Press.

Petrie, W. M. F. (1892). *The story of a "Tell": A lecture delivered for the Palestine Exploration Fund*. Palestine Exploration Fund.

Phythian-Adams, W. J. (1936). Jericho, Ai and the Occupation of Mount Ephraim. *Palestine Exploration Quarterly*, *68*(3), 141-149.

Sagan, C. and Druyan, A. 1997. *The demon-haunted world, Science as a candle in the dark*. New York: Random House.

Sober, E. (2009). Absence of evidence and evidence of absence, Evidential transitivity in connection with fossils, fishing, fine-tuning, and firing squads. *Philosophical Studies*, *143*(1), 63-90.

Putnam, H. (1981). *Reason, truth, and history*. Cambridge University Press.

Rainey, A. F. (1971). Bethel is still Beitin. *Westminster Theological Journal.*, *23*(2).

- (1980). Redating the Exodus and Conquest (Journal for the Study of Old Testament Supplement Series 5) By John Y Bimson. *Israel Exploration Journal*, 30(3/4), 249-251.
- (1988). Letter to the editor: on the Location of Bethel and Ai. *Biblical Archaeological Review*, 14(5), 67-8.
- (2006). Looking for Bethel: An Exercise in Historical Geography. *Confronting the Past: Archaeological and Historical Essays on Ancient Israel in Honor of William G. Dever*. pp. 269-273.

Robinson, E. (1856). *Biblical Researches in Palestine and the Adjacent Regions: A Journal of Travels in the Years 1838 and 1852, in three volumes*. Murray.

Salmon, M. H. (1993). Philosophy of archaeology: Current issues. *Journal of archaeological research*, *1*(4), 323-343.

Schubert, S. and Olsson, E. (2013). Coherence and Reliability in Judicial Reasoning. In M. Araszkiewicz (Ed.) *Coherence. Insights from Philosophy, Jurisprudence and Artificial Intelligence*. Springer.

Sellin, E. (1899 and 1890). Mitteilungen von meiner Palästinareise 1899. *Mitteilungen and Nachrichten des Deutschen Palästinaverein*, 97-100 (1899); 1-4 (1990).

Shanks, M., and Tilley, C. Y. (1987). *Re-constructing archaeology: theory and practice*. Psychology Press.

Shepard, A. O. (1956). *Ceramics for the Archaeologist*. Washington, DC: Carnegie Institution of Washington.

Sober, E. (2009). Absence of evidence and evidence of absence: Evidential transitivity in connection with fossils, fishing, fine-tuning, and firing squads. *Philosophical Studies*, *143*(1), 63-90.

Stanford, K. (2017). Underdetermination of Scientific Theory. In E. N. Zalta (ed.) *The Stanford Encyclopedia of Philosophy* (Winter 2017 Edition). URL = <https://plato.stanford.edu/archives/win2017/entries/scientific-underdetermination/>.

Trigger, B. G. (2006). *A History of Archaeological Thought*. Second edition Cambridge University Press.

Tucker, A. (2004). *Our knowledge of the past: A philosophy of historiography*. Cambridge University Press.

Turner, D. (2005). Local underdetermination in historical science. *Philosophy of Science*, *72*(1), 209-230.

- (2007) *Making Prehistory: Historical Science and the Scientific Realism Debate*. Cambridge University Press.

Ussishkin, D. (1987). Lachish–Key to the Israelite Conquest of Canaan? *Biblical Archaeology Review*, *13*(1), 18-39.

Van De Velde, C. W. M. (1854). *Narrative of a Journey through Syria and Palestine in 1851 and 1852*. W. Blackwood.

Vincent, L. H. (1937). Les fouilles d'Et–TellL='AÏ. *Revue Biblique*, *46*(2), 231-266.

Wallach, E. (2018). Bayesian representation of prolonged archaeological debate. *Synthese*, *195*(1), 401-431.

- (2019a). Historiographic narratives and empirical evidence - a case study. *Synthese*, DOI 10.1007/s11229-018-02065-w.

- (2019b). Inference from absence: the case of archaeology. Palgrave Communications, 5.

Wilson, C. W. (1869). On the Site of Ai and the Position of the Altar which Abram Built between Bethel and Ai. *Palestine Exploration Quarterly*, *2*(4), 123-126.

Wright, E. G. (1936). The Chronology of Palestine in the Early Bronze Age. *Bulletin of the American Schools of Oriental Research*, (64), 12-21.

- (1937). *The pottery of Palestine from the earliest times to the end of the Early Bronze Age*. New Haven: American Schools of Oriental Research.

Wylie, A. (1992). A hierarchy of purposes: Typological theory and practice. *Current Anthropology*, 33(4), 486-491.

- (1993). A proliferation of new archaeologies: “beyond objectivism and relativism”. In N. Yoffe and N Sherratt (eds.) *Archaeological theory: who sets the agenda*. Cambridge University Press. pp. 20-26.
- (2002). *Thinking from Things: Essays in the philosophy of archaeology*. University of California Press.

Yadin, Y. (1965). *Military and archaeological aspects of the conquest of Canaan in the Book of Joshua*. The Jewish Education Committee of New York in cooperation with the World Jewish Bible Society and the Department for Education and Culture of the Jewish Agency.

- (1982). Is the Biblical Account of the Israelite Conquest of Canaan Historically Reliable? *Biblical Archaeology Review*, *8*(2), 16-23.

Yeivin, S. (1937). The Exodus and the Conquest (current problems). In N. H. Torczyner (Ed.) *Klausner Festschrift*. "Omanut" Publications, pp. 67-78. (In Hebrew).

- (1971**).** *The Israelite conquest of Canaan*. Uitgaven van het Nederlands Historisch-Archaeologisch Instituut te Istanbul.

Zevit, Z. (1983). Archaeological and Literary Stratigraphy in Joshua 7-8. *Bulletin of the American Schools of Oriental Research*, (251), 23-35.