**In Memoriam: Mrs. Joan Huntoon and Dr. Garman Harbottle**

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**APPENDIX 4:**

**TELL EL-DAB`A POTTERY LINE-DRAWINGS TO ACCOMPANY APPENDIX 1**

**WITH A PETROGRAPHIC ADDENDUM BY DR. CHRISTOPHER WNUK**

***The Foreign Relations of the "Hyksos":***

***A Neutron Activation Study of Middle Bronze Age Pottery***

***from the Eastern Mediterranean***

**by**

**Patrick E. McGovern, with a contribution by Tine Bagh**

**BAR International Series 888**

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As explained in the Preface (p. iii) and the “Illustrations and Sample Data” guidelines (p. 98) to this book, Dr. Manfred Bietak was first given the opportunity to publish the line-drawings of the pottery analyzed by Neutron Activation Analysis (NAA) from Tell el-Dab`a (Appendix 1). He has now largely accomplished this in his ongoing multi-volume series devoted to the site’s excavation, viz., *Tell el-Dab'a*, Untersuchungen der Zweigstelle Kairo des Österreichischen Archäologischen Instituts, vols. V (1992), VIII (2012), XII (2004), XX (2010), and XXIII (2013), Denkschriften der Gesamtakademie, Austrian Academy of Sciences, Vienna. The figures are here made available in their entirety, so that they can be more easily consulted by the reader. Four reviewers explicitly pointed out that the omission of the figures impeded their use of the volume.[[1]](#footnote-1)

**New Findings and Hypothesis Testing**

In the nearly 20 years since the publication of this BAR volume, one might expect that some updating of the text would be needed. To the first author’s mind, the most important new findings have come from Ashkelon. In my closing remarks in the book (p. 83), I had already touched upon how pivotal this large fortified Canaanite city-state might be for determining the origin of the Hyksos. It belonged to the “Gaza group of Middle Bronze Age (MBA) sites” (or “Southern Palestine,” *sensu strictu*) that our NAA results showed, at a minimum, had very intense trade relations with Tell el-Dab`a from later Middle Bronze (MB) IIA through MB IIC, the period of the rise to power of the Hyksos in Egypt. At the time, however, one could seriously question this result, because the MBA of Southern Palestine was relatively poorly known, with limited architectural, pottery, and other cultural parallels to Tell el-Dab`a.

The excavation of Ashkelon was a game changer, as I pointed out when the first results of the excavation there started to come in (see below). As a coastal site, it had one of the few natural harbors in Southern Palestine for mooring boats and managing shipments of every kind. And it lay within “striking distance” of Tell el-Dab`a, only about 300 kilometers by land or sea. If we were looking for the “origin” of the Hyksos, even common sense—as defined by the principle of Occam’s Razor that the simplest, most straight-forward explanation is often the right one—might suggest that any increase in the population of Ashkelon and its neighboring Gaza region city-states might have “spilled over,” as it were, into Egypt.

Since Bietak has long argued and continues to argue[[2]](#footnote-2) that the Hyksos came from the northern Levant in MB IIA,[[3]](#footnote-3) specifically Byblos, and since our NAA study showed minimal contacts with this region (and none for Byblos), I proposed to him that we test his theory by his choosing an additional group of samples from Tell el-Dab`a of supposed northern Levantine origin(p. 36). The 29 samples (PMG103-131, Appendix 1: pp. 155-157; Figs. 112-115) primarily included later MB IIA material, continuing through to the end of the MBA, since earlier MB IIA strata had been only minimally excavated. The samples included 20 Canaanite Jars, 2 Polished jars/jugs, 3 Kamares Ware cups, 1 Bichrome Painted jug, and 3 miscellaneous types (a jar, a juglet, and a bowl). Deducing and testing hypotheses are the sine qua nons of any historical science, like archaeology, especially since it is based on extremely limited, fragmentary, degraded, and sometimes contaminated evidence.[[4]](#footnote-4) Based on Bietak’s recommendations, we analyzed the follow-up group of samples. The Canaanite Jars typified the picture of where the imported pottery originated from: 13 of the 20 jars (60%) tested belonged to the Southern Palestine group, fully in keeping with the percentage of imports from this region in later periods, except for MB IIB-C when the percentage rises to 90-100% (p. 73; compare Fig. 26). The northern Levant, including Byblos, did not produce a single NAA match for the group.

I then proposed to Dr. Bietak that we put his hypothesis to yet another test. Since Ashkelon was currently being excavated by Dr. Lawrence E. Stager of Harvard’s Semitic Museum and had already produced what was the best stratified MBA sequence for Southern Palestine, why not carry out NAA analyses on pottery from this site, to see whether the chemistry of local pottery there matched the imported pottery at Tell el-Dab`a assigned to “Southern Palestine?”

Bietak agreed, and Stager provided our project with 50 pottery sherds from his site, again covering later MB IIA through to the end of the MBA. The pottery types, very similar to those of same periods at Tell el-Dab`a, included Canaanite Jars, jugs, juglets, jars, bowls, and cooking pots, together with possible Levantine Painted Ware (LPW), Chocolate-on-White Ware, painted Cypriot White Slip Ware, and Painted Bichrome pottery (PMG518-567; published here for the first time as Table 47, below). Thirty-two of the samples (62%) fell squarely, to a very high level of probability, into our well-established chemical grouping for the Gaza sites, i.e., they had been locally produced using the loess clay of Southern Palestine. If another 9 sherds, which also likely belonged to this group, albeit at a lower probability level, were included, the percentage from Southern Palestine rose to 80%. Three sherds of Southern Palestinian type were made of Egyptian Nile alluvial clay. Finally, 6 samples—2 jugs, a jug or cooking pot, a possible Lisht Ware juglet, a cooking pot, and a rim sherd of uncertain type—were of questionable provenance. The Ashkelon stratified sequence of MBA pottery had passed the test. For all intents and purposes, Ashkelon pottery, made in Southern Palestine, was identical to most of the imported pottery at Tell el-Dab`a.

Based on our original NAA findings of the very close ties of Southern Palestine to Tell el-Dab`a, further corroborated by the two follow-up tests, I concluded that Southern Palestine had indeed been a major trading partner with Tell el-Dab`a, beginning by at least later MB IIA and intensifying in MB IIB and MB IIC.

Only in earlier phases of MB IIA, which were poorly represented for both Ashkelon and Tell el-Dab`a in the NAA datasets, were there any signs of connections with the northern Levant at sites along the coast and inland. I stressed this fact in this book (pp. 35, 52, and 70). Of course, more NAA analyses of earlier MB IIA pottery, now available from more recent excavations at Tell el-Dab`a, Ashkelon, and elsewhere, might show that the northern Levantine connection was stronger then, but the picture for later MB IIA down to the end of the MBA was incontrovertible, according to the NAA results.

In short, my commonsense working hypothesis of the Hyksos being of Southern Palestine origin appeared to be the most likely possibility. This hypothesis was further bolstered by other cultural affiliations between Tell el-Dab`a and Southern Palestine, including handmade cooking pots reflecting a traditional Syro-Palestinian cuisine, mudbrick vaulted tombs with equid interment and nearly indistinguishable burial assemblages in the two regions, etc. (see “Possible Ethnic Origins,” pp. 80-82).

**A Two-Stage Process?**

Our evidence was also consistent with a two-stage process of population movement over the course of the MBA (for what follows, see Chapter 6 and, especially, the general overview provided in my *Ancient Wine* book[[5]](#footnote-5)). Such a scenario was also appreciated by Bietak when he learned of the NAA results showing strong ties between Southern Palestine and Tell el-Dab`a.[[6]](#footnote-6) Imported and locally made MBA scarabs and sealings in the northern Levant versus those found in Southern Palestine provided strong supporting evidence (also see below).[[7]](#footnote-7)

In the first stage during late EB IV (First Intermediate Period of Egypt) and into early MB IIA, people from the northern Levant probably began to move into the southern Levant, which was populated mainly by pastoralists and small villages. The northerners would have brought with them their traditions of architecture, equid burials, pottery styles and technology (e.g., the fast-wheel for throwing the distinctive MB pottery types including Canaanite Jars, highly polished jugs and juglets, and painted vessels in the LPW tradition, etc.). The northerners built fortified sites throughout Palestine and likely integrated the local non-urban people into their socio-political system. They especially consolidated their power in the south, in the Gaza region, which was closest to Egypt.

With an ever-growing population in the Gaza region and ever-increasing trade between the latter and Tell el-Dab`a during later MB IIA and into MB IIB, the stage was set for the second stage of the process. Egypt had gradually descended into political instability, even chaos, at the end of Middle Kingdom and continuing into the Second Intermediate Period, following the collapse of the powerful 12th Dynasty. The eastern Nile Delta, which beckoned with its rich, well-watered agricultural fields, was a natural “relief valve” for a burgeoning Southern Palestinian population, and it was within relatively easy reach by land or sea. Other Semitic peoples, who had long lived there and were engaged in trade and various occupations, could help smooth immigration to the new land, particularly if they had family ties.

Tell el-Dab`a was already a trading entrepôt during the Middle Kingdom, with especially close ties with Byblos as well as other city-states along the northern Levantine coast (e.g., Sidon), and those connections probably expanded to encompass newly established ports farther south (e.g., Tel Ifshar). The “Hyksos” 15th Dynasty, whose capital was at Avaris (Tell el-Dab`a), was the crowning achievement of this process in MB IIB-IIC (ca. 1700-1550 B.C.[[8]](#footnote-8)). During this period, the Tell el-Dab`a population exploded, and the site expanded to some 250 hectares.

More excavation of Tell el-Dab`a and the Southern Palestinian sites is obviously needed to firm up the two-stage hypothesis. For example, it might appear that LPW is largely a “northern Levantine phenomenon,” because of the current distribution of this pottery class at many sites in the north. But as the Ashkelon excavations have begun to show,[[9]](#footnote-9) LPW there is very similar to that from Tell el-Dab`a and follows a comparable stratigraphic sequence in its development, implying close ties between the sites. I would go further and argue for a “Syro-Palestinian” pottery industry at Tell el-Dab`a, which was initially established by immigrant potters from Southern Palestine as early as the late 13th Dynasty and which operated separately from the local Egyptian industry and eventually diverged somewhat from workshops in Southern Palestine. As I write on p. 80 of the monograph:

Early in the MBA, the imitations of Syro-Palestinian pottery types [made of Nile alluvial clay] were indistinguishable, stylistically and technologically, from true imports. By MB IIB, when the relative percentage of imitations also noticeably increases, a range of local "Syro-Palestinian" types has emerged, which were further elaborated upon during the remainder of the MBA, including piriform, biconical, and combed varieties of Tell el-Yahudiyeh jugs and juglets…and piriform and globular painted Tell el-Yahudiyeh juglets.

The available archaeological evidence is very much a product of serendipitous discovery of ancient sites, sometimes buried deep beneath overlying strata, and current evidence can easily be skewed in one direction or another. As I concluded my book on p. 83, 19 years ago: “The clear implication is that MB sites in Southern Palestine have not been sufficiently excavated and/or published, so that [what are now described as] "northern" types [there] are under-represented in distributional studies.” Eventually, specific LPW types may need to be more generally reclassified as “Syro-Palestinian types” or even “Southern Palestinian types.” A similar case might be made for the temple and palace architecture at Tell el-Dab`a, which currently find their best parallels in the northern Levant.

**Does Goren’s Petrography Support Bietak’s Byblos Theory?**

The discussion of a petrographic study by Drs. Anat Cohen-Weinberger and Yuval Goren[[10]](#footnote-10) of the Tell el-Dab`a imported pottery is appropriate here, since their results were very much at odds with our NAA findings. They analyzed several of the same samples that we did by NAA, together with many more comparable imported Levantine pottery types from Tell el-Dab`a. Overwhelmingly, they assigned the imported pottery at Tell el-Dab`a, dated to the entire MBA and not just the earliest phase of MB IIA, to the northern Levant (viz., Lebanon, Syria, and northern Palestine) and central Palestine. Southern Palestine played a minor role for the whole of the MBA, according to their analyses.

The data collection and the methodology on which the Cohen-Weinberger and Goren paper were based, however, are fundamentally flawed. Goren, in collaboration with his student Cohen-Weinberger at the time, was principally responsible for the petrographic approach. The criticisms are many and varied, based on information provided to me by professional geologists and petrographers (specifically, Drs. Christopher Wnuk and Jack Donahue), materials scientists (Drs. Pamela Vandiver and W. David Kingery), and concerned archaeologists (especially Dr. R. Thomas Schaub).

The key deficiencies of Goren’s methodology can be summarized by a series of numbered points, which follow:

1) The first and overriding requirement of any scientific study, including those based on petrography and/or chemical analysis (such as NAA), is to collect their data independently of one another and separate them from the archaeological and textual data and any proposed interpretations derived from the latter. Otherwise, you run the risk of prejudicing proper collection and processing of the data. As described below (#7), Goren has fallen into this trap. The proper procedure, once you have collected your data using a specific technique, is to infer a “working hypothesis” that is then tested further by enlarging the sample database, modifying procedures for greater accuracy, etc. (see further, below). You also tentatively begin to integrate working hypotheses using other scientific methods. Finally, you attempt to fit the various working hypotheses into a general interpretation that does justice to the archaeological and textual data, and continue with your testing, such as the follow-up NAA tests of supposedly northern Levantine imports into Tell el-Dab`a according to Dr. Bietak and the Ashkelon follow-up study. It is the same approach we applied in our ceramic technology program, going from pilot studies to progressively more enlarged, more directed databases (footnote 17 and below).

2) 300+ Tell el-Dab`a pottery samples (“Daba Petrography,” p. 69), about 300 Amarna Letter clay tablets (*Inscribed in Clay*, p. 2), thin-section collections of varying sizes in Israel and abroad (e.g., (“Daba Petrography,” pp. 71-72, *passim*), and an indeterminate number of unpublished thin-sections from on-going excavations were examined petrographically in defining 11 petrographic regional groups for the Levant (“Daba Petrography,” Fig. 1; *Inscribed in Clay*, p. 21). Since the comparative material is largely unpublished or not described in detail in Goren’s or other publications, the reader is asked to accept his interpretation of the thin-sections. As such, Goren’s results are based on a relatively small, ill-defined dataset—the latter being the essential prerequisite for any statistically reliable scientific study—for such a large geographical area (ca. 325,000 km2 or about 125,500 mi2, nearly the size of California). Moreover, inland areas which might be important for Syria, Lebanon, Jordan, Palestine and Israel, except for the West Bank, were omitted from the Levantine groups. It is not known how many samples were assigned to each Levantine group.

3) The primary archaeological data for the samples assigned to each Levantine group were not provided, including site name, pottery type and description (including evidence of production technique, Munsell fabric colors of surface, sub-surface and core fabric related to the original firing temperature, surface treatment such as slip, paint and/or burnishing, any design or inscription, etc.), archaeological locus, date, and present storage location. Such non-petrographic information is important in relating the technology and cultural affiliations of the pottery samples to where they were made, i.e., the provenances of their clays and inclusions.

4) The primary petrographic data for measured characters and features were not provided, including frequency percentages of specific minerals according to well-established and standard point-counting (at least 100­–300 discrete random points but ideally 300-500; N.B.: frequency charts are sometimes adequate for major inclusions) together with their sizes, angularity, any color differences, etc. Pore structure and degree of clay vitrification, as well as the presence/absence of special features (e.g., clay nodules which might point to the mixing of clays), were not noted. Despite the general lack of detailed information, however, some pertinent petrographic information was provided for the Tell el-Dab`a samples (“Daba Petrography,” table 1) and the Amarna tablets (*Inscribed in Clay*, *passim*). As a general rule, the more thin-sections included in the study and the more detailed variables recorded, the better.

Selected thin-section photomicrographs are provided for 19 Tell el-Dab`a samples (“Daba Petrography,” pl. 1) said to derive from a specific Levantine group, but they provide limited information about the distinctive mineralogical and other inclusions that distinguish the Levantine groups from one another. Goren’s petrographic database is also said to include reference raw materials and a collection of pottery thin-sections from southern Levantine sites, in addition to thin-sections for many sites in Syria and Lebanon (“Daba Petrography,” p. 78, note 4), but details are lacking.

5) The essential archaeological, petrographic and other information were not compiled into a readily accessible database for testing and corroborating the 11 Levantine groups by other investigators independently. Raw data that I and others have requested has not been forthcoming.

6) An uncertain number of local clay and mineral samples for the 11 Levantine groups were collected and refired as clay briquettes for preparing thin-sections for comparison with the ancient pottery.

7) In lieu of collecting samples of clays and minerals in the field, geological maps were used as the principal source of possible provenances (“Daba Petrography,” *passim*; *Inscribed in Clay*, pp. 20-21, *passim*). The maps cited, some of which are outdated, often give several possibilities for the mineralogy of a pottery thin-section, and the petrographer might then choose one region over another based on other non-geological, often subjective, criteria, such as what one might expect for a specific period based upon the available archaeological evidence. Once that petrographic profile has been assigned to that region, it might then be applied uncritically to other thin-section identifications, in a circular reasoning fashion.

Goren has used this approach in other studies.[[11]](#footnote-11) It is not known to what extent it was used in the Tell el-Dab`a study. Since the eastern Mediterranean littoral was subject to frequent incursions of the Tethys Sea, as well as drainage from the adjacent hills and mountains, clays there are especially difficult to distinguish from one another petrographically.

8) No heavy mineral separation and analysis, which can often provide further clues for clay provenances, was carried out.

9) Thin-sections were sometimes bypassed in favor of unorthodox petrographic techniques developed by Goren, including “scattered petrographic analysis” (SPA), “peeling,” and “blocking” (*Inscribed in Clay*, pp. 11-12). The goal was to do minimal damage to samples, but the reliability of these non-destructive techniques is uncertain.

10) No rigorous statistical tests, using as many measured petrographic characters and features as possible, were done to define the 11 Levantine groups.

11) The evidence for a “reliability index” in assigning a sample to one of the 11 regional groups, based on the size and the quality of the sample, was not provided, so their actual significance is uncertain. It is also not explained why five indices were used in the Amarna tablets study (“high,” “satisfactory,” “moderate,” “fair,” or “unreliable;” see *Inscribed in Clay*, pp. 14-15) and three in the Tell el-Dab`a study (A: the proposed origin is “highly reliable”; B: “fairly reliable”; and C: “poorly reliable;” see “Daba Petrography,” p. 71) nor how those categories relate to one another.

12) Goren’s use of chemical data, obtained by inductively coupled plasma atomic emission spectrometry and mass spectrometry (ICP-AES and ICP-MS), to bolster his petrographic analysis is methodologically flawed. His ICP comparative database is too small to be of any value in statistically establishing provenances. He is guilty of the same “methodological” errors—small chemical datasets and limited clay sampling—that he accuses me of, albeit more egregiously. His use of a less precise portable X-ray fluorescence instrument in the field to collect chemical data in support of his petrographic results is even more problematic.[[12]](#footnote-12)

Many of these points are further buttressed and elaborated upon in an Addendum by Dr. Wnuk., who has worked for the United States Geological Survey and as a consultant to private exploration companies in the Middle East and around the world since his graduation from Penn in 1984. During his time at Penn and continuing until 1993, he served as my geological petrographer, carrying out studies of pottery from the Baq`ah Valley (Jordan), Beth Shan (Israel), and Wadi al-Jubah (Yemen) projects. The latter investigations integrated petrography into a pioneering ancient ceramic technology program at the Museum Applied Science Center for Archaeology (MASCA).[[13]](#footnote-13)

Dr. Wnuk’s critique is based on the data collection and methodology provided by the Cohen-Weinberger and Goren paper, together with a more extended and thorough methodological discussion in the Goren and colleagues’ book on the Late Bronze Age (LBA) Amarna Letters, *Inscribed in Clay[[14]](#footnote-14)* (to which theCohen-Weinberger and Goren paper makes reference for clay provenancing) and other related Goren articles. The Addendum is not meant as an exhaustive catalogue of every mistake, misinterpretation, or ambiguity by Goren of geologic methodology, interpretation, and terminology, because their sheer number is enormous and would overwhelm the non-geologic reader. Several trenchant comments by Wnuk (personal communication by email, March 18, 2019) as he worked his way through the various Goren manuscripts perhaps best sum up his assessment of Goren and his colleagues’ work: “This is worse than I ever expected. I literally have a page or two of [negative] comments per paragraph in some places…They have built a house of cards that is on a shaky foundation.”

This opinion was seconded by another geological petrographer, Jack Donahue of the University of Pittsburgh, who was on the staff of the Bab edh-Dhra` Project and past editor of the Journal of Geoarchaeology. He emailed me on July 23, 2004 to say that he had reviewed a 1996 article by Goren[[15]](#footnote-15) on EB IV pottery from the southern Levant, which greatly concerned him and Dr. Schaub, as follows:

I have gone over the ms by Goren and can make the following comments.

1. Goren is not a geologist and as far as I can tell does not have any geological training. He is self-taught and has learned to look through a petrographic microscope.

2. I would really need to look at the same thin sections he is describing in order to know if he is accurately describing what is present in the thin section. He is using jargon and terms as though he knows what he is talking about but I am not convinced.

3. Look at Fig. 2 in the paper that you sent to me. All he has done is to plot the values of cerium vs. strontium and cerium vs nickel from a small number of analyzed samples and drawn his own lines around what he has chosen as his clusters. If you look at the plots without his lines, the only group that stands as a cluster is the Taqiya Marl but that is only on the cerium strontium plot - not the cerium nickel plot.

4. He is trying to overwhelm the reader with a barrage of terms. Just as one example, he uses the term arkose (Fig. 5, page 53) and describes large angular grains of arkose derived from granite. Arkose is an old, little used term for a sandstone derived from a granite source rock. Even if the term were still in common use, angular grains of arkose would not exist, especially not in pottery.

5. In the discussion of Taqiya Marl + Grog, (page 52) the last sentence of the Characterization section, "The wadi sand temper is rather variable and contains calcareous rock fragments, chert and quartz in differing proportions" is almost a direct quote from my paper with Diane Beynon, Tom Schaub and Bob Johnston (1986). Goren lists our paper in his bibliography and may have lifted part of it for his paper.

6. I could go on with numerous other examples. As far as I can tell, the clay source areas were not visited by Goren. Instead, he used some general and old references (Bender, 1974, Geology of Jordan) to identify (?) clay sources. You are absolutely right in your criticism. He should be doing point counts for each thin section with on the order of 200 to 300 points per sample. He should be using multivariate statistical analysis to sort his data into groups that are similar to each other.

You are correct in criticizing his work. It is very poorly done, and you cannot draw any valid conclusions from it.

Similarly, Dr. Vandiver of the University of Arizona emailed me on June 23, 2004 that “Goren in his plaster work basically said that he could not find any [Neolithic] plasters [derived from limestone, but] only clay plasters in his sample[s], and that because he could not find any, we [including Dr. Kingery] must be wrong. He later told me that he had found some [calcite] plasters, but he did not retract his original sting [i.e., a vitriolic criticism].” Vandiver later told me (oral communication at the Luoyang (China) Early Writing Conference, October 2000): “Goren must be stopped.”

In short, Goren’s petrographic methodology falls far short of current practice in archaeological petrography, which stresses data collection, sampling of clays and other raw materials within a circumscribed region or site, firing briquettes of these local raw materials and comparing their thin-sections to the unknown ancient pottery samples, etc.[[16]](#footnote-16) Admittedly, point-counting and statistics have not been well-integrated into some of these current programs.

It is also instructive to compare the petrographic approach favored by New World archaeologists, which produced results contrary to those obtained by NAA.[[17]](#footnote-17) In this instance, unidirectional trade relations were unequivocally shown by NAA analyses to have existed between the Early Formative Olmec “mother culture” at San Lorenzo in southeastern Mexico and its outlying “sister cultures.” This debate is a mirror-image, as it were, to the Old World controversy about the imported pottery at Tell el-Dab`a’s primarily originating from Southern Palestine according to our NAA results, rather than from sites farther to the north according to the petrographic results.

**Contrasting Results**

As if to drive home these points, I recently uncovered in my files the original manuscript that Goren sent me in April 1997, before I left Philadelphia to complete and submit the final manuscript to the Austrian Academy of Sciences during my stay at the Institute of Egyptology at the University of Vienna in May. As it turned out, it proved impossible to adequately address key issues of debate between the petrographic and NAA results at that time, so it was decided to resolve those issues and publish a joint article in *Ägypten und Levante*. The specific issues, which were not answered by Goren, are listed in an email that I sent him on April 16, 1997:

Dear Yuval,

Just a few more comments as you do your editing (and enjoy the holiday):

1) Are you able to say anything more precisely about possible tempering for some of the groups, based on binormal size distribution of inclusions, crushing of the temper, etc.?

2) What about possible firing-ranges, based on green/brown hornblende, quartz fracturing, etc.?

3) I note in your original table that you assign PMG120 and 121 to the Jezreel Valley. Are you arguing that this is a sub-group of the larger basaltic, foraminiferous marl group, which is variously found in the Galilee and upper Jordan Valley? Basaltic gabbros outcrop in northern Syria, which I believe is a better possibility for JH113.

4) The end of your chapter seems to trail off and is confusing--you must have been getting tired at this point, or just wanted to send me the "bulk" of the article. Since the lower Cretaceous clay is so widely distributed, how can one know whether samples come from southern Lebanon, Transjordan, etc.? The Arkose group is referred to in \*\*\*-footnote(?), but not discussed. The discussion of central Negev wares seems peripheral.

5) The Motza (or Moza?) group is very important, and I am still a bit mystified by it. You say that there are 6 samples belonging to it, but I can only come up with 5 from the table. The fact that the samples (JH013, JH084, JH091, PMG117. and PMG122) are all possibly or definitely from Southern Palestine or the mid-coastal region also poses a problem that will need to be addressed in the "Discussion." Is it possible that dolomitic sands could have been washed down into the Shephelah?

6) The geographical limits of the Taqiya group are confusing--you discuss at length the unique predominance of the fabric at Har Yeruham, but then point out its prevalence at Jebel Qa`aqir. How are these findings to be reconciled with the assignment of this group to the Shephelah? You suggest that more evidence for the latter region will be provided, but I don't see it.

7) It is not clear which sample belongs to the "Hamra" group. Does the absence of chalk, nari, and chert distinguish it from the Terra rosa group?

In general, I think that things will be clearer once you've had a chance to add the Dab`a sample numbers for each petrographic group. It would also be good to clearly indicate the full range of possible geographic locations for each group. That will make it easier to make realistic correlations with the NAA results, which also cast as wide a "net" as possible.

Best, Pat

It will be noted that at the time I raised many of the same methodological problems discussed by Chris Wnuk in his Addendum.

While recognizing that Goren’s petrographic assignments might have changed or been refined between 1997 and 2004 after he began working with his doctoral student, Anat Cohen-Weinberger, and after more thin-sections had been analyzed, it is again instructive to compare his 1997 petrographic results with those of the final 2004 publication, together with the NAA assignments cited from this monograph, published in 2000. The comparisons, showing many disparities, are compiled in Table 48, below.

What immediately jumps out at the reader when examining the comparisons are how often the Goren 1997 petrographic results differ from those of 2004 (see column 7, far-right in bold type). Omitting 23 samples that were not reported as being analyzed in 1997, 34 of the assignments (79.1%) made in 1997 have been reassigned in 2004. Only 9 samples (2.1%) remained the same (including one that was possibly positive and two that were probably positive).

Of the reassignments, those relocated from the Southern Palestinian region (including the “Southern Coastal Plain” and the “Shephelah”) in 1997 to various regions in the northern Levant (viz., “Lebanon east of the coast line Beirut-Byblos,” the “Northernmost Israeli coast or the Lebanese coast,” “Northwestern Syria,” the “Northernmost Israeli coast or the Lebanese coast,” the “Middle Orontes north of Qedesh,” the “Northern Lebanese coast [north of Tripoli],” the “Mt. Carmel region,” etc.) in 2004 stand out as accounting for the greatest number of changes: 19 samples (44.2%). By contrast, only 4 samples (9.3%)--registration nos. 4107A, 4503, 4536, and K3456--were retained as originating from the Southern Palestinian region. Another 4 samples-- registration nos. 4505A, 4551F, K2567, and K3656—went from being Southern Palestine in 1997 to “Central coast of Israel” in 2004.

The fluidity of petrographic assignments is especially apparent when what was initially assigned to the southern or mid-coastal Palestine in 1997 have been reassigned in 2004 to Egypt (registration nos. 4550B, 4549A, and 4777), and vice versa when other samples (registration nos. 2532C, 4539, 4549C, and ?/JH672) that are said to come from Egypt in 1997 are reassigned in 2004 as coming from the “Lebanon east of the coast line Beirut-Byblos,” the “Northwestern Negev,” the “Northernmost Israeli coast or the Lebanese coast,” and the “Lebanon east of the coast line Beirut-Byblos,” respectively. Only 2 samples (registration nos. 4548C and K2810A) were assigned to Egypt in both 1997 and 2004. While one of the latter samples (no. K2810A) was in accord with the NAA result, the other (no. 4548C) was not. Note, too, that the 1997 Egyptian assignments are mostly in agreement with the NAA results, as opposed to those for 2004 (compare columns 4 and 6).

The changes between 1997 and 2004 of samples assigned to the “Central and Southern Hill Country” to elsewhere, especially the northern Levant, also undergo changes. Only one sample (registration no. 4099C) remains the same, as opposed to 7 others (registration nos. 4551C, 4537, 4537A, 4630D, 5826, 5894, and 5894C), which were variously assigned in 2004 to “Lebanon east of the coast line Beirut-Byblos,” the “Northernmost Israeli coast or the Lebanese coast,” “Undetermined northern Levant,” the “Northern Lebanese coast [north of Tripoli],” “the Eastern Galilee or the Yarmuq area, or the Akkar or Middle Orontes north of Qedesh,”or “Northwestern Syria: Ugarit, 'Amuq area or the Cilician coast.”

There appears to be little rhyme or reason to why the provenance changes were made between 1997 and 2004. They are troubling, because they have gone unexplained by Goren or Cohen-Weinberger. How could so many sample assignments made in 1997, especially for those supposedly from the southern Levant (in line with the two-phase hypothesis), have been reassigned in 2004 to the northern Levant (in keeping with Bietak’s theory)? Moreover, the evident flip-flops in whether a vessel was made of an Egyptian or Levantine clay are similarly disconcerting and raise the issue of how seriously these petrographic assignments are to be taken in general.

Dr. Cohen-Weinberger’s unpublished doctoral dissertation[[18]](#footnote-18) is similarly deplete in providing essential data to explain the disparities, apart from brief descriptions and single photomicrographs for each sample petrographically analyzed. Hence, there is no way to independently assess the statistical basis of the assignments. Her methodology in the dissertation suffers from the same deficiencies as those enumerated for Goren’s studies (#’s 1-10, above).

**Were the NAA Results Incorrect?**

By contrast to the limited data collection and weak methodology of Goren’s approach, the NAA database and methodology were “data-heavy” and very transparent and compelling for their group and site provenance assignments, as follows:

a) 608 well-provenienced and well-dated MBA pottery samples from Tell el-Dab`a, as well as clay bed samples from throughout Egypt, were analyzed;

b) 810 pottery samples, primarily MBA in date and generally well-provenienced and well-dated, from 55 coastal and inland sites of Syria, Lebanon, Jordan, Israel, the West Bank, and Gaza Strip, together with numerous clay bed samples from the same areas, were analyzed. Where local clay deposits were lacking, mudbricks, cooking pots, and wasters of probable local origin were analyzed and tested for local compositional group membership;

c) 4583 well-provenienced and dated pottery and clay samples from elsewhere in the ancient Near East covering other periods were analyzed.

Unlike Goren’s petrographic studies, the primary archaeological and NAA data are fully published in this book and on-line for the samples run by the Missouri lab and those in the Brookhaven Old World database ([www.archaeometry.missouri.edu](http://www.archaeometry.missouri.edu)).

The NAA data provided by these 6001 samples were then subjected to rigorous statistical analysis to establish site-specific and regional groups to high degrees of probability. Powerful multi-variate algorithms, based on as many as 35 chemical elements at part-per-million levels, served as independent variables in our NAA study. The accuracy of the data and statistical criteria were so stringent that there was, amazingly, **a nearly 0% probability that the 268 pottery vessels comprising the Gaza group of imported pottery at Tell el-Dab`a were misassigned** (p. 25). That finding alone should have been reason enough to reject the petrographic results.

The first pilot study of the Baq`ah Valley (Jordan) Late Bronze (LB) pottery that I did with Dr. Wnuk (see footnote 17a),nearly 40 years ago, demonstrated the advantages of integrating detailed archaeological information with that obtained by the complementary scientific techniques of petrography and NAA. We collected eight clay samples and six sand/sandstone samples from the valley for comparison to the ancient pottery, which was dominated by quartz inclusions. We employed random point-counting of inclusions. The statistical groupings of the fully detailed petrographic characters and features observed were quite comparable to those obtained by NAA. Later, we expanded the study to include a total of 58 pottery samples from a well-provenienced and well-dated continuous sequence of LB and early Iron Age phases (see footnote 17c). The results from the pilot study were confirmed. Other scientific techniques, especially xeroradiography, interrelated the materials properties of the pottery fabrics with formation techniques. For example, as the fabrics became coarser in LB II, possibly because of mass production, hand-made slab bases for bowls and kraters, rather than wheel-thrown bases made in the upside-down mode, became the rule. This development became even more prominent in Iron IA at the same time that a new cultural and technological constellation was taking shape (the results and working hypotheses are summarized in reference e of footnote 17.

By stressing the advantages of NAA for provenancing the clay origins of Levantine pottery over that of petrography, I have no intention of depreciating the value of the latter. Petrography, properly practiced and applied, is essential to evaluating the NAA results and supplying essential technological information, which NAA cannot provide. Petrography was a critical component of our ceramic technology program, which combined a coordinated series of scientific approaches to elucidate the technological, environmental, and cultural underpinnings of pottery manufacture (see, especially, the flow diagram in reference e of footnote 17). For example, xeroradiography sheds light on formation processes--whether by hand, slab/coils or wheel--and sequential firing of briquettes made from local clays and minerals, coupled with scanning electron microscopy to observe vitrification of clay particles and other changes in the pottery fabric, enabled the original firing temperature of the ancient pottery to be determined. Petrography was integral at every stage of­ the investigation in providing evidence of the workability of the clay for a given production method, recording mineralogical changes with increased firing temperatures, establishing preliminary fabric types for the archaeological corpus, and more. This holistic approach to ancient pottery technology, in conjunction with other archaeological, textual and environmental data, enabled us to propose novel working hypotheses for further study. I’ve already mentioned the dramatic cultural and technological changes on the central Transjordanian plateau during the 500-year transition from the LBA to the early Iron Age. Those developments contrasted sharply with a continuity of traditions on Thailand’s Khorat Plateau from the 3rd to the 1st millennium B.C. (see footnote 17e).

Ideally, the goal of a pottery provenance study is to determine the source of the ancient clay from which the pottery was made. Yet, petrography cannot characterize and localize the clay per se despite this possibility and all its other advantages for understanding ancient pottery technology. For that, a highly sensitive and precise chemical technique like NAA is usually needed, “tempered” as it were by petrographic analyses. The NAA elemental results can be skewed by the washing out of native inclusions or the addition of foreign inclusions from a non-local, more distant mineralogical deposit by natural geological processes, such as erosion, which can produce highly selective dilution or concentration effects for the native clay. Inclusions might also have been intentionally mixed with the clay, as tempering agents, by the ancient potter, to produce a more workable clay or better-firing product. Petrography, by comparing thin-sections of fired clay briquettes of local clay and minerals with those of the ancient pottery, provides the all-important check on such variables.

Even at one remove from the clay bed itself, however, petrography can sometimes work better than NAA provenancing ancient pottery. For example, when a region has a very distinctive petrology, such as an igneous regime, the inclusions in the clay might be sufficient to characterize and source the clay bed. The Levant, however, does not meet that criterium, as already explained and further elaborated upon in the Addendum.

To determine the origin of a Levantine clay, which might differ watershed by watershed along the Levantine coast, intensive chemical analytical methods, such as NAA, are needed. As I stated on pp. 70 and 73 of this monograph: “A gradual increase in transition metals (particularly chromium, manganese, and cobalt), alkalis and alkaline earths, and, to a more limited extent, in the rare earths are observed in the clays and the local compositional groups going from south to north along the Mediterranean coast. This trend is even more pronounced than it might otherwise seem, since the pottery from northern sites were tempered more heavily with calcite, which has a diluent effect on chemical composition.”

It should be noted that, in lieu of petrographic details for most archaeological sites in the Levant, correction for calcium dilution of other elements by various mathematical procedures—viz., best relative fit by least squares—did not improve the statistical discrimination of local groups using the uncorrected NAA data. As more petrographic analyses are carried out, it should be possible to identify distinctive mineral suites for specific local group samples by percentages of inclusions, including quartz and related silicate minerals of igneous origin, so as to apply correction factors. It is doubtful, however, that these corrections would significantly improve the local group separations of this study.

Admittedly, the more visual, lower-tech approach of petrography is more readily understood by archaeologists, who are generally not trained in the natural sciences, and if the results are in accord with preconceived ideas, they will be more likely accepted than results based on sub-microscopic chemical elements and sophisticated statistical arguments. The much lower cost of petrography in comparison to NAA, which requires a nuclear reactor, is also a consideration. However, when weighing the relative merits of the NAA evidence for MBA pottery provenances vis-à-vis the results obtained by petrography, even as a non-specialist, consider this: how much reliance would you put in a radiocarbon date that was based on very imprecise data and no statistics?

In contrast to three generally favorable reviews, two reviewers[[19]](#footnote-19) of the book failed to appreciate the relative merits of NAA for Levantine pottery provenancing over those of petrography. Both were members of the Tell el-Dab`a team, Goren himself as the Tell el-Dab`a petrographic specialist, and Dr. David Aston, the long-time pottery consultant for the Tell el-Dab`a project. Neither was well-qualified to assess the methodology and results for the NAA analyses, since Goren had been trained as an archaeologist and petrographer and Aston as an Egyptologist. By contrast, the review by Dr. Hector Neff, an accomplished NAA practitioner and archaeologist, should carry more weight. Additionally, the reviews by Stephen Bourke (a Palestinian archaeologist, who directs the excavations of MBA levels at Pella), and Dr. Linda Hulin (an Egyptologist and archaeologist) pointed out both the strengths and weaknesses in my book, nearly all of which I agree with (for references to reviews, see footnote 1).

Both Goren and Aston’s critiques concur in stressing that the NAA data coverage for the northern Levant was relatively poor for the NAA study. Yet, I admitted in the book that our NAA coverage of the northern Levant needed to be expanded, and I qualified my conclusions accordingly. What both reviewers overlooked was that petrographic delimitation of northern Levantine regional groups was much more severely imprecise due to a lack of archaeological, petrographic and raw material data, as well as unfounded geological assumptions, than were the NAA groups for the same region (for details, see above and the Addendum).

Goren claimed that “this entire databank [of the Brookhaven and Missouri NAA labs] is of little significant value,” and advocated rechecking the NAA results against the “better-selected” database of the Lawrence Berkeley lab (footnote 23, p. 109). He concluded: “In its present state, McGovern’s conclusions should be treated with much skepticism and reservation.” Dr. Michael Glascock, director of the Missouri lab, and his associate Dr. Hector Neff do not agree, nor is it likely that that Mrs. Joan Huntoon and Dr. Garman Harbottle at the Brookhaven lab, who ran many of the Levantine and Old World samples, would if they were still alive.

**Goren’s “Petrographic Origin” for the Scorpion I Jars from Abydos: Caveat Emptor!**

Another instructive example of how Goren’s methodology can lead to implausible results and an unworkable hypothesis is his and Dr. Naomi Porat’s study of the Scorpion I wine jars in tomb U-j at Abydos, which belonged to one of the first kings of ancient Egypt (Dynasty 0) near the beginning of the Early Bronze Age (EBA).[[20]](#footnote-20) The NAA results for 18 samples[[21]](#footnote-21) unquestionably pointed to 15 of the jars being made of southern Levantine clays, centered on the Jordan Valley and the adjoining hill country plateau to the east and west in Jordan and on West Bank. Two additional samples possibly matched clay farther south along the Rift Valley, while a third was possibly made of the loess clay of the Gaza region (i.e., Southern Palestine). Not a single Egyptian clay was even remotely related to the Scorpion I jars, although both marl and alluvial clays from the entire country were very well-represented in the database.

The southern Levantine origin of the jars in Scorpion I’s was in good agreement with other findings, which provided collaborative evidence that the NAA results were correct. Organic residue analysis showed that the jars had originally contained a resinated grape wine,[[22]](#footnote-22) which had been liberally infused with southern Levantine herbs (balm, senna, coriander, germander, mint, sage, and/or thyme) that did not grow in Egypt (except possibly senna and sage).[[23]](#footnote-23)

Most tellingly, neither the wild nor the domesticated Eurasian grape (*Vitis vinifera*) grew in Egypt at this early period.[[24]](#footnote-24) Any wine therefore had to have been imported from abroad, and, in keeping with Occam’s razor and common sense, the southern Levant was the nearest and most likely possibility. According to archaeobotanical findings at sites in the Jordan Valley, a wine industry had been established there as early as ca. 4000 B.C., and large-scale production is attested in the EBA at sites along the Transjordanian eastern side of the Dead Sea.[[25]](#footnote-25) A special peculiarity of the wine in the Scorpion I jars was that 11 of them uniquely contained sliced and perforated figs, and figs were especially prominent in the botanical assemblages at the Dead Sea sites.

Only several hundred years later did the Egyptians initiate their own wine industry in the Nile Delta, probably under the tutelage of Levantine peoples, later denoted as the Canaanites. One might again argue for a two-stage process in the transfer of the wine industry from the Levant to Egypt, albeit over a millennium earlier than the Hyksos phenomenon. First, the domesticated grape and vinicultural expertise were transferred from farther north in the mountainous Levant, where the earliest evidence of winemaking is ca. 6000 B.C.,[[26]](#footnote-26) to the Jordan Valley and environs several thousand years later. In the second phase, the grape domesticated and necessary technology for winemaking were introduced into Egypt after approximately another thousand years, ca. 3000 B.C.

The style of the Scorpion I wine jars themselves supplied further clues about where the wine had been made, on the reasonable assumption that both the pottery and the wine originated from the same general area. Their decorations of smeared red and white slips, narrow painted bands, or dramatic, swirling “tiger-stripes” set them apart from anything made in Egypt. Only one time period and area fit the bill: an early phase of the EBA(specifically, EB IB or Naqada IIIa2), at sites in the vicinity of Gaza on the southern Levantine coast, in the inland Jezreel and Jordan Valleys, and in the hill country of Transjordan, as far south as the Dead Sea sites of Bab edh-Dhra` and Numayra.

The southern Levantine stylistic affinities of the jars were further corroborated by numerous clay sealings scattered in and around the jars. They had apparently been originally attached as labels by strings that held down an organic cover, such as leather, over the mouth of each jar, which then disintegrated and caused them to fall to the floor. The sealings displayed finely cut cylinder seal impressions of distinctively non-Egyptian types, which combined free-flowing designs of animals (including antelope, fish, birds, and snakes) with geometric patterns. A thorough search of the archaeological literature revealed no exact matches for them, but the closest parallels pointed again to the northern Jordan Valley and the eastern shore of the Dead Sea.

With such a wealth of evidence from a wide range of disciplines—NAA, organic residue analysis, pottery typology, art history, etc.—for the importation of the Scorpion I wine jars from the southern Levant, it came as a shock when Porat and Goren claimed that of “about 130” jars that they examined petrographically, all were made from a local arkose clay bed in a single workshop, likely at Abydos itself. The assignment of samples to more specific groups was made by comparing the Scorpion I thin-sections to a database of some 8000 clay and pottery samples from the southern Levant. But any comparisons for that region were dismissed in favor of a geological map showing that the requisite clay was located in the Abydos area. It is said to be a marl containing arkose. As Goren emailed me in January 1997: “Minerals of igneous origin can be derived, as single fine sand particles, by aeolian forces inland. However, in the case of the pottery from Abydos we are dealing with arkose, that is to say very coarse matter with granitic particles sizing 3 mm and sometimes even more. The particles are angular, indicating a short distance from their mother rock.”

Yet, as Dr. Wnuk expands upon and makes clear in his Addendum, Goren appears not to understand what arkose is in geological terms In a March 10, 2019 email, he writes:

Reading through the paragraph on the arkose was the most painful experience of all. It is pretty clear that whoever wrote this is clueless. It has been a while since I read so much pure nonsense. There is so much wrong with this paragraph, I don’t know where to start. I could probably generate 10 pages of criticism on this paragraph alone. The statement that arkose derives from a weathering granite – wow! Yes arkose is often generated from decomposing granite, but any rock that contains feldspar will generate an arkose. The arkose in the Furness building [at the University of Pennsylvania] comes from a diabase which is a course grained version of a basalt (which is kind of the opposite of granite).

The issue of arkose in Goren’s analyses is not restricted to Abydos. It has seemingly played a large part in many of Goren’s petrographic analyses. For example, Goren claims that Moza clay from Jerusalem and the central hill country of the West Bank was used to make EB IV pottery found in the Negev, some of the imported MBA pottery from Tell el-Dab`a, and a group of LB Amarna tablets.

As was my usual practice when the NAA results appeared to disagree with another method’s results, I proposed to Goren that we run some of his "Abydos" samples. When we did, they again matched the southern Levant profiles. In an oral communication, Goren told me that he had made a mistake with those particular samples, but remained adamant that the other samples, not tested by NAA, were made of the local Abydos clay. We decided to conclude our attempt at reconciling the divergent results and publish our results separately from one another.

If Goren was unconvinced by the NAA results for the southern Levantine origin of the Scorpion I wine jars , then one wonders whether a follow-up study by another petrographer, Dr. Mary Ownby, that disprove his hypothesis is more persuasive?[[27]](#footnote-27) Regrettably, the latter study is also methodologically flawed by its very small database (a total of 20 sherds for several EB periods and only four U-j jars, also analyzed by Porat and Goren). No clay or mineral collections were done in the field for comparison, but instead, geological maps were referred to in establishing the putative region of origin, viz., the northern Levantine coastline, specifically Byblos. Since most of the text was written by an associate of Bietak at the Institute of Egyptology at the University of Vienna and published in his journal, it is perhaps to be expected that they would discover their provenances in this region. While I might wish it were so, having long argued that the Canaanites and Phoenicians of this region were the principle conduit for viniculture throughout the Mediterranean (most recently, in the new edition of *Ancient Wine*5), the NAA results show no matches, not even a slight chemical hint, for a northern Levantine provenance. Admittedly, however, samples were taken from different jars in the follow-up study by Dr. Ownby than those tested bt BAA. Meanwhile, the evidence for winemaking and pottery and sealing parallels to the Scorpion I jars continue to accumulate for the southern Levant.

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Unfortunately, many others, especially those in the Bietak/Dab`a camp but also including other archaeological petrographers,[[28]](#footnote-28) have uncritically accepted Goren and Aston’s arguments and conclusions. The latest critique of the NAA results in favor of the petrographic viewpoint comes from Bietak in his review[[29]](#footnote-29) of A.-L. Mourad’s published Ph.D. dissertation from the Ancient Studies Department of Macquarie University. Bietak points out that Mourad approaches the Hyksos question of origins from an archaeological and textual standpoint. He should have gone further to say that Mourad was trained as an Egyptologist and had little training in the natural sciences. As such, she is poorly qualified to pass judgment on whether the petrographic results should take precedence over those based on NAA. Yet, she accepts without question, based only on the critiques of Goren and Aston, the petrographic conclusion that the primary ties of Tell el-Dab`a were with the northern Levant throughout the MBA. Bietak has no problem with this position when he writes “the author stresses that there is no evidence for a southern Levantine origin.” Mourad now holds a postgraduate position at the Austrian Academy of Sciences under Bietak.

**Does Bietak Now Agree with the NAA Results?**

Strangely, Dr. Bietak appears to agree with the principal conclusions of the NAA study in his review of Mourad’s book when he writes: “Only in the late 13th Dynasty did imports from the southern Levant arrive in growing numbers, until during the Hyksos Period the percentage imported from the north fell dramatically.” This statement accords with the founding of Ashkelon and other major city-states in the Gaza region earlier in MB IIA, followed by the growth of Tell el-Dab`a later in the period with the emergence of close trading ties with Southern Palestine. Bietak’s statement, however, goes against the petrographic data and their interpretation that the northern Levantine influence on Tell el-Dab`a far exceeds that of Southern Palestine throughout the MBA, from early in MB IIA and continuing through the Hyksos 15th Dynasty up until the end of the period.

Bietak’s apparent about-turn fits with his accommodation to or denial of scientific data when they are in accord or disagree with his strongly held theories. As another example, he is willing to accept physical anthropological evidence from Tell Kamid el-Loz in the Beqaa Valley of Lebanon supposedly showing a close genetic relationship between Iron Age males of that northern region with the MBA Tell el-Dab`a male population, who then married local Delta women.[[30]](#footnote-30) Yet, a recent paper[[31]](#footnote-31) argues for the exact opposite. Based on strontium and oxygen isotope ratios of human tooth enamel from Tell el-Dab’a skeletons, the women appear to be of non-local Levantine origin (perhaps from Southern Palestine?), who then married into the local ruling Hyksos family.

Dr. Bietak follows the same approach when he endorses Goren’s petrographic arguments that the imported pottery at Tell el-Dab`a came from the northern Levant, because those data and their interpretations fit with his hypothesis that the Hyksos settlers of Tell el-Dab`a originated from there. By the same token and without adducing any cogent scientific explanation, he is willing to discard the NAA data. Now, it seems that he is ready to reject the petrographic data for the Hyksos period in preference to “imports from the southern Levant,” without providing his evidential basis and when only a short time previously he used that data to discredit the NAA data (see footnote 2).

**Case in Point for Rigorous Hypothesis Testing: Radiocarbon Dating**

Another example of Bietak’s seeming disregard for data from the hard sciences is his recent dismissal of 47 short-lived radiocarbon dates from his own site of Tell el-Dab`a—together with many from sites throughout the Levant (Tell el-‘Ajjul in Southern Palestine, Tel Ifshar and Tel Kabri in Israel, Jericho in the West Bank, Tell el-Hayyat in Jordan,and Tell el-Burak in Lebanon—because they were more than 100 years higher than his long-advocated chronology for the Middle Kingdom and Second Intermediate Period.[[32]](#footnote-32) Like he was when the NAA evidence for imported pottery at Dab`a pointed squarely to Southern Palestine as its origin and not Byblos in the northern Levant, Bietak is loath to give up a cherished theory, which he has long held.

As an Egyptologist, Dr. Bietak apparently feels more comfortable with the historical, textual, and archaeological evidence than with that provided by the natural sciences. His discomfort is revealed by his pitting an unsubstantiated idea against the ascertained scientific data. Thus, he variously proposes that the high dates were due to the erosion of older construction material which contained older seeds, wind-borne deposition of the latter from surrounding fields, and/or bioturbation in which seeds from a lower level worked their way up into higher stratigraphic levels of later date. [[33]](#footnote-33) Such explanations essentially ascribe the earlier dates to poor excavation technique at Tell el-Dab`a by not distinguishing stratigraphic features and intermixing earlier material with later.

The evidence for such physical explanations is minimal to non-existent. How could all 47 seeds have moved upwards en masse into later levels?; such an explanation is tantamount to discounting all small radiocarbon samples found in excavations. Where is the evidence for the erosion of older construction material, wind-borne deposition, or bioturbation that affected all the variously situated samples in equal measure? Bietak goes so far as to propose that only the most recent radiocarbon dates should be accepted. The goal should not be to wiggle out of the radiocarbon data, but to take them at face value and try to explain the disparity between those data and your historical reconstruction in a scientific fashion that does justice to both disciplines. It is no wonder that Bietak has once again fallen into an unproductive and sometimes acrimonious debate with the natural scientists, including those in his own academy.[[34]](#footnote-34) We can only hope that he will give more credence to the radiocarbon data in the future, which, if they are to be dismissed, should be done in a scientifically responsible way.

Scientific method starts with a well-ascertained and robust dataset from which “working hypotheses” are induced (for a fuller discussion of what follows, see footnote 4). If you’re on the right track, you should then be able to deduce other consequences, which, if confirmed, strengthen your case. Like a modern forensic science investigation, the smallest, most unintentional piece of evidence—a bit of DNA or a smattering of grape juice on the floor—may be most compelling. One thing you do not do is throw out any meaningful data, like the NAA corpus, or cherry-pick your data such as by citing only flawed and misinterpreted petrographic data, to support your theories.

These strictures are all the more important for an historical science like archaeology, whose interpretations are based on an extremely narrow, fragmented, degraded, and often contaminated body of evidence, truly buried in the past, where replicative experiments, the hallmark of the hard sciences, cannot be carried out; they can only be approximated to by experimental archaeology. The temptation to hold on to a poorly substantiated theory, despite mounting contrary evidence, is greater in archaeology than in the physical sciences. If you make a mathematical mistake or misapply an equation in physics or chemistry, that may lead to disastrous consequences in the real world, such as a bridge collapsing or a rocket blowing up. In archaeology, the only check on speculation is the application of scientific method.

While Dr. Bietak is to be commended on fully documenting the artifacts and stratigraphy at Tell el-Dab`a from an archaeological perspective, he fails in his appropriation of data and interpretations from the natural sciences. Attempting to block the publication of the NAA data and a contrary viewpoint to his own, reported on in this volume, continuing to attack its conclusions in numerous articles, and marshalling others to follow suit are other cases in point. Such dogmatism does not foster the pursuit of the truth in true scientific fashion.

**Final Comments and Looking Back**

This writer did the best he could with the archaeological samples that were passed on to him by Dr. Bietak, who personally chose most of the Tell el-Dab`a samples and who conferred closely on the sampling of MBA pottery from sites throughout Syria-Palestine carried out by Mrs. Joan Huntoon, a member of his staff and from whom I took over the project after her death. The regrettable limitations of the Byblos evidence are specifically pointed out in the book (pp. 9, 29, and 70). I went to considerable lengths to apply powerful statistical tests, however, to the available data, and the small Byblos group was clearly shown to differ from the Southern Palestinian group and be closer to better-defined groups in the northern Levant and Jordan (compare the dendrogram, Fig. 2, and the principal component plot, Fig. 14).

While I remain convinced of the central tenet of the book that Southern Palestine had the closest relationship with Tell el-Dab`a of any region in Syria-Palestine beginning in later MB IIA and continuing throughout the MBA, based on the samples made available to me, I do not rule out the possibility of a more active role by sites in the northern Levant, especially Byblos, with Tell el-Dab`a in late EB IV and early MB IIA (see above). I can even envision a further modification of the two-stage hypothesis in which ships of Canaanite settlers from Byblos, in addition to those from Southern Palestine, made their way by ship to the Nile Delta, much like Phoenicians were said to have done by later classical writers when they fled from Tyre by ship to found the colony of Carthage some 1000 years later.

Sadly, Dr. Stager died in 2017. Our follow-up testing of the 50 pottery sherds from Ashkelon had already been put on a back-burner by the Ashkelon project, as Goren’s petrographic results came to play the principal role in establishing the foreign relations of the site. The recent publication of the Middle Bronze Age volume[[35]](#footnote-35) does not mention the NAA results for either Tell el-Dab`a or Ashkelon or even cite this NAA volume. Yet, throughout this volume—whether one examines the stratigraphy, Levantine pottery types and technology, Egyptian imported pottery and seals, foreign connections, burial customs, etc.—the correspondence between the two sites is remarkably similar. This can hardly be an accident, and is best explained, in my opinion, by the two-stage working hypothesis.

If one went looking for another site in the Levant that explained the Hyksos phenomenon at Tell el-Dab`a, you would be hard pressed to find a better candidate than Ashkelon. The inferences drawn from the NAA analyses, as summarized and discussed in Chapter 6 of this BAR volume, have thus passed another crucial test. As I concluded on the last page of the book:

It is rare that a technical pottery study entails rewriting history, or, at least, archaeological hypotheses. But according to the Neutron Activation Analysis results presented here, the very earliest contacts of the "Hyksos" at Tell el-Dab`a/

Avaris were overwhelmingly with its nearest Levantine "neighbors" in Southern Palestine and not farther north, and this pattern continued throughout the MBA. [N.B.: Although contacts between Southern Palestine and Tell el-Dab`a had already begun in later MB IIA, the “Hyksos” period proper belongs to MB IIB-IIC.]

Some readers may wonder why I have waited so long to present a rebuttal to the highly partisan reviews and comments of Drs. Bietak, Goren, Aston, and others. Primarily, I did not feel competent enough in petrography and geology to present a cogent and fair review, and secondarily I was much involved in organic residue analyses at the time. I was probably too cautious. Archaeologists like Dr. Schaub felt that they were too ill-informed on petrography to protest in print (personal communications by email on June 17, 18, and 20, 2003, and on June 23, 2004), although behind the scenes, they and their petrographers had serious doubts. Lack of competency in chemical analysis, however, had not stopped Goren, Aston, Bietak, and others from assuming that they could critically evaluate the NAA data, its statistical basis, and results.

An email to Dr. Dapha Ben-Tor in July 1999 sums up my feelings at the time:

…I should have been more concerned than I was when Bietak tenaciously held to theories or modified them in an unscientific fashion. His delays in responding to simple requests (e.g., for more precise provenience data) or not doing what he promised (e.g., writing an introduction to the volume) were major frustrations. His submission of poorly edited articles to Penn and BASOR were alarming. In short, without going into all the details, I am now embroiled in one of the most baffling and frustrating academic quagmires of my career. I still don't know what's really happening with my manuscript, and whether it will be published as agreed upon.

So, it looks like I'm the loser in all of this...and it's possible that Yuval Goren had a hand in it, although he says not. One of Bietak's main points when we met in New York last January with Garman Harbottle was that Yuval gave my MS a bad review. And yet, Yuval writes to me that he is not qualified to judge the NAA results. Meantime, he is churning away at producing more petrographic and even chemical results, using inductively coupled plasma spectroscopy, for Bietak, with the eventual plan to publish those results. Clearly, it's in everyone's interest that my book not be published, because it either challenges cherished convictions (Bietak) or makes other researchers' findings superfluous (Goren). Much easier to suppress my data and take credit later.

As an aside, in the course of collaborating with Yuval on other projects, I have come to have serious reservations about his petrographic results. As one example, samples from Scorpion I's Tomb U-j at Abydos (EB IB--ca. 3150 BC) have been analyzed by both of us, and we have come up with irreconcilable conclusions. Yuval and Naomi Porat have argued that the vast majority of the S-P type jars in the tomb were made of local Abydos clay, yet none of our analyses show any matches with any Egyptian alluvial or marl clay. In rechecking the same samples that Yuval said were made of local Abydos clay according to their petrography, the NAA analyses clearly showed these to be from the southern Levant. I have never gotten a satisfactory explanation from Yuval.

Similarly, he argues that some of the Dab`a MB pottery is made of Motza Central Hill Country clay and comes from Jerusalem; this clay is well-represented in our database, and does not statistically match any of the Dab`a pottery. The problem with petrography is that it is much more qualitative and does not determine the provenience of the clay per se. Middle Eastern geology can be much the same, and minerals supposedly determinative of a particular region are widespread. Rather than applying statistical argumentation, Yuval's approach is more that of apprehending the petrographic "Gestalt," which can arguably relate to any number of clay sources. How is his work and approach viewed in Israel?

I took a look at Bietak's web-site for the Chronology Project. Not surprisingly, my research lab in MASCA is still listed as being a participant, although I never consented to working any further with Bietak after I turned in my MS 2 years ago. Peter Kuniholm was also written into the project without his concurrence. Like you, I was aghast at whom he listed as being responsible for Israel/Palestine. He evidently feels most comfortable in "controlling" young scholars who have few other options. He lists himself and Irmgard Hein as in charge, but neither are archaeologists of the region. One of the things that told me that his chronology project was ill-conceived is that he adopted David Aston's view that Beth Shan Levels IX-VII should be dated on the basis of the scarab evidence (see Aston's review of F. James' and my book in JEA). Bietak presented this throw-back to the days of Rowe in a detailed discussion of Israel/Palestine included in his Chronology proposal. I won't go into his unscientific approach to radiocarbon dating...

I've gone on at some length, since Bietak will try to paint a different picture. But, the facts are that he (and the Austrian Academy) are essentially stifling sound scholarship and wasting a considerable amount of time and money.

It would appear in the long run that the basic thesis of my NAA study will hold up. When I first argued (some 10 years ago) that most of the very MB IIA pottery at Dab`a was coming from the "Gaza region," I was told by eminent authorities that this was impossible--there were few if any sites of this period in the region. I said that it was possible that a major site lay hidden and would be eventually discovered--in particular, I suggested Ashkelon. When Larry Stager began uncovering a huge MB IIA site here, we immediately began analyzing amphoras and other S-P types, and they were so close chemically to the large Dab`a group that had already been analyzed and assigned to this region that it was astounding. Moreover, I actually ran a large group of "Syrian" types (as defined by Bietak) and 90% belonged to the Gaza region (the other 10% were of uncertain provenience). These two follow-up studies confirmed the initial conclusions that I had arrived at; one can hardly imagine better examples of deductive testing which bear out a hypothesis.

It is indeed encouraging to hear that your scarab studies arrive at similar conclusions, and provide further confirmatory evidence. ..

I hoped over time that the obvious disparities between the NAA and petrographic results would be resolved, and that it would be concluded that the NAA results for a Southern Palestine provenance for most of the imported pottery at Tell el-Dab`a was correct (omitting the earliest phase(s) of the MB IIA which was admittedly not well represented in the NAA database, but showed some evidence for northern Levantine connections to Tell el-Dab`a). I also hoped that other petrographers would step forward and point out the inadequacies of Goren’s petrographic methodology, or that some archaeologists or textual scholars might take issue with the petrographic results.

I was largely wrong. Opinion strongly swung to Goren’s side, and still remains there, principally in archaeological circles. A discussion by Drs. Daphna Ben Tor and Lanny Bell of the clay sealings, which were impressed with Egyptian scarabs, from the Moat Deposit at Ashkelon[[36]](#footnote-36) illustrates the problem. Unpublished petrographic results by Goren are cited as being authoritative, whereas the unpublished NAA results go unmentioned. In advance of Goren’s petrographic analysis of the sealings, I had emailed Dr. Ben-Tor in July 2000 the NAA results for five sealings, three of which demonstrated their having been made in Southern Palestine (one most likely at Ashkelon itself). Two of sealings were of uncertain origin. There were no Egyptian "matches" for any of the samples.

The Ashkelon sealings are unique to Southern Palestine, apart from a single sealing stamped with an Egyptian private-name scarab from Tell el-`Ajjul, often argued to be Sharuhen (although other locations have been suggested including Gaza itself, now covered by a modern city). The latter, which was captured by Ahmose I after a three-year siege at the beginning of the LBA, was a major stronghold in Southern Palestine to which the Hyksos retreated. This amazing group of sealings bears out the hypothesis that the earliest contacts between Southern Palestine and the eastern Nile Delta were in later MB IIA, which accords with the NAA results and associated evidence, especially scarabs.  Coming from the fill between two MB IIA massive defensive walls at Ashkelon, the finding fits with the two-stage hypothesis of, first, the building of city-states in Southern Palestine, earlier in MB IIA, and then the movement of people, given an ever-expanding population, from Southern Palestine to the eastern Nile Delta. Once a large Southern Palestinian population had settled at Tell el-Dab`a, the administrative practice of sealing boxes, special stone and pottery vessels, doors and silos, etc. with sealings, marked by official Egyptian scarabs, might well have been transferred back to Southern Palestine and used there.

While Goren’s petrographic results are generally in accord with the NAA results in this instance, one needs to see his evidence before accepting his results of “local” production for the Ashkelon corpus of 41 sealings. For example, it has not been possible to interrelate the two sealings of “uncertain origin,” according to NAA, to Goren’s results. The fact that these two sealings had unusually high amounts of calcium (upwards of 40%) could indicate that large amounts of calcite or another calcium-containing mineral was mixed with the clay as temper, and that once a correction is made for this, the NAA data will fall in line with the those of the other three “local” sealings. Another issue is just how extensive “local” is: does it refer only to Ashkelon or a larger area within the Gaza group of MBA sites? If the latter, it is possible that some of the containers come from elsewhere within the region, such as Sharuhen.

One of the sealings was impressed on the stopper of a juglet, a practice unrecorded in Egypt and that is unique to Southern Palestine. Intriguingly, the similarly unique Palestinian practice of stamping a handle with a scarab is attested on a Second Intermediate Period (MB IIB-C) Canaanite Jar: JH091 (Fig. 61), excavated at Tell el-Dab`a. The NAA results confirm that the jar is an import from Southern Palestine. Goren’s petrographic result was that the vessel came “Central and Southern Hill Country” in 1997, which was subsequently changed to “Lebanon east of the coast line Beirut-Byblos” in 2004. Cohen-Weinberger and Goren (“Daba Petrography,” p. 84) “cautiously suggest” that the scarab and jar is that of a Byblian prince, which fits well with Bietak’s theory.

The scarab’s hieroglyphic inscription reads *h3ty- ` shimw*, "ruler [of] shimu," and is variously interpreted as referring to a city-state or individual in the northern Levant (perhaps Damascus or the latter’s prince[[37]](#footnote-37)) or Southern Palestine (e.g., Sharuhen--this monograph, p. 33). Dr. Ben-Tor makes a strong case that the stamped jar must originate from Palestine, because stamped handles only occur there (apart from imported jars at Tell el-Dab`a and Lisht) and because there is not a single private-name scarab of a Byblite or other northern Levantine prince after the late Middle Kingdom (personal communication, Feb. 21, 2001). She also informed me in the same email that *h3ty- `* on Byblite scarabs is always accompanied by the name of Byblos and that royal-name scarabs are as yet unattested in the northern Levant following the Middle Kingdom.[[38]](#footnote-38) She concluded by stating: “I strongly believe the jar comes from Palestine.” That inference can now be narrowed down to Southern Palestine to a very high level of probability according to the NAA results.

As Drs. Ben-Tor and Bell cogently argue,[[39]](#footnote-39) the best explanation for the Ashkelon sealings and other scarab evidence—as well as the NAA findings—is that Egyptian administrative practice was already in place at Tell el-Dab`a in the late Middle Kingdom. Some elements of this practice were subsequently adopted by rulers of already well-established city-states in Southern Palestine. This development is comparable to what occurred at Byblos when rulers there began enclosing their hieroglyphic names in cartouches and using Egyptian royal epithets about the same time.[[40]](#footnote-40) A concomitant amuletic function need not be ruled out.

Follow-up NAA tests, especially of pottery vessels on which the sealings were used (e.g., the stoppered juglet), are needed to further clarify the purposes, origins, and the extent of this administrative system in Southern Palestine. Organic residue analysis should be carried out to determine what the vessels contained. The question also remains whether such hypothesized administrative practices continued into Hyksos times in Southern Palestine.

As an aside, the “discoverers” of major Egyptian-Southern Palestinian contacts in later MB IIA deserve to be mentioned here. The hypothesis was initially proposed by Dr. James Weinstein in his 1981 article,[[41]](#footnote-41) based on royal-name Hyksos scarabs that were concentrated in the southern Levant during MB IIB-IIC, but non-existent in the northern Levant. Dr. Garman Harbottle and I came to the same conclusion in October 1988, based on the NAA data. It should be noted that Dr. Weinstein argued for a “Hyksos” area south of the Jezreel Valley, while the NAA results restricted it to the Gaza group of MBA sites (“Southern Palestine”).

More time is clearly needed for a final resolution of the NAA-petrography debate. New findings have begun to go counter to Goren’s petrographic results, specifically Goren’s provenancing of the Scorpion I wine jars to a local Egyptian clay source.25 The Addendum to this Appendix catalogues numerous problems with his and his followers’ methodological approach to petrography, geochemistry, geology, and statistics. Goren’s and Bietak’s hypothesis of the importation of Middle Bronze IIB-IIC vessels from northern Levant to Dab`a should eventually collapse. An independent, objective observer should now be able to come to the conclusion that virtually all the available evidence—whether scarabs, burial customs and artifacts, etc., but particularly the NAA evidence— points to very close connections between Tell el-Dab`a and **only** Southern Palestine during the Hyksos period.

Now, having seemingly reached the twilight of my career, a fortuitous series of events led to my delving once again into the quagmire of archaeological debate to produce a final “swan song” on the Hyksos. The initial impetus came from lectures I gave for Harvard College and its Fogg Museum in October 2018 that provided me the opportunity to examine archaeological artifacts that I had previously analyzed in the Semitic Museum storerooms. In digging back through my correspondence with Dr. Stager, I also came across some previously unpublished NAA results for Ashkelon, which included the moat sealings and the testing of the Southern Palestine working hypothesis, discussed above.

A second impetus that drew me back to the Hyksos question was when I found an unpublished manuscript by my doctoral dissertation supervisor, Dr. Frances W. James, in the Penn Museum archives. It included a discussion of the stratigraphic situation and finds of a large MBA graveyard which had been excavated by Gerald M. Fitzgerald on the tell. Even the renewed excavations at the site by Dr. Amihai Mazar, which had reached the same levels (especially XI) as the “new” graveyard, did not know of its existence.[[42]](#footnote-42)

In reviewing Dr. James’ manuscript, one tomb (1803) of probable later MB IIA date stood out, because it was a well-defined, intact burial of a single individual, interred with his/her burial goods. Surpisingly, the tomb group included an imported Egyptian scarab of the Sobekhotep-group, one of only a handful of this type found in the southern Levant. Such scarabs mark the end of Middle Kingdom influence in the region in accord with the two-phase hypothesis based on the scarab evidence. Nine whole vessels (2 bowls, 5 juglets, a red-painted jug, and an amphora jug), together with a copper-based toggle-pin, rounded out the corpus. Another large jar with a painted decoration similar to the Montet Jar from Byblos was found nearby in Locus 1803; it may or may not be related to the tomb per se. The pottery can be broadly characterized as of probable northern Palestinian and possibly local manufacture. It should be stressed, however, that pottery typology is far from being an exact science, no radiocarbon determinations were made or indeed are possible, and that NAA analyses are needed to establish which vessels might be of local manufacture or were imported.

These Beth Shan findings naturally led me to undertake a fresh appraisal of this monograph, since I had cited parallels to some of the same pottery types as those from grave 1803 vessels and needed to recheck the latter in addition to collecting *comparanda* from the many Levantine sites and tombs which have since been published. It took me some time to get back up-to-speed on the latest publications, radiocarbon dating, and other matters related to this important period.

As I delved back into the NAA-petrographic controversy with Goren, I realized that I needed a professional geologist to assess that data, and by another stroke of serendipity, I tracked down Dr. Wnuk, my petrographer from nearly 30 years ago, sent him an email, and within days we were back in touch. In the intervening years, he had had a fascinating career discovering coal, gold, and other mineral deposits throught the Middle East. He was in between projects and offered to fully review Goren’s methodology for this appendix. I now had the technical backup that I needed to move forward not just on publishing the Tell el-Dab`a pottery drawings at long last, but in moving forward on a full-fledged critique of an approach and results that I have seriously questioned for many years.

Since obtaining the best data you can, developing working hypotheses to be tested, and making it available to the larger academic world for further investigation are the prerequisites of any science, properly so-called, I also reinitiated contact with Dr. Glascock of the Missouri lab, to have as much of the Brookhaven and Missouri data for this study be made available on-line on the Missouri homepage. He gladly agreed to this, and the data is now available at [www.archaeometry.missouri.edu](http://www.archaeometry.missouri.edu). Archaeological petrographers are advised to do the same.

We can only hope that this critical appraisal of Goren’s and his followers’ methodology and conclusions here will help correct the record and prevent any errors being perpetuated in the literature. I also remain hopeful that the unpublished NAA data from Ashkelon will eventually be published, since it bears importantly on a period of exceptional cultural interaction, trade, the transfer of technologies, and ethnic origins in the ancient Near East, Egypt, and the Mediterranean world.

**Table 47: NAA Results for Follow-Up Test and Moat Sealings[[43]](#footnote-43)**

PMG518

Jug rim; MB II

2.67.L17.B7.+.(1)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: ?

PMG519

Jug body sherd; MB II

2.67.L18.F18.B19+.(6)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG520

Red-slipped jug rim; MB II

2.67.L19.B21+.(1)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: ?

PMG521

Amphora rim; MB II

2.67.L19.B21+.(4)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG522

White-slipped and painted sherd (possibly Chocolate-on-White Ware); MB II

2.67.L19.B21+.(21)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG523

Cooking pot rim; MB II

2.67.L19.B21+.(25)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: ?

PMG524

Painted jug rim; MB II

2.76.L163.F163.B11+.(17)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG525

Red-slipped body sherd; MB II

2.76.L163.F163.B11+.(27)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

PMG526

Painted body sherd; MB II

2.76.L163.F163.B11+.(29

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Probably Southern Palestine

PMG527

Red-slipped and burnished bowl; MB II

2.76.L163.F163.B11+.(33)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG528

Red-slipped and burnished bowl; MB II

2.76.L163.F163.B38+.(8)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG529

White-slipped jug rim and neck (possibly Chocolate-on-White Ware); MB II

2.76.L166.B21+.(2)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG530

Jug rim and neck; MB II

2.76.L166.B21+.(4)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG531

Bichrome Painted sherd; MB II

2.76.L166.B21+.(5); MB II

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG532

Cooking pot rim; MB II

2.76.L166.B21+.(6)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG533

Cooking pot rim; MB II

2.76.L166.B21+.(7)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG534

Cooking pot rim; MB II

2.76.L166.B21+.(8)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG535

White-slipped and painted sherd (possibly Chocolate-on-White Ware); MB II

2.76.L166.B21+.(11)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG536

Amphora rim; MB II

2.76.L166.B21.(12)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG537

Amphora/pithos upper body; MB II

2.76.L166.B25+.(32)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Probably Southern Palestine

PMG538

Red-slipped and painted sherd; MB II

2.76.L166.B25+.(37)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG539

Rim; MB II

2.76.L166.B25+.(46)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: ?

PMG540

Amphora rim with shoulder collar; MB II

2.76.L166.B25+.(48)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Probably Southern Palestine

PMG541

Cooking pot rim and sidewall; MB II

2.76.L166.B26+.(28)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG542

Amphora rim and neck; MB II

2.67.L17.B7+.(1)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG543

Jug or cooking pot (?); MB II

2.67.L17.F18 B19+.(6)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: ?

PMG544

Amphora rim and neck; MB II

2.67.L19.B21+.(1)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG545

Amphora rim and neck; MB II

2.76.L166.B21+.(4)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Probably Southern Palestine

PMG546

Painted amphora (?) body sherd; MB II

2.76.L166.B21+.(21)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Probably Southern Palestine

PMG547

Cooking pot; MB II

2.76.L166 B21+.(25)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG548

2.76.L163.F163.B11; MB II

Cypriot White Slip Ware bowl, painted

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: ?

PMG549

Jar; MB II

2.76.L163.F163.B11

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Probably Southern Palestine

PMG550

Jar; MB II

2.76.L163.F163.B11

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Probably Southern Palestine

PMG551

Jar; MB II

2.76.L163.F163 B11

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG552

Bichrome Painted and burnished jar body sherd; MB II

2.76.L163.F163.B38

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG553

Jar rim; MB II

2.76.L166.B21+.(2)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Probably Southern Palestine

PMG554

White-slipped jar rim and neck (possibly Chocolate-on-White Ware); MB II

2.76.L166.B21+.(4)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG555

Bichrome Painted jar body sherd; MB II

2.76.L166.B.21+.(5); MB II

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG556

Jar rim; MB II

2.76.L166.B21+.(6)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Probably Southern Palestine

PMG557

Amphora rim and neck; MB II

2.76.L166.B.21+.(7)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG558

Jar rim; MB II

2.76.L166.B.21+.(8)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG559

White-slipped and painted jar body sherd (possibly Chocolate-on-White Ware); MB II

2.76.L166.B.21+.(11)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG560

Bichrome-painted bowl

2.76.L166. B21+.(12) ; MB II

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG561

Amphora rim and neck

2.76.L166.B21+.(37) ; MB II

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG562

Amphora rim and neck; MB II

2.76.L166.B21+.(32)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Probably Southern Palestine

PMG563

White-slipped and painted juglet sherd (possibly Chocolate-on-White Ware); MB II

2.76 L.166 B.25+.(46)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG564

Lisht Ware juglet (?); MB II

2.76.L166.B25+.(48)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Southern Palestine

PMG565

Tell el-Yahudiyeh Ovoid type base; MB II

2.76.L166.B26+.(28)

Harvard Semitic Museum; Israel Antiquities Authority license no. A41/89

NAA Provenience: Egyptian Nile alluvium

PMG566

Tell el-Yahudiyeh Piriform 4 type base; MB II

2.85 L.99 B.22

Harvard Semitic Museum; Israel Antiquities Authority license no. A72/92

NAA Provenience: Egyptian Nile alluvium

PMG567

Tell el-Yahudiyeh Piriform 2 base; MB II

2.85 L.99 B.22+.(nn[[44]](#footnote-44))

Harvard Semitic Museum; Israel Antiquities Authority license no. A72/92

NAA Provenience: Egyptian Nile alluvium

PMG576

Unstamped clay bulla; MB IIA

2.56.L17.B303.MC[[45]](#footnote-45)#51658

*Ashkelon 6[[46]](#footnote-46)*: fig. 13.28 or 13.32-33

Harvard Semitic Museum; Israel Antiquities Authority license no. A73/98

NAA Provenience: Southern Palestine

PMG577

Stamped clay bulla; MB IIA

2.56.L17.B436.MC#51647

*Ashkelon 6*: catalogue no. 25 and fig. 13:43

Harvard Semitic Museum; Israel Antiquities Authority license no. A73/98

NAA Provenience: ?

PMG578

Stamped clay bulla; MB IIA

2.56.L17.B316.MC#51627

*Ashkelon 6*: catalogue no. 7 and fig.13:42

Harvard Semitic Museum; Israel Antiquities Authority license no. A73/98

NAA Provenience: Southern Palestine (probably local Ashkelon)

PMG579

Unstamped clay bulla; MB IIA

2.56.L17.B316.probably MC#51660

*Ashkelon 6*: probably fig. 13:44

Harvard Semitic Museum; Israel Antiquities Authority license no. A73/98

NAA Provenience: Southern Palestine

PMG580

Unstamped clay bulla; MB IIA

2.56.L17.B343.MC#51667

*Ashkelon 6*: cf. catalogue no. 21 and fig. 13.42

Harvard Semitic Museum; Israel Antiquities Authority license no. A73/98

NAA Provenience: ?

**Table 48: Comparison of 1997 and 2004 Petrographic Assignments Versus 2000 NAA Results**[[47]](#footnote-47)

| **Registration no., NAA no.** | **2000 NAA Assignment** | **1997 Petrographic Assignment** | **Comparison of**  **1997 and NAA Results** | **2004 Petrographic Assignment** | **Comparison of**  **2004 and NAA Results** | **Comparison of**  **2004 and 1997 Petrographic Results** |
| --- | --- | --- | --- | --- | --- | --- |
| 2497E, JH073 | Southern Palestine | Shephelah (Southern Palestine) | + | Northernmost Israeli coast or the Lebanese coast | - | **-** |
| 2497G, JH072 | ? (possibly Southern Palestine) | Not recorded | No information | Akkar plain/Jezreel Valley | - | **No information** |
| 2532C, JH055 | ? | Egypt | ? | Lebanon east of the coast line Beirut-Byblos | N.D. (should be -) | **-** |
| 2660B, JH136 | Southern Palestine | Not recorded | ? | Northernmost Israeli coast or the Lebanese coast | - | **No information** |
| 2879G, JH915 | Most similar to Tell el Dab'a JH130, Southern Palestine | Shephelah (Southern Palestine) | + | Northernmost Israeli coast or the Lebanese coast | - | **-** |
| 3336A, MB025 | Southern Palestine | Not recorded | ? | Northernmost Israeli coast or the Lebanese coast | - | **No information** |
| 3423C, JH916 | ? | Cyprus | - | Northwestern Syria: Ugarit, Amuq area or the Cilician coast or Cyprus | N.D. (should be -) | **Possibly +** |
| 3955A, JH075 | ? (possibly Southern Palestine) | Shephelah (Southern Palestine) | + | Lebanon east of the coast line Beirut-Byblos | - | **-** |
| 3959B, JH077 | ? (possibly Southern Palestine) | Central and Southern Hill Country | - | Northernmost Israeli coast or the Lebanese coast | - | **-** |
| 4030B, JH066 | Southern Palestine | Not recorded | No information | Northwestern Negev | + | **No information** |
| 4060D, JH010 | Southern Palestine | Shephelah (Southern Palestine) | + | Mt. Carmel region | - | **-** |
| 4099C, JH084 | ? (possibly Southern Palestine) | Central and Southern Hill Country | - | Judea or Samaria in Israel | ? (should be -) | **+** |
| 4107A, JH903 | ? (possibly Southern Palestine) | Southern Coastal Plain (Southern Palestine) | + | Shephelah/Wadi I`ron | ? (should be +) | **+** |
| 4223B, MB016 | ? (possibly Southern Palestine) | Not recorded | No information | Unknown | N.D. | **No information** |
| 4226, MB027 | Most similar to Tell el-Dab'a JH083, Southern Palestine | Not recorded | No information | Undetermined northern Levant | - | **No information** |
| 4426C, JH064 | Southern Palestine | Not recorded | No information | Judea/Samaria/Galilee mountains | ? (should be -) | **No information** |
| 4503, JH115 | Southern Palestine | Southern Coastal Plain (Southern Palestine) | + | Negev coastal plain in Israel | + | **+** |
| 4505A, JH061 | ? (possibly Southern Palestine) | Southern Coastal Plain (Southern Palestine) | + | Central coast of Israel | ? (should be -) | **-** |
| 4536, JH089 | ? (possibly Southern Palestine) | Shephelah (Southern Palestine) | + | Northwestern Negev/Southern Shephelah | + | **+** |
| 4537, JH091 | Southern Palestine | Central and Southern Hill Country | - | Lebanon east of the coast line Beirut-Byblos | - | **-** |
| 4537A, JH013 | ? (possibly Southern Palestine) | Central and Southern Hill Country | - | Undetermined northern Levant | - | **-** |
| 4539, JH152 | Nile alluvium | Egypt | + | Northwestern Negev | - | **-** |
| 4548C, JH040  4548C, JH040 | Nile alluvium | Egypt | + | Nile Valley | + | **+** |
| 4549C, JH029 | Nile alluvium | Egypt | + | Northernmost Israeli coast or the Lebanese coast | - | **-** |
| 4550B, JH033-036 | Southern Palestine | Central Coastal Plain | - | Nile Valley | - | **-** |
| 4551B, JH045 | Southern Palestine | Not recorded | No information | Northernmost Israeli coast or the Lebanese coast | - | **No information** |
| 4551C, JH046 | ? (possibly Southern Palestine) | Central and Southern Hill Country | - | Northernmost Israeli coast or the Lebanese coast | N.D. ? (should be -) | **-** |
| 4551E, JH048 | Southern Palestine | Not recorded | No information | Northwestern Negev | + | **No information** |
| 4551F, JH049 | ? (possibly Southern Palestine) | Shephelah (Southern Palestine) | + | Central coast of Israel | ? (should be -) | **-** |
| 4551L, JH043 | Southern Palestine | Shephelah (Southern Palestine) | + | Northernmost Israel or the Lebanese coast | - | **-** |
| 4552E, JH079 | ? (possibly Southern Palestine) | Shephelah (Southern Palestine) | + | Northernmost Israel coast or the Lebanese coast | - | **-** |
| 4553A, JH095 | ? (possibly Southern Palestine) | Shephelah (Southern Palestine) | + | Northernmost Israel coast or the Lebanese coast | - | **-** |
| 4630A, JH109 | Most similar to Southern Palestine | Shephelah (Southern Palestine) | + | Undetermined northern Levant | - | **-** |
| 4630D, JH113 | Most similar to Ebla JHEB03, of local origin | Central and Southern Hill Country | - | Eastern Galilee or the Yarmuq area, or the Akkar or Middle Orontes north of Qedesh | + | **-** |
| 4636A JH132 | Most similar to Qatna JH482, import from Southern Palestine | Not recorded | No information | Mount Carmel area in Israel | - | **No information** |
| 4777, JH657 | Southern Palestine | Southern Coastal Plain (Southern Palestine) | + | Nile Valley | - | **-** |
| 4779, JH611 | Southern Palestine | Shephelah (Southern Palestine) | + | Undetermined northern Levant | - | **-** |
| 4951A, JH726 | ? | Not recorded | No information | Mount Carmel region | N.D. (should be -) | **No information** |
| 4958, JH858 | Southern Palestine | Not recorded | No information | Lebanon east of the coast line Beirut-Byblos | - | **No information** |
| 4972, JH717 | ? (possibly Southern Palestine) | Palestine? | ? | Lebanon east of the coastline Beirut-Byblos | - | **-** |
| 4978, JH688 | ? (possibly Southern Palestine) | Shephelah (Southern Palestine) | + | Lebanon east of the coast line Beirut-Byblos | N.D. (should be -) | **-** |
| 4980, JH856 | Nile alluvium | Not recorded | No information | Nile Valley | + | **No information** |
| 5203, JH703 | Southern Palestine | Shephelah (Southern Palestine) | + | Northernmost Israeli coast or the Lebanese coast | - | **-** |
| 5226A, JH837 | Southern Palestine | Not recorded | No information | Undetermined northern Levant | - | **No information** |
| 5226B, JH838 | ? (possibly Southern Palestine) | Not recorded | No information | Undetermined northern Levant | N.D. (should be -) | **No information** |
| 5709, PMG124 | Southern Palestine | Not recorded | No information | Undetermined northern Levant | - | **No information** |
| 5816, PMG131 | ? (possibly Southern Palestine) | Shephelah (Southern Palestine) | + | Mount Carmel region | - | **-** |
| 5822Q, JH696 | ? | Shephelah (Southern Palestine) | ? | Northern Lebanese coast (north of Tripoli) | N.D. (should be -) | **-** |
| 5824, PMG123 | Southern Palestine | Not recorded | No information | Northern Israel or the Lebanese coast | - | **No information** |
| 5826, PMG117 | Southern Palestine | Central and Southern Hill Country | - | Lebanon east of the coast line Beirut-Byblos | - | **-** |
| 5827, PMG118 | Southern Palestine | Not recorded | No information | Nile Valley | - | **No information** |
| 5828, PMGI19 | Southern Palestine | Not recorded | No information | Northernmost Israeli coast or the Lebanese coast | - | **No information** |
| 5894, PMG120 | ? | Central and Southern Hill Country | ? | Northwestern Syria: Ugarit, 'Amuq area or the Cilician coast | N.D. (should be -) | **-** |
| 5894C, PMG121 | ? (possibly Southern Palestine) | Central and Southern Hill Country | - | Northern Lebanese coast (north of Tripoli) | N.D. (should be -) | **-** |
| 6114E, MB029 | Southern Palestine | Not recorded | No information | Undetermined northern Levant | - | **No information** |
| 6114L, MB032 | Nile alluvium | Not recorded | No information | Nile Valley | + | **No information** |
| 6115G, MB015 | ? (possibly Southern Palestine) | Not recorded | No information | Eastern Galilee or the Yarmuq area, or the Akkar or Middle Orontes north of Qedesh | N.D. (should be -) | **No information** |
| 6115Y, MB018 | Southern Palestine | Not recorded | No information | unknown | N.D. (should be -) | **No information** |
| 7029A, PMG105 | ? | Southern Lebanon or Northern Israel | - | Lebanon east of the coast line Beirut-Byblos | N.D. (should be -) | **Probably +** |
| K411, JH002 | ? (possibly Southern Palestine) | Northern Syria, Cyprus, Cilician Plain, or Southern Anatolia | - | Northwest Syria or Cilicia | + | **Probably +** |
| K0478, PMG107 | Southern Palestine | Shephelah (Southern Palestine) | + | Undetermined northern Levant | - | **-** |
| K2567, PMG111 | Southern Palestine | Shephelah (Southern Palestine) | + | Central coast of Israel | ? (should be -) | **-** |
| K2574, PMG114 | Most similar to Tell el Dab'a JH859, which is most similar to Tell el- Hesi DBP A44, of local origin  1 | Shephelah (Southern Palestine) | + | Northernmost Israeli coast or the Lebanese coast | N.D. (should be -) | **-** |
| K2574, PMG115 | ? | Shephelah (Southern Palestine) | ? | Northernmost Israeli coast or the Lebanese coast | N.D. (should be -) | **-** |
| K2771, PMG106 | ? (possibly Southern Palestine) | Shephelah (Southern Palestine) | + | Lebanon east of the coast line Beirut-Byblos | - | **-** |
| K2810, JH108 | Southern Palestine | Shephelah (Southern Palestine) | + | Northern Lebanese coast (north of Tripoli) | - | **-** |
| K2810A, JH111 | Most similar to Nile alluvium | Egypt | + | Nile Valley | + | **+** |
| K3456, PMG125 | ? (possibly Southern Palestine) | Southern Coastal Plain (Southern Palestine) | + | Northwestern Negev | + | **+** |
| K3656, PMG104 | Southern Palestine" | Shephelah (Southern Palestine) | + | Central coast of Israel | ? | **-** |
| ?, JH672 | Fayyum-Maadi marl clay | Egypt | + | Lebanon east of the coast line Beirut-Byblos | - | **-** |

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Center for the Analysis of Archaeological Materials (CAAM), University of Pennsylvania Museum

Dr. William Gilstrap

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Research Reactor Center, Archaeometry Laboratory, University of Missouri—Columbia

Dr. Gretchen R. Hall

Biomolecular Archaeology Project, University of Pennsylvania Museum

Include Daphna?

Dr. Daniel Master

Check with Chris

Others to be added:

**Addendum**

**Assessment of the Petrographic, Geochemical, Geological, and Statistical Methodologies Used by Yuval Goren and His Colleagues**

**Dr. Christopher Wnuk**

Chief Geologist, Transformation Advisors Group

The following critique of the geologic, petrographic and geochemical methodology of Yuval Goren and his colleagues is based on the following articles:

1. Cohen-Weinberger, A., and Goren, Y., 2004, Levantine-Egyptian interactions during the 12th to the 15th dynasties based on the petrography of the Canaanite pottery from Tell el-Dabʿa: Austrian Academy of Sciences Press, *Ägypten und Levante* (*Egypt and the Levant*), v. 14, p. 69-100.
2. Goren, Y., 1996, The southern Levant in the Early Bronze Age IV: The petrographic perspective: *Bulletin of the American Schools of Oriental Research*, v. 303, p. 33-72. <https://doi.org/10.2307/1357469>.
3. Goren, Y., Bunimovitz, S., Finkelstein, I., and Na'aman, N., 2003, The Location of Alashiya: New Evidence from petrographic investigation of Alashiyan tablets from el-Amarna and Ugarit: *American Journal of Archaeology*, v. 107, p. 233-255.
4. Goren, Y., Finkelstein, I., and Na’aman, N., 2004, *Inscribed in clay: provenance study of the Amarna tablets and other ancient Near Eastern texts*: Tel Aviv University, Sonia and Marco Nadler Institute of Archaeology, Monograph Series Number 23, 407 p.
5. N. Porat and Y. Goren, Petrography of the Naqada IIIa Canaanite pottery from Tomb U-j in Abydos, pp. 466-481 in *Umm el-Qaab II: Importkeramik aus dem Friedhof U in Abydos (Umm el-Qaab) und die Beziehungen ägyptens zu Vorderasien im 4. Jahrtausend v. Chr.* by U. Hartung, German Institute of Archaeology at Cairo Archaeological Publications 93, Mainz: P. von Zabern, 2001; *idem*, Petrography of the Naqada IIIa Canaanite Pottery from Tomb U-j in Abydos, pp. 252-270 in *Egypt and the Levant: Interrelations from the 4th through the Early 3rd Millennium B.C.E.*, eds. E. C. M. van den Brink and T. E. Levy, London and New York: Bloomsbury T&T Clark, 2001.
6. Goren, Y., Mommsen, H., and Klinger, J., 2011, Non-destructive provenance study of cuneiform tablets using portable X-ray fluorescence (pXRF): *Journal of Archaeological Science*, v. 38, p. 684-696.

The geological, petrological, and geochemical tools that are used by Goren and his coworkers have been used by geologists for more than 200 years.[[48]](#footnote-48) Geologists started using these tools to study rock provenance as soon as or shortly after they became available. As a result, the capabilities and limitations of these instruments and procedures are well understood, and instances of their misapplication to problem analysis well documented. There now exists an enormous body of literature that defines standard procedures for using these tools to collect meaningful data. These procedures are based on testing, retesting, and independently cross-comparing analytical outcomes.

Based on a review of their analytical methodologies as published in their reports and book, Goren and his coworkers would appear to be unaware of these standard procedures. Consequently, many of the analytical results upon which they make their archaeological interpretations are highly suspect. This circumstance, in turn, impacts the validity of the conclusions drawn from the analytical results for collecting and analyzing petrographic and chemical data for assessing the provenances of pottery samples from Middle Bronze (MB) Age Tell el-Dab`a, Early Bronze (EB) Age IV in the southern Levant, and the EB IB tomb U-j (of Scorpion I) at Abydos in Egypt, as well as the LBA Amarna tablets between rulers of Egypt, the Near East, Anatolia, and Cyprus.

The one over-riding problem with all these analyses is that the authors clearly do not understand how critical proper sample size is to the validity of an analysis. They concede that their samples are too small, but then go on to ignore this issue as if it has no real consequences. They seem quite comfortable in ignoring the extraordinary bias they have introduced into their entire analysis and ignore the possibility that many of their conclusions are unsupportable because they are studying unrepresentative samples.

In general, the methodology of the analyses is disorganized, so much so that this reader had difficulty in determining which tests and analyses the authors were using to collect and assess the significance of their data, or to determine which assumptions Goren and his colleagues were making during their analyses. Further complicating any assessment was the fact that these authors consistently and incorrectly use technical terms that strongly create the impression that they don’t understand the science behind their technical analyses and therefore, that they are misinterpreting their results. Their constant misuse of technical terminology leaves the reader confused and spending additional time trying to determine if the authors are simply careless using words or if they truly do not understand the concepts behind the words. Misused terminology is so commonplace, the latter interpretation seems to be the more likely.

In the sections which follow, the problems with the methodologies as used in the above cited studies will be reviewed. Procedural inadequacies will be discussed, and explanations will be provided showing why Goren and his coworkers are using flawed approaches for their provenance studies. The assessment is largely based on the interrelated studies that were published in 2004, viz., the Cohen-Weinberger and Goren paper on the petrography of pottery sherds from Tell el-Daba, and the book on the provenance of the Amarna tablets. Related and highly relevant comments, bearing on the conclusions of the 2004 studies, are also made to earlier and later studies.

**Sampling for Petrographic Analysis**

Decades of research resulting in thousands of methodological studies have been devoted to defining effective petrographic analytical methodologies. Goren and his colleagues have ignored this established body of work and have introduced untested petrographic methodologies that are demonstratively flawed.

Goren et al. (2004) state that the minimum size for a thin section sample is 10 mm x 5 mm[[49]](#footnote-49), i.e. 50 mm2. Even so, the authors concede that most of their “peel” samples are smaller than this minimum. No reference is provided for the origin of this minimum. The Office of the Wyoming State Archaeologist recommends that pottery samples be 22 mm x 42 mm (i.e. 924 mm2) to ensure the best possible sample for petrographic analysis of pottery material (OWSA 2019). In the geologic petrographic literature, it is generally accepted that the sample must be large enough to allow between 300 and 500 unique point counts (Hutchison 1974; Howard 1993; Poole and Sims 2016; among many others). For certain applications, several thousand points per slide are required (Los Alamos National Laboratory 1990). Active research continues regarding how best to minimize counting and sampling error when making point counts (Demirmen 1971; Neilson and Brockman 1977; Bustin 1991; Vermeesch 2018; etc.). Ultimately, the optimal size of the thin section will be controlled by the grain size of the material being studied. The coarser the grain size, the larger the thin section required to allow the 300-500 unique grain counts.

Goren et al. (2003) and Goren et al. (2004) apparently introduce a new petrographic sample collection strategy, Scattered Petrographic Analysis (SPA), but they provide no justification to support their contention that the method provides representative samples of the material to be analysed. The studies referenced in Flanagan (1986) and USDOE (2019) among many others demonstrate the intensity of testing and assessment that any analytical procedure must experience before being deemed sufficiently robust to be accepted as a standard methodology. As far as can be determined from the Goren et al. (2004) methodology section, the only testing of SPA occurred “…in the pilot phase of the study [when] a new sampling and examination method was developed by Goren and named ‘Scattered Petrographic Analysis’ (henceforth SPA)[[50]](#footnote-50).” The very description of the SPA procedure raises alarms. Goren et al. (2004) state “A tiny flake of the clay matrix (ca. 1x1 mm) is chipped from a previously fractured surface using a scalpel. The inclusions exposed on the surface of the object are identified under the stereomicroscope and a representative sample dragged as single grains (usually from the edges of the tablet[[51]](#footnote-51)…” The whole process of hand selecting inclusions from a preexisting surface (which poses its own problems) is the very definition of a non-random sample. Inclusions which may not be obvious under the microscope may not be collected even though they offer key provenance information. Easily observed inclusions may be over-sampled. The authors provide no explanation for how they avoid these specific pitfalls. Given that the authors provide no evidence that the results of their SPA outcomes were rigorously tested to document the methods effectiveness by comparing SPA outcomes against known and accepted standards, the methodology remains unvalidated and the results are all suspect.

The SPA reliability[[52]](#footnote-52) designations defined by Goren et al. (2004) are meaningless. There has been no testing to show that these category designations are reliable in the way that the petrographic point counting methodology has been tested (van der Plas and Tobi 1965; Dennison 1966; see also references in Flügel 1982). The authors’ categories are arbitrary and clearly, have been defined to create the illusion that there is rigorous statistical analysis to support the reliability assurance assignments.

Cohen-Weinberger and Goren (2004) make reference to the fact that they use their own petrographic database. The authors do not tell the reader how many reference raw materials are contained in this database or the geographic distribution of samples from across the Levant. With this information unreported, investigators cannot know if this database is representative of the range of raw materials that potters may have used. Cohen-Weinberger and Goren then indicate that their database contains “thin sections of pottery from most of the significant archaeological sites in the southern Levant.[[53]](#footnote-53)” The authors do not mention how this database was determined to be composed of sherds reliably identified as locally manufactured. Given the uncertainties associated with this comparative database, and the unwillingness of the authors to better define the database content, provenance determinations using this information will always be questioned. The fact that their petrographic observations are qualitative rather than quantitative and the classification criteria have not been subjected to any sort of rigorous statistical analysis makes their A through C reliability criteria meaningless.

The reliability indices used are not static. They change from report to report. Goren et al. (2004) uses five reliability categories; Cohen-Weinberger and Goren (2004) use three. They are named and defined differently in each manuscript. The authors do not provide a rigorous definition for how a petrographic assessment will be assigned to a category. The criteria vary from manuscript to manuscript and are subjective – to be applied to the data collected in that study rather than generally applicable to other studies. The subjective nature of the criteria means that they can be manipulated to deliver required outcomes.

**Sampling for Geochemical Analysis**

Since chemical analysis was used extensively in Goren’s book on the Amarna tablets and provenances established there were used in provenancing MB pottery at Tell el-Dab`a and since Goren and his colleagues have been highly dismissive of another chemical technique (NAA), it is important to assess their geochemical methodology.

There is a major body of literature concerned with sampling and processing methods to develop procedural standards for ICP-MS, ICP-AES and ICP-OES analyses. Different materials typically require custom standards for sampling and processing to ensure reliable results. Many of these standards are codified as ISO or ASTM standards that can then be purchased from the International Standards Organization (ISO[[54]](#footnote-54)) or the American Society for Testing and Materials (ASTM[[55]](#footnote-55)). The interested reader should view the US Department of Energy website (USDOE 2019) for a list of 400+ procedural studies on ICP-MS methodology. Similar compilations almost certainly exist for the other analytical techniques as well.

It is not our purpose to review the standards literature. Our concern is whether or not Goren et al. (2004) had collected representative samples from the clay tablets they were studying. Their description of the sampling methods shows conclusively that they did not. Given the miniscule sample sizes collected, which by the authors’ own admission was “in most cases … far smaller than the 250 mg of material that is commonly recommended for ICP analysis of ceramic materials[[56]](#footnote-56)”, this methodology is problematic from many perspectives.

First, the authors do not reference the source of the 250 mg minimum size. Goren (1996) chooses to use 500 mg also without referencing this choice of quantity. A review of recommended sample size made by laboratories and standards groups for analysis of sediments and archaeological materials indicate that minimum sample size should be between 1 g and 8 g (Michigan State University 2019; Sandström et al. undated; among many others). Thus, the clay tablet samples appear to be 1 to 2 *orders of magnitude* smaller than the recommended minimum size. Minimum size requirements are not set at random. Minimum size requirements are specified as part of the ISO/ASTM standards setting process and as part of the specialized research into instrument performance as indicated in the list of studies cited in USDOE (2019) and other such document compilations. This author has personal experience in submitting an undersized sample to a commercial analytical lab for an ICP-MS analysis. The laboratory notified the author that the sample was too small for analysis (about 50% smaller than required, not several orders of magnitude smaller as were the samples submitted by Goren et al. 2004. After authorizing the lab to process the sample, the lab results were accompanied with a letter clearly stating that the results were unreliable because of sample inadequacy.

It is true that smaller samples of biological materials may be analysed, but the purposes of those analyses are very different from trying to establish provenance of pottery clay, so the use of smaller samples in this special case cannot be used as a potential justification for the small samples used by Goren et al. (2004). It is also possible that homogeneous manufactured goods like glass and metal alloys that exist as liquids before being fashioned into solid artefacts might yield accurate analyses from smaller samples. However, clay tablets do not fall into such a category. The descriptions by Goren et al. (2011) of the structural characteristics of several of the tablets indicates their potential to be nonhomogeneous[[57]](#footnote-57). When clays accumulate in any natural depositional system, some fraction of non-clay sized grains is likely to accumulate as well. Almost certainly silt sized grains will be present and possibly sand fractions also. The clay is further modified by the purposeful addition of tempers. Flanagan (1967, 1986) among many others describes the process of creating a homogeneous material from rocks and sediments. No clay tablet maker would have followed the process described by Flanagan (1967, 1986). Moreover, the addition of temper undoes the process of making a homogeneous material by purposefully adding inhomogenieties (i.e. temper grains). Flanagan (1986) and SGS (2014) among many others address issues of sampling and homogenizing nonhomogeneous materials in order to get an accurate representative analysis of their trace element profiles. In all cases, this starts with a sample large enough to sample as many as possible of the potential inhomogenieties within the material to be tested.

The origin of the samples sent for trace element analysis is problematic. The authors state: “In most cases the samples were collected from the sediment that crumbled from the tablets in the process of peeling.[[58]](#footnote-58)” Nowhere in the methodology section do the authors discuss specific precautions taken to ensure that surface contamination derived from the burial medium and from materials deposited from circulating/percolating ground waters were not included with the material sent for analysis. In a previous section they state: “This method is almost non-destructive, requiring a sample of only a few milligrams that can be taken as tiny grains from one or several hidden or fractured spots in the artefact’s surface.[[59]](#footnote-59)” If these statements actually reflect the level of precautions taken to minimize contamination, then there is a high probability that the trace element profiles include a significant contribution of material from the burial medium, ground water deposits, and even museum dust! Given the small sample size, the trace element contribution from contaminants is likely to be disproportionate and the trace element profiles therefore meaningless.

**Geochemical Data Analysis and Interpretation**

Under methodology, Goren et al. (2004) describe their basic assumptions regarding the interpretation of the meaning of their geochemical data. Most of their assumptions are based on an inaccurate understanding of major, minor, and trace element geochemistry, and therefore, are mostly incorrect. Confidence in their understanding of the significance of their geochemical findings is further eroded by their tendency to use technical terminology incorrectly throughout (see discussion in a later section), further indicating that they do not understand the science behind the words they are bandying about.

In describing ICP-AES and ICP-MS results, the authors make the following statement: “However, due to the small size of most samples the results of several elements were inaccurate and omitted from the list.[[60]](#footnote-60)” In making this statement, Goren et al. (2004) declare to the reader that the entirety of their geochemical results are unreliable. The statements that the authors make in the paragraph containing this sentence confirm their lack of understanding of basic geochemical reality. The authors have placed themselves into an inescapable trap. By admitting that their sample size is too small for *some* elements, they are *de facto* admitting that the sample size is too small to provide any reliable results whatsoever. One cannot declare for a given sample that some parts of the analysis are correct while other parts are not. The analysis as a whole is either correct or it isn’t.

The very small sample size has another potential impact on the accuracy of the geochemical analyses. The trace element profile of the sample can be made unrepresentative of the true trace element profile by the random inclusion of a single rare mineral grain. Because the sample size is so small, rare grains contribute disproportionately to the trace element profile.

The geochemical methodology is so poorly described, the reader is left speculating about what Goren et al. (2004) did. Presumably the elements being excluded are elements that fell below the level of detection. It is unclear if different elements are being excluded from different analyses. Comparing table 3-1 with other tables in the report suggests a certain level of inconsistency in the elements being included for different analyses. The authors cannot address the issue that the absence of trace elements may be provenance diagnostic, because they can’t verify if elements are below the level of detection due to inadequate sample size or because of primary depositional absence. The way this methodology statement reads, it could be concluded that the analyses might have been rejected because they did not fit preconceived interpretations.

Goren et al. (2011) clearly state that they remove elements from the database that are below the level of detection (LOD). Being below the LOD is a datum point. It means that a particular element (or more typically a suite of elements) is not present in the analysis. Tablets with and without these elements are therefore likely to have different provenance. Excluding this information from the database potentially changes the outcome of the statistical analysis based on this data.

Developing a nondestructive trace element analytical petrographic method would be a tremendous advance for provenance determinations. It would allow more vessels to be tested quickly and cheaply. The portable X-ray diffraction analyser is just one of several technologies that have been been developed and are in widespread use in mining and mineral exploration, materials testing, factory materials quality control, and other similar applications. It was encouraging to see Goren et al (2011) testing one of these technologies to determine its usefulness for provenance determination. Unfortunately, Goren and coworkers’ testing methodology was seriously flawed, and raised more questions that it answered regarding the usefulness of the technology for his objective.

Goren used the Niton™ XL3t GOLDD+ XRF Analyzer[[61]](#footnote-61). In 2013, I used a similar product made by this manufacturer during a copper exploration program in Afghanistan. These devices have several preprogramed analytical algorithms designed to optimize analytical efficiency for the intended application. Goren et al. (2011) used the mining and minerals setting which is likely the most appropriate, but not the only setting, available. Goren had the opportunity to test the capabilities of this instrument and presumably had access to comparative NAA data for the tablets that he tested. When a new technology is introduced a full range of tests would ordinarily be performed to define best practice standard operating procedure. For this instrument, one might expect that the initial procedural testing would be done on a great many different places on each tablet, perhaps 10 to 20, maybe even more places in order to determine the optimal number of sample sites that should be analysed per tablet to provide stable average analyses. ThermoFisher provides recommended minimum run times for each analysis. It would have been wise to test whether longer run times would provide more accurate analyses. The Niton™ I used in the field had 4 preprogrammed analytical algorithms. The instrument used by Goren likely had a similar number. It would have been wise to test all of the algorithms to see how the results differ and to see if different algorithms yielded better analyses for different suites of trace elements. These are the kinds of experiments that would be run to establish best practice. Goren and coworkers did not do this. They did three analyses per tablet at various combinations of recommended settings, declared a successful outcome, and then went on to use the results to discuss provenance groups. They did not even compare their results to the existing NAA data.

According to conversations I have had with U. S. Geological Survey (USGS) chemists who routinely use x-ray diffraction in the lab, very low concentrations of some trace elements, especially in complex materials like rocks (or pottery clay) can confuse the algorithm, thus yielding less accurate results. For the work we did in Afghanistan, USGS provided a number of helpful suggestions that significantly improved the performance of our instrument under the conditions we were testing. The Niton™ can nominally analyze for as many as 30 elements. Goren and coworkers, by the time they eliminate the elements that are below the LOD and elements that are likely to be external contaminants unrelated to provenance, only 14 elements remain for their analysis, and of these, only 7 can be considered trace (V, Cr, Ni, Rb, Sr, Zr, Nb). For the small subset of tablets tested, these may have shown sufficient differences in concentration that allowed discrimination into well-defined groups, but with larger datasets there would likely be too much overlap for effective discrimination.

Goren et al. (2004) make the statement: “In most cases the elemental analyses supplied sufficiently accurate data.[[62]](#footnote-62)” On what basis do they make this statement? They provide no evidence to support this assertion. The authors discuss the “precision” of the analyses as if it is a fundamental verifier of the quality of the data set. Knowing the degree of error that is associated with a particular analytical result is useful information that should be included with the data table. The discussion of this information in the methodology section is irrelevant and seems to have been included to give the impression that the underlying data are of high quality (which they are not). Do the authors understand that even though the results may be very precise, that does not make them accurate? Precision is a measure of how similar multiple analyses of a sample are to one another, NOT an indication that the analytical results are “correct.”

The authors provide an extended discussion of geochemical issues in their statistics section. This discussion is rife with inaccuracies and irrelevant information. The authors state: “One of the shortcomings of ICP for ceramic characterization studies is the deficiency of a database of standards, such as in the case of NAA studies of pottery. Chemical compositions of clay sources collected by other methods often proved to be insufficient, since they included only major and sometimes also minor elements but not traces.[[63]](#footnote-63)” If the authors have no provenance standards to compare their data with, how can they be so certain of their provenance assignments?

Paragraph 2 under the “Statistics” section (page 19) is filled with inaccurate and very confused geochemical observations that, as written, strongly suggest that Goren et al. (2004) do not understand basic geochemical principles. The authors state: “[It] is well known from the geochemical literature, Ca is often associated with elements such as Sr and Ba.[[64]](#footnote-64)” The statement is partially true, but also completely irrelevant. Mg is also closely correlated to Ca. Why was Mg not included in this list of associated elements? Perhaps if Goren et al. (2004) explained why they made this particular observation the reader might understand why the authors think this observation is significant. These three elements may associate, but their proportions relative to one another can still be diagnostic of different provenance (Stanienda-Pilecki 2016).

Goren et al. (2004) state: “Iron, Sc and other transition metals usually exhibit highly

correlated relationships.[[65]](#footnote-65)” With this statement, the authors demonstrate that they have no conception of how the elements of the periodic chart behave in natural systems. What do they mean by correlated relationships? They provide no examples to support or explain this statement. Basic mining geology proves this statement to be completely wrong. Take a simple example— copper deposits. If the transition metals all had correlated relationships, then all of the world’s copper mines should produce the same suite of metals. This is not the case. Some copper mines also produce gold, some gold and silver, some gold and cobalt, some contain molybdenum, zinc, lead and/or arsenic, and some have just copper. It all depends on how and where they formed (Cox and Singer 1992, among others)

Goren et al. (2004) then state: “In pottery, negative correlations often occur between Ca and Si, or Al and Si, as a result of the dilution of clay elements with those of the non-plastic components.[[66]](#footnote-66)” The authors do not reference the basis for these statements, and the statement explaining the observed relationships is either inconsistent with mineral chemistry fact or incorrect depending on which element pair is being considered. This monograph is focused on the provenance of clay tablets. Regarding the first part of the statement in which a negative correlation is said to exist between Ca and Si, the authors state that “marls[[67]](#footnote-67)” are typically used to manufacture the tablets, and that the correlation between Ca and Si in a marl is determined by the proportion of carbonate to clay fractions in the marl. By definition, however, marl is an “intimate mixture of clay and particles of calcite or dolomite” (AGI 1962), so why do the authors ignore Mg in this discussion? The substitution of Mg for Ca (i.e. the degree of dolomitization of the marl) will also affect the Ca-Si proportions. The addition of temper will further modify these relationships.

The second part of the statement— that there is a negative relationship between Al and Si— is even more difficult to defend. Clay is an aluminosilicate. By definition the relationship between Al and Si is determined by the formula of the clay mineral(s) present. An extraordinary amount of temper would have to be added to make major changes to the Al-Si ratios, and most minerals also contain Al and Si in their crystals, so the tablet maker would have to be very specific about the choice of temper to significantly alter the Al:Si element ratios.

Goren et al. (2004) state: “It is also known that rare earth elements (REE) are commonly correlated.[[68]](#footnote-68)” The authors do not explain the significance of this statement to the present study. This information is irrelevant, because the nature of the correlations between various REEs is determined by the geochemical processes that formed the source rocks from which the REEs are derived, so the proportions and correlations are very diagnostic of provenance (for example see Temple and Walsh 1994; Bounouira et al. 2007; Randive et al. 2014; Koç et al. 2016; among many others).

The third paragraph under “Statistics” is filled with more errors and basic chemistry misunderstandings. SiO2, Al2O3, Fe2O3, TiO2, P2O5, CaO, MgO, and SO3 are not elements[[69]](#footnote-69), they are compounds. Most of these happen to be minerals as well.

Goren et al (2004) state “…in the statistical analyses several elements were omitted due to the risk of bias.[[70]](#footnote-70)” They do not tell us which elements are being excluded from the analysis, nor do they definitively say whether these elements are excluded from every analysis or just certain analyses. The authors do not clearly describe which statistical analyses are being performed. Goren (1996) reports the ICP-AES results for only 16 elements. Normally between 30 and 50 elements would be run in this kind of analysis (USGS 2013). No statistical analyses are performed on this reduced data set. Only two element pairs are hand plotted. The author does not explain why he is using such a small element suite or why a more rigorous statistical analysis is not being performed.

In an attempt to explain their various omissions Goren et al (2004) make a series of inaccurate or incorrect statements. They say: “Several elements, especially those with high ionic charge or ionic radius, are more sensitive to post-depositional processes that may occur in buried ceramics due to their solubility in groundwater.[[71]](#footnote-71)” The authors confuse ionic charge with electronegativity. An element can have a high ionic charge (as most transition metals do) and still be relatively insoluble, because their electronegativity is not great and they are bonded covalently rather than ionically with an anion. The iron in Fe2O3 has a high positive ionic charge (3+), but is water insoluble due to its covalent bond with oxygen, which has high electronegativity. The sodium in NaCl has a lower ionic charge (1+), but is extremely water soluble, because the very high electronegativity of chlorine draws an electron away from sodium.

The discussion of ionic radius is both completely irrelevant and wrong. The authors seem to say that ions with a large atomic radius are easily soluble. Gold and cesium have similar atomic radii. Gold is virtually insoluble in water (or just about anything else except aqua regia); cesium is readily soluble.

The authors do list several elements and compounds that they exclude from the analysis, but it is unclear from their description whether these are the ONLY elements and compounds being excluded or if there are others. Their argument for excluding P and S is reasonable. Their argument for excluding Co and Ba is not. Their argument against including Co is basically an admission that their entire data set is unreliable. Goren et al (2004) do not explain on what basis they believe that ONLY Co is being affected by the small sample size[[72]](#footnote-72). In reality, if Co is affected by the small sample size, so is every other element in the analysis. Their decision to exclude Ba is another indicator that they don’t understand the geochemistry of the depositional systems they are working with. They are excluding Ba (as barite Ba SO4) for the same reason that they exclude gypsum (CaSO4·2H2O), namely that gypsum is a contaminant that is commonly deposited from circulating groundwater. However, barite (BaSO4) is not precipitated from circulating ground waters unless there is a major hydrothermal system someplace in the watershed. High concentrations of Ba in a clay is *highly* provenance diagnostic.

Goren et al. (2011) perpetuates the Ba fallacy. These authors cite a study by Katz et al. (2009) as proof that barite behaves identically to gypsum and precipitates from ground water. Goren et al. (2011) specifically reference the fact that that the barite is found in Lake Lisan seismites. This description should have been an alert to these workers that something unusual is happening to the Lake Lisan sediments and that, perhaps, the origin of the barite may not be due to simple ground water evaporation. The geology of barite is discussed in detail by Johnson et al. (2017). where it is pointed out that barite is not especially mobile in ground water environments and thus requires special conditions to become concentrated. According to Johnson et al. (2017), virtually all barite deposits are associated in some way with magmatic systems. The Dead Sea (i.e. the residual Lake Lisan) is part of the Red Sea Rift, which continues to be volcanically active (Weinstein and Garfunkel 2013). Continued deep seated magmatic movements cause the earthquakes that produced the seismites. Evidence of hydrothermal circulation has also been found on the floor of the Dead Sea (Ben-Avraham and Ballard 1984). Given the rhyolitic composition of some of the lavas in the rift zone, at least some of the magmas have the correct composition to supply Ba to the hydrothermal waters. So, the exclusion of Ba by Goren et al. (2011) is another example of excluding a provenance diagnostic trace element.

Goren et al. (2004) make confused arguments about various elements being correlated to one another without discussing what such relatedness may mean for their provenance analysis. Petrography can provide insights into manufacturing techniques, firing temperature, tempering practices and mineral constituents, but it cannot provide a detailed fingerprint of provenance diagnostic mineral chemistry. To understand this issue, consider this simple example—plagioclase. Plagioclase is a solid solution series with end members having the formula NaAlSi3O8 (albite) and CaAlSi3O8 (anorthite). Na and Ca freely substitute for one another in the plagioclase crystal structure and virtually any ratio of Na:Ca can exist. Sometimes a small amount of K might also present in the crystal structure. The ratio of Na:Ca in a plagioclase is determined by the composition of the magmatic melt from which it crystalized. Granites produce Na rich plagioclase that may contain a very small percentage of K, mafic rocks contain equal to dominant percentages of Ca:Na in the plagioclase crystal, and ultramafic rocks are dominated by Ca plagioclase almost to the exclusion of Na. If enough grains are present, the petrographer can determine the Na:Ca ratio.

For other minerals, only a chemical analysis will reveal the cation solid solution ratios. For example, consider the heavy mineral monazite. Monazite varieties are named for their dominant cation. Monazite-(Ce) has the chemical formula (Ce,La,Nd,Th)PO4. For monazite-(Sm) the formula is (Sm,Gd,Ce,Th)PO4. And these formulas only account for the *primary* solid solution cations. Any of these monazite species will contain other REE cations at much smaller percentages. The specific ratios of REEs in a monazite are HIGHLY provenance diagnostic, since no two monazite occurrences have exactly the same REE ratios.

That Cohen-Weinberger and Goren (2004) should accept precedence of petrography over NAA[[73]](#footnote-73) indicates that they and presumably other archaeological petrographers who he has trained or influenced do not understand the geochemistry or the geology of the minerals they study. That Goren and his coworkers would argue that sherds with identical NAA trace element profiles but different petrographies must have different provenance is inconceivable. Their argument is backwards. Trace element profiles are as easy to duplicate as fingerprints! If two petrographically distinctive sherd assemblages have identical NAA profiles it is incumbent on the archaeologist to explain the cultural reason why these different assemblages co-occur in time and space. As a trained geologist and petrographer, I would always allow the chemical evidence to take precedence over the petrographic evidence.

**Clay-Temper Factor (CTF) and the Use and Interpretation of Geologic Maps**

Goren and his coworkers depend almost exclusively on geologic provenance data acquired from the study of geologic maps for all the articles and book under review here.. The concept of the CTF (i.e. the Clay-Temper Factor[[74]](#footnote-74)) indicates that these workers have a fundamental misunderstanding of basic geologic principles and sedimentologic processes. From the description explaining how the authors calculate the CTF, it is clear that the authors assume that a formation is uniform throughout the area of its occurrence. Furthermore, the assumption is made that the description of the formation’s characteristics provided on summary large scale geologic maps accurately describe the characteristics of that formation everywhere that it occurs. Nothing could be further from the truth. Consider the characteristics of some of the formations mentioned by Goren et al. (2004). The Pakhna marls[[75]](#footnote-75) of Cyprus are mentioned. From reading their discussion of this rock unit, one would conclude that it is a marl throughout its area of occurrence. In fact, the Pakhna Formation “…records heterogeneous, mainly carbonate, sedimentation… The succession begins with deep-water pelagic carbonates and shows increased input of shallow-water bioclastic and terrigenous sediment upward” (Eaton and Robertson 1993). Pelagic carbonates do not contain significant clay fractions and would not be suitable for clay tablet manufacture. A variety of other non-marl lithologies are also described in this formation.

In the same sentence, Goren et al. (2004) mention the Moni clays. The Moni Mélange (see Robertson 1977) is a rock type that forms in a subduction zone. A mélange can literally contain any kind of rock that can be imagined. The rocks that it WILL contain are whatever rocks happen to be on the edges of the crustal masses that are colliding. A mélange will consist of sediments, metamorphic, volcanic, and plutonic rocks (often including ophiolites). Clays that are derived from such rocks will have whatever character the local rocks allow. Derived clay characteristics can change radically over a distance of a few hundred meters.

Of specific importance to this critique is what Goren has to say about the site of Byblos in the northern Levant, which Bietak posits as the origin of the Tell el-Dab`a population. Goren et al. (2004) state: “The ancient mound of Byblos is located in an area where Neogene to recent marine sediments were deposited. These include a series of chalks, clays, marls and sand dunes.[[76]](#footnote-76)” This description appears to be derived directly from the literature and not from an on-site investigation of the actual clay sources. It is true that the Mio-Pliocene deposits contain marls, but a review of the lithologic descriptions of this rock sequence indicates that in the 1500 m thickness of this sequence, marls are present in limited parts of the section (Buchbinder and Zilberman 1997). Even though this rock unit may be present around Byblos, the marl-containing part is not necessarily exposed.

Such misinterpretations of lithologic descriptions contained in geologic reports are pervasive in Goren and his coworkers’ manuscripts. For every formation named in Goren et al. (2004), a review of the published lithologic descriptions for that formation is likely to reveal that formation composition is far more complex than the synoptic descriptions offered on a geologic map. As the Eaton and Robertson (1993) description shows, formations can vary significantly in character and composition laterally. A formation that is marl in one place could be a sandy limestone several kilometers away. This lithologic change will not be noted on a geologic map.

Given the lateral complexity of sedimentary systems, it is not surprising that one might want to reduce this complexity to a simplistic map reference. This strategy removes the need to find actual clay or temper deposits with characteristics similar to the artefact. Using a map reference allows one to simply postulate that suitable clay/temper deposits must be nearby, because the map shows formations that are likely to have appropriate lithologies in the general area. By not making an effort to collect and analyze representative clays and tempers from around an excavation, Goren and his coworkers miss an opportunity to improve existing databases and to develop a better understanding of sediment geochemistry variations that will significantly improve future provenance determinations. Goren and his coworkers prefer to ignore the fact that modern NAA databases are so useful today because they are the product of 50 years of data accumulation. Instead of arbitrarily declaring that these databases are flawed and then using untested and seriously flawed analytical methodologies that deliver unreliable provenance data, Goren would contribute more to ongoing research by working to properly expand existing databases.

Goren et al. (2004) and Cohen-Weinberger and Goren (2004) make constant reference to the fact that the geologic maps show that suitable formations are locally present to provide clay for pottery manufacture. Implicit in their statements is the assumption that the clay is derived from these formations, but never do they point to a quarry where this clay had been mined from the named formation. Clay for pottery manufacture can be obtained in two ways. The rock in formations that are predominantly clay can be mined, crushed and processed until a material suitable for a potter has been produced. This is difficult work. It is much simpler to harvest and process soft alluvial clay. If geologic maps are studied where alluvial clay is being mined today to provide material for village potters, the reader will find that most clay pits that do not exploit a specific rock formation will be mapped as “Quaternary alluvium.” The clays in these pits may be derived in part from the erosion of local formations, but they will also be mixed with materials washed in from other places within the watershed. This situation has significant geochemical implications. Clays that are mined and processed directly from *in situ* rock will have the geochemical profile that prevailed when that rock was first deposited. That profile can be completely different from the current watershed’s geochemical profile. But, when that rock is eroded, its geochemistry becomes part of all downstream sediments. For this reason, consideration of the geologic map alone is a poor tool for understanding provenance. The complex origin of alluvial clay deposits requires that an extensive range of reference clays must be collected as comparative materials to support both petrographic and especially chemical testing.

Porat and Goren (2002) and Goren et al. (2003, 2004) make the specious argument that chemical databases are incomplete because atypical clays were chosen to manufacture specialized artefacts (like clay tablets) and chemical analyses of these clays are not contained within the reference databases. If these workers see this as a problem, the simplest solution is for these workers to collect samples of these clays to be analysed and added to the database. Porat and Goren (2002) also make the claim that inappropriate reference materials (like cooking pots and mud bricks[[77]](#footnote-77)) have been sampled to build existing databases and these clay samples are irrelevant. Misconceptions like these make it clear that (a) Goren and his coworkers do not understand the value of building a comprehensive reference database that captures the characteristics of every type of local clay that manufacturers might use, and (b) do not understand the nature of clay deposits. Acquiring as broad a sampling of all of the various types of clays being used by a society increases the likelihood that a properly executed multivariate statistical analysis will more clearly define provenance groups. Pretending that clay tablets are somehow unique and special and therefore their chemistry and petrography cannot be studied using standard methodology[[78]](#footnote-78) is an indefensible argument. This is particularly true when dealing with Levantine marls which are used widely to make both tablets and earthenware vessels throughout the region (Goren 1995, 1996; Porat and Goren 2002; Goren et al. 2003; Cohen-Weinberger and Goren 2004; Goren et al. 2004; Master 2018). This widespread usage of such clays would seem to negate the argument that Goren et al. (2004) make that the compositions of clay tablets are not likely to match any known clay source.[[79]](#footnote-79) This argument seems to have been designed to create an artificial class of objects for which standard analytical techniques can be ignored and new, untested techniques be used instead.

From a petrographic standpoint, Goren’s (1995) comment that “The distribution of the Taqiya Formation is widespread and therefore cannot be used for a definitive provenance distinction[[80]](#footnote-80)” might *possibly* be correct, but given the geology of the Taqiye Formation, properly executed petrographic point counts are likely to reveal minor but systematic regional variations in mineral constituents. Analysis of the trace element profiles would almost certainly show regional variations. To understand why this is true, one needs to consider the Cretaceous and Paleogene paleogeographic reconstructions of the Levant (see Scotese 2001, 2014) and details of the depositional history (see e.g. Kuss et al. 2003). During the Cretaceous, northern Africa and the Levant were submerged beneath a shallow epicontinental sea slightly north of the equator of that time. Carbonate deposition predominated forming reefs and other types of limestone when terrigenous sediment input was limited, whereas marls formed when and where more clay was being added to the depositional system (see Sass and Bein 1982 to get a sense of how quickly local lateral facies changes occur). There were at least three major sea level changes that occurred during this time that significantly altered land-sea boundaries (Flexer et al. 1986). These sorts of changes have provenance implications. A formation like the Taqiye and its various lateral equivalents as discussed in Goren et al (2004) may occur from Morocco to Turkey, Syria, Jordan, and possibly as far east as Iraq, but there will be regional differences in the specific characteristics of this formation. This may be less true for formations like the Moza Formation (Scarpa 1990), which was deposited during a time of relative geologic quiescence in the region, but the Taqiye accumulated during a time of geologic upheaval when the collision between Africa and Eurasia was well underway. All manner of volcanic and plutonic rocks were being erupted and emplaced, and existing rocks metamorphosed leading to the development of very complex regional trace element profiles (Adatte et al. 2005; Dupuis et al. 2005). Without realizing the implications of their statement, Goren et al. (2004) admit the existence of this variability when they cite the Schreier (1988) analysis of the Esna clays (the Egyptian equivalent of the Taqiye). Goren points out that Schreier noted that the clay chemistry changes from bed to bed within the formation[[81]](#footnote-81). By avoiding the systematic sampling of Taqiye marls and their correlatives from around their excavations, Goren and his coworkers have missed the opportunity to better understand the regional variability of the Taqiye marls and the other important marls used in pottery manufacture.

Goren et al. (2004) also makes reference to the Esna being *equivalent* to the Taqiye.[[82]](#footnote-82) The Esna is NOT equivalent, it is correlative. The two concepts differ significantly. The two formations may have been deposited at the same time but they accumulated in different places and under very different erosional and depositional influences.

Goren et al. (2004) spend two pages[[83]](#footnote-83) discussing how modern potters locate their manufactories near to their source of raw materials. The studies referenced by Goren indicate that modern potters rarely forage more than 10 km from their workshops for clay, and significantly shorter distances for temper. On the basis of these modern studies, Goren defines his CTF as being the geologic diversity within a 10 km radius of the manufactory[[84]](#footnote-84). After spending pages defining his criteria for the CTF, it is curious to see Goren abandon these criteria in discussing the Amarna tablets. The reasoning behind the Goren et al. (2004) claim that the Esna shale is the ONLY possible clay source for his selected group of Amarna tablets is difficult to accept when they don’t present comparative analytical chemistry data from any potential clay sources. Instead the authors claim that their conclusions are supported by chemical analyses of clay bodies presented in three geological studies (Basta et al. 1979; Schreier 1988; Ahmed et al. 1992). This evidence is inconsistent with their CTF claim that clay sources are unlikely to be more than 10 km away from the site of artifact manufacture. The samples analysed in the cited geologic reports were collected much farther away than the maximum 10 km from the Amarna site. The Beris Oasis, for example, is more than 325 km southwest of Amarna and more than 240 km distant from the nearest bank of the Nile River. Even if Goren’s argument is correct that the Esna clay was harvested from places distant from Amarna but accessible to river transport, it is unlikely that anyone would travel 240 km into the Western Desert to acquire clay for tablet manufacture.

Using the clay chemistry from these geologic reports introduces additional interpretational problems. By Goren’s admission these reports only provide major element chemistry. Major element analyses provides chemical information about the clay minerals but contains no significant trace element fingerprint data. Schreier’s report confirms the regional variability in trace element chemistry for the paleogeographic reasons discussed above, so it is precarious to suggest that clay sources separated by millions of years in time and by hundreds of kilometers in space would have the same geochemical signatures. And this discussion assumes that Goren’s tablet chemistry data are valid in the first place given the substandard sample sizes (see below).

Goren and his coworkers do not fully understand biostratigraphic applications. For example, in Chapter 3, I. Egypt, they discuss the evidence that the Esna shale is the primary clay source for many of the Egyptian tablets. As part of their evidence, they mention that tablet EA-357 was found to contain several (badly preserved) foram genera that could be referred to the Paleogene and, therefore, must have come from the Esna Formation. The Paleogene Period is very different from the Paleocene Epoch[[85]](#footnote-85). The Paleogene spans the time from 66 MYBP to 23 MYBP, the Paleocene from 66 MYBP to 56MYBP. Though it is true that the Paleocene Esna shales and marls are widespread across Egypt (Aubry et al. 2007; Obaidalla et al. 2015), there are other Paleogene marl bearing formations in the country (e.g. Sallam et al. 2015; King et al. 2017) . These are admittedly minor, but Goren and coworkers have not eliminated these as possibilities.

Map scale and map age are also concerns. Geologists’ understanding of rock units evolves over time and formation definitions become more refined. Old maps[[86]](#footnote-86) rarely reflect modern thinking. The information quality on the map is determined by the capabilities of the mapper. Skilled mappers identify more details and map more accurately than less skilled or inexperienced mappers and map makers (Campbell 2005; Wilson 2016). Large scale maps, by their very nature, will show significantly less detail than small scale maps. One needs only to look at the visual comparison between the two scales to realize that a map can potentially obscure the occurrence of numerous suitable clay or temper containing formation outcrops simply because they are too small to be shown at 1:250,000 but can easily be shown at 1:50,000 (see USGS 2002; USGS 2019). The authors only occasionally identify the age and/or scale of the maps they are using.

**Geologic Terms Misused and Misunderstood**

Goren and his coworkers use geologic terms throughout their manuscripts. Unfortunately, they use terms incorrectly with such frequency that they create the impression that they likely do not understand the science behind the words they use. For example:

* Goren (1996) states: “…argillaceous, ferruginous, shale-rich clay…[[87]](#footnote-87)” This wording is the equivalent of saying clayey clay. Constantly referring to shale as being clayey indicates a lack of understanding of the nature of shale. Porat and Goren (2002) are also fond of referring to clayey clays. From a geologic perspective, this terminology makes no sense and it would appear that the authors do not know the proper terminology to describe what they are observing under the microscope.
* Porat and Goren (2002) refer to a dolomitic clay that is noncalcareous[[88]](#footnote-88). This is terribly confusing terminology since it is, by definition, impossible. Dolomite is calcareous. Likely the author means that there are a small number of dolomite grains present in an otherwise carbonate free clay. They also refer to carbonate poor marls[[89]](#footnote-89) which is a contradiction in terms since marl is defined as “calcareous clay” (AGI 1962)
* Goren (1996), Cohen-Weinberger and Goren (2004), and Goren et al. (2003, 2004) all refer to “grains of arkose.[[90]](#footnote-90)” Arkose is a *sandstone* containing at least 25% feldspar (AGI 1962). The grains of “arkose” Goren et al. (2004) illustrates in Fig. 5 are merely feldspar grains that may or may not be derived from granite or “related igneous rocks.” There is no arkose in Fig. 5. Also, if there are enough twinned plagioclase grains present, it might be possible to determine what specific kind of igneous rock produced the plagioclase based on the Ca percentage in the crystals, which, in turn, could provide additional provenance information.
* Cohen-Weinberger and Goren (2004) make reference to “geode quartz[[91]](#footnote-91).” There is no such mineral. The quartz in a geode is chert usually presenting as agate. There is no way to petrographically determine that a chert fragment originated specifically from a geode since agate from a geode will look like any other chert.
* Cohen-Weinberger and Goren (2004) make reference to “igneous mafic minerals[[92]](#footnote-92)” and then name serpentine as one of these minerals. Serpentine is not an igneous mineral. It is a metamorphic mineral that forms when certain ultramafic rocks (often found in ophiolite sequences) are metamorphosed under water saturated conditions.
* Cohen-Weinberger and Goren (2004) make reference to “igneous mafic minerals” and then name pyroxene as one of these minerals. There are two main groups of pyroxene, orthopyroxene and clinopyroxene, and many different minerals in each group. The specific species of pyroxene can be very diagnostic of provenance. The authors do not discuss this or identify the specific pyroxene present.
* Cohen-Weinberger and Goren (2004) state: “Tuff, dykes and basalts occur in the slopes of Mount Hermon, where Lower Cretaceous volcanics expose nearby Jurassic limestone formations containing fossil reefs.[[93]](#footnote-93)” This sentence demonstrates exceptionally well that the authors are repeating words that they have read in geologic reports without having the slightest understanding of what they these words actually mean. In the referenced sentence, tuff, dyke and basalt are used as if they are three distinct characteristics that in some way help to ascribe provenance. A tuff is a rock made from consolidated volcanic ash. In this case, it is probably a basaltic ash. A dyke is a volcanic rock that has a specific geometric shape and a specific 3D relationship to the other rocks in the field. Again, in this case, it is probably a basaltic dyke, but in other geologic environments dykes are made of other rock types. The sentence then goes on to state that somehow the Lower Cretaceous volcanics cause the Jurassic limestones to be exposed. This statement is an impossibility. Basalts are lavas. They flow out and cover things. Since the basalts are younger than the limestones the basalts would have flowed over and covered the limestones. To suggest otherwise is a violation of Steno’s Law. If the limestones are exposed, it is because some other geologic process removed the basalt to expose the limestone.
* Cohen-Weinberger and Goren (2004) describe calcite inclusions as: “…dense, idiomorphic, silty calcite crystals which tend to be spherical[[94]](#footnote-94)…” This string of words is self-contradictory on several levels. If the grains are rounded, there is no way to know if they were once idiomorphic. It is not clear what the authors mean by “silty.” Presumably there are silt inclusions in the calcite grains which means that their crystal growth was interrupted by the preexisting silt grains and therefore the calcite grains could be regarded as xenomorphic (another word which is used freely in this manuscript). ALSO, Goren et al. (2004) make frequent reference to “idiomorphic” dolomite grains. Calcite and dolomite are relatively soft minerals (mohs hardness = 3) and have perfect rhombohedral cleavage. In all likelihood the frequent references to idiomorphic carbonate grains are really references to mechanically crushed cleavage flakes. Genetically, these are very different from idiomorphic crystals.
* Bentonic[[95]](#footnote-95) is the name of a rock band (of the musical kind). I believe the authors meant to say bentonite or bentonitic. The two instances may be typographical errors.
* In all of the manuscripts reviewed, the authors focus excessively on the geologic age of the source lithology creating the impression that they regard the age as somehow provenance diagnostic, which it is not.

The above examples are just several of the dozens of such examples of misused or misunderstood geologic terminology throughout the various Goren manuscripts. The occasional misuse of a geologic term by a non-geologist can be ignored, but to be confronted by paragraph after paragraph, manuscript after manuscript strongly suggests that the authors are using the terminology in an attempt to sound knowledgeable when, in fact, they have no understanding of the geologic processed and characteristics they are describing. A list of minerals along with the mention of nearby rock types that could be the source of these minerals is more than sufficient to discuss likely provenance. Perhaps the most extreme example of this tendency is the one and a half pages allotted to describing the Troodos ophiolite and associated rocks[[96]](#footnote-96). Goren et al. (2004) provide an exhaustive list of rock types—andesitic basalt, quartz andesitic basalt, quartz microdolerite, olivine basalt, quartz microgabbro, mugearite, dolerite sheet dykes, unspecified detrital minerals (whatever those might actually be), umberiferous olistoliths, reefal limestone, and on and on and on. This entire listing of geologic terminology is meaningless. The authors need to tell us what minerals are present and whether the surrounding geology is likely to produce those minerals. Do the authors even know what minerals make a mugearite?

The discussion of clay geology by Goren et al. (2004)[[97]](#footnote-97) supports the notion that these authors probably do not understand the geology of clay minerals or the depositional systems in which they are found. The discussion of clay as a grain size[[98]](#footnote-98) is archaeologically irrelevant. They discuss clay mineralogy, but they are not clear about the specifics, suggesting that clays are *usually[[99]](#footnote-99)* phyllosilicates. Clays are *alway*s phyllosilicates (Kerr 1977). They create artificial categories that no geologist would recognize. They state that the clay in *in situ* clay deposits forms in a different way from the clay in sedimentary deposits. The authors state: “The first may develop from the chemical alteration of other minerals (such as feldspars) and produce beds of pure clay that may be used for stoneware or porcelain production. The latter are deposits originating from weathering processes of rocks[[100]](#footnote-100)…” All clays are weathering products, whether formed by hydrothermal processes or at surface temperatures and pressure (Velde 1995; Foley 1999). High purity kaolinite clays used for porcelain are nothing more than clays that have not yet been eroded and transported away from their place of formation. They are also clays that form from a very specific rock type, one that is extremely rich in feldspar minerals.

Cohen Weinberger and Goren (2004) state that Group J sherds contain abundant inclusions of quartz grains which “indicates an aeolian contribution from the coast.[[101]](#footnote-101)” They provide no documentation to support this claim. Aeolian quartz has a number of distinctive characteristics— very narrow range of grainsizes (usually very fine and fine) with the grains being generally well rounded, pitted and frosted. None of these characteristics are mentioned in their description. The authors provide no basis at all for assigning Group J to the Shephelah area other than it is near the coast. Assuming that the quartz is actually a dune sand, it more likely originates from aeolian desert dunes, which cover a significant part of the Shephelah study area.

**Statistical Analysis**

Goren et al. (2004) indicate that they use the STATISTICA software as their primary statistical tool, but they do not tell the reader how they analyze their data. The statistical methodology section is mostly devoted to incorrectly describing geochemical concepts (as discussed above). They have filled the statistics section with a lot of statistical terms (scattergrams, bivariate plots, multivariate statistics, principle component analysis, cluster analysis, lognormal distribution, quasi-standardization, hyperspherical space, etc.), but there is no discussion of how the analyses were run or what data were used. Their methodology section leaves too many questions unanswered:

* Which data are being input for the cluster and principal components analysis? Is it just the (unreliable) geochemical data or are the qualitative petrographic data also included as observed characteristics?
* When a statistical analysis is run, which tablets or pottery are included in the run?—all samples in the study or just selected subsets?
* Are the geochemical results from the local materials included with the petrographic analyses?
* Why do we never see the outcome plots of the PCA or the cluster analyses?

Ultimately we are being asked to simply accept that Goren and his coworkers did a thorough analysis without them being required to show us the actual clusters/groupings. The disjointed and meandering discussion of statistical methodology and the bandying of statistical terminology without any substantive discussion of the analytical design gives the reader the impression that the Goren team does not really understand the nature of the analyses they are performing.

For every group of tablets analysed, Goren et al. (2004) always refer to observed clusters, but they never show the graphical results of the cluster or PCA analyses. Consequently we never know which samples specifically were included in the analysis or which vessel characteristics were used to run the clustering/PCA statistics. In the entire report there is one PCA illustrated (Fig. 3.2) and one cluster analysis (Fig. 3.4). Since the reader does not know how tightly constrained the character input data are, the reader is not in a position to evaluate if the clusters identified by Goren and coworkers are realistic.

Goren and his coworkers have amassed a significant collection of sherd and raw clay thin sections. According to Porat and Goren (2002) this collection contains more than 8,000 thin sections (as of 2002) and serves as their primary reference database for comparing new study material to the various defined provenance categories (Goren 1996; Porat and Goren 2002; Masters 2018). Though Goren and his coworkers frequently reference this database, they do not provide details about its specific composition. Of particular interest would be the number of raw clay samples included and how many of these clays have associated geochemical data. Given that Goren and his coworkers tend to be overly reliant on geologic maps, there is significant concern that analyses of raw clays are underrepresented in this database. Nor do we know the nature of the types of chemical analyses that were performed. NAA is the accepted standard trace element analysis methodology for archaeological materials. Goren et al (2004) admit that (a) ICP-MS/AES methodologies are not as commonly used in provenance analyses, and (b) that ICP-MS/AES results cannot be directly compared to NAA results. By using this nonstandard methodology, they are reducing the size of the comparative regional chemistry database.

A regional sampling of 8,000 sherd and clay thin sections provides an ideal opportunity to test how well the subjective observational provenance categories compare to objectively determined statistical categories. Given that this thin section database has been petrographically studied, it is surprising that such a statistical analysis has not been performed. A successful analysis should provide objective support to the subjective sherd categories. On the other hand, since rigorous point counting does not seem to be used by Goren and his coworkers, there would seem to be a lack of objective point count data for most, if not all, of this thin section collection.

**Concluding Comments**

There are many significant methodological errors throughout the reviewed reports by Goren and his coworkers. Some of them might be considered relatively minor, but the majority are sufficiently serious that they bring into question the quality of the data being acquired, and therefore the validity of the conclusions drawn. To summarize:

* The methodology sections are poorly written. They are confusing, they meander, and they often contain information irrelevant to the methodological discussion. It is often difficult for the reader to understand what Goren and his coworkers are actually doing. Often, their methods have to be inferred based on this reader’s preexisting familiarity with the techniques being discussed.
* Goren and his coworkers incorrectly use geological, geochemical, chemical and statistical terminology throughout their manuscripts creating the impression that they do not understand the underlying science behind the technologies they are applying to their studies.
* Goren and his coworkers do not understand geologic processes. This causes them to draw unwarranted conclusions from geologic reports. There are innumerable instances that can be cited.
* Geologic maps can be very helpful in explaining why particular mineral suites or trace element profiles occur in wasters and modern clay and temper samples collected from potential pottery clay and temper sources near the excavation. Geologic maps, however, are NOT a substitute for collecting and analyzing potential clay and temper sources that the ancient potters may have utilized. Geologic maps do not contain the level of information required to correlate a rock formation to a clay or temper source. Goren and his coworkers do not consider or understand the degree of lateral variability that formations characteristically exhibit both in their mineral and trace element profiles.
* After decades of testing, Standard Operating Procedures (SOP) are well established to guide sample collection from the materials to be studied by the analytical technologies being used by Goren and his coworkers. Many of these SOPs are formally published as ISO or ASTM standards. Goren and his coworkers ignore many of these standards in favor of introducing untested methodologies.
* Goren and his coworkers do not follow the rigorous testing procedures required to prove that new methodologies are effective. Their description of how these techniques have been tested show how poorly based their approach is. The SPA methodology is the very definition of non-random sampling, so the information it provides will skew any analysis that results.
* Systematic point counts were never reported in any of the cited petrographic studies. As a result, the petrographic analyses are all qualitative, and, therefore, can more easily be manipulated by researchers to conform to preconceived provenance categories.
* It is unclear why Goren and his coworkers would have chosen ICP-MS/AES as their primary analytical tools when by their own admission, the analyses are not comparable to NAA. NAA is the standard analytical methodology used by archaeologists and geologists in the region and contains the largest comparative database for provenance determinations.
* The samples collected for the ICP-MS/AES are too small to provide accurate chemical analytical results.
* The samples collected by “peeling” a tablet are too small to provide a statistically significant petrographic analysis.
* The workers do not clearly explain why certain results are excluded from their analyses. It is also unclear if the same elements are being excluded from every assessment or if different elements are excluded from different assessments.
* The absence of quantitative point counts makes it more difficult to objectively code petrographic characters for cluster or principle components analysis. For example, should the presence of a single unusual mineral grain in a thin section carry the same weight as a thin section in which 10% of the inclusions are of that mineral type? In a qualitative analysis, it is much easier to code the characters to create the appearance that the two thin sections are very similar for this character.
* The authors discuss numerous statistical tests that have supposedly been applied to their data, but they never illustrate the results of these tests. The readers are expected to accept their written description of what the analyses revealed. Nor do they describe carefully what input data were used, most specifically if the petrographic results were tested statistically.

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11. E.g., Y. Goren, The Southern Levant in the Early Bronze Age IV: The Petrographic Perspective, *Bulletin of the American Schools of Oriental Research* 303 (1996), pp. 33-72. [↑](#footnote-ref-11)
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43. MB = Middle Bronze. Sample nos. are cited in the following order as provided: grid no, square no., locus no., fill no., basket no. (if more than one, indicated by +.(basket no.)). The Israel Antiquities Authority license no. is followed by the year that it was issued. The pottery typological data provided by the Ashkelon expedition and Y. Goren were cursory (e.g., paint colors and other specific features were not described), and figures and/or plates were not provided to double-check the descriptions. Also, the specific MB II phases to which the samples should be assigned were not provided. [↑](#footnote-ref-43)
44. nn = no number. [↑](#footnote-ref-44)
45. MC = material culture. [↑](#footnote-ref-45)
46. *Ashkelon 6* = *Ashkelon 6: The Middle Bronze Age Ramparts and Gates of the North Slope and Later Fortifications*, Final Reports of The Leon Levy Expedition to Ashkelon, eds. L. E. Stager, J. D. Schloen, and R. J. Voss, University Park, PA: Pennsylvania State University Press and Eisenbrauns, 2018. See chapter 12 (D. Ben-Tor and L. Bell, ”Clay Sealings from the Moat Deposit”) for the catalogue entries., and chapter 13 (B. Brandl, “Morphology and Function of the Sealings from the Moat Deposit” for the figures). [↑](#footnote-ref-46)
47. This table is based on a modified version of table 3 in Cohen-Weinberger and Goren 2004, together with an unpublished 1997 manuscript of Yuval Goren, submitted for publication in this monograph. Corrections to the original table include substituting “? (possibly Southern Palestine)” for “most similar to Southern Palestine,” because these NAA assignments are **uncertain** according to stringent statistical criteria. The phrase “Most similar to” is only used where NAA samples are within a 0.1 mean Euclidean distance of the defined regional group (see the discussion in Chapter 1 of the monograph).

    Note that the following petrographic samples were omitted from the 2004 table, although they were examined according to the 1997 table: JH103 (Red Polished Dipper Juglet), JH118 (Canaanite Jar), and JH833 (Black Polished Juglet). No explanation was provided for these omissions.

    + = good agreement; - = poor agreement; ? = possible agreement according to Cohen-Weinberger and Goren 2004: 84-85 (appendix), but here parenthetically corrected as “? (should be -)” or “? (should be +) due to “no information” or questionable NAA provenance; and N.D. = “not determined,” but here parenthetically corrected as “N.D. (should be -)” due to “no information” or questionable NAA provenance ? is reserved for truly questionable provenances in all instances, rather than possible matches (Cohen-Weinberger and Goren 2004: 85). [↑](#footnote-ref-47)
48. The petrographic microscope was invented in 1829, the first mass spectrometer was built in 1927, and the first optical emissions spectrometer built in 1937. [↑](#footnote-ref-48)
49. See page 11, line 31 of Goren et al. (2004). Line citations include [↑](#footnote-ref-49)
50. See page 11, lines 19-21 of Goren et al. (2004). [↑](#footnote-ref-50)
51. See page 11, lines 34-37 of Goren et al. (2004). [↑](#footnote-ref-51)
52. See page 14, Sample Evaluation section of Goren et al. (2004). [↑](#footnote-ref-52)
53. Cohen-Weinberger and Goren (2004), page 71, col. 1, lines 34-36. [↑](#footnote-ref-53)
54. A catalogue of the 22,500+ ISO standards can be obtained at: <https://www.iso.org/standards-catalogue/browse-by-ics.html>. [↑](#footnote-ref-54)
55. A catalogue of the 12,000+ ASTM standards can be obtained at: <https://www.astm.org/BOOKSTORE/BOS/index.html>. [↑](#footnote-ref-55)
56. Goren et al. (2004), page 13, lines 9-10; page 63, line 11. [↑](#footnote-ref-56)
57. Goren et al (2011) page 687, column 2, lines 1-4. [↑](#footnote-ref-57)
58. See page 13, lines 5-6 of Goren et al. (2004). [↑](#footnote-ref-58)
59. See page 11, lines 23-24 of Goren et al. (2004). [↑](#footnote-ref-59)
60. See page 13, lines 16-17 of Goren et al. (2004). [↑](#footnote-ref-60)
61. https://www.thermofisher.com/order/catalog/product/XL3TGOLDDPLUS. [↑](#footnote-ref-61)
62. Goren et al. (2004) page 13, lines 10-11; page 63, lines 13-14. [↑](#footnote-ref-62)
63. See page 19, Statistics section, 1st paragraph of Goren et al. (2004). [↑](#footnote-ref-63)
64. See page 19, lines 26-27 of Goren et al. (2004). [↑](#footnote-ref-64)
65. See page 19, lines 28-29 of Goren et al. (2004). [↑](#footnote-ref-65)
66. See page 19, lines 29-30 of Goren et al. (2004). [↑](#footnote-ref-66)
67. See page 6, line 3 of Goren et al. (2004). [↑](#footnote-ref-67)
68. See page 19, lines 30-31 of Goren et al. (2004). [↑](#footnote-ref-68)
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70. See page 19, lines 36-37 of Goren et al. (2004). [↑](#footnote-ref-70)
71. See page 19, lines 37-39 of Goren et al. (2004). [↑](#footnote-ref-71)
72. Goren et al. (2004), page 63, lines 13-14. [↑](#footnote-ref-72)
73. Cohen-Weinberger and Goren (2004), page 85, col. 2, lines 20-22. [↑](#footnote-ref-73)
74. See page 4, paragraph e under Clay Production Systems in Goren et al. (2004). [↑](#footnote-ref-74)
75. See page 76, line 27 of Goren et al. (2004). [↑](#footnote-ref-75)
76. See page 134, lines 13-14 of Goren et al. (2004). [↑](#footnote-ref-76)
77. Porat and Goren (2002), page 467, column 1, last 3 lines. [↑](#footnote-ref-77)
78. See Goren et al (2004), page 9, last 5 lines. [↑](#footnote-ref-78)
79. Goren et al. (2004) pages 9 bottom and page 10 top. [↑](#footnote-ref-79)
80. Goren (1995), page 291, lines 15-16. [↑](#footnote-ref-80)
81. Goren et al. (2004) page 30, lines 33-34. [↑](#footnote-ref-81)
82. Goren et al. (2004) page 24, geologic interpretation of EA-1. [↑](#footnote-ref-82)
83. Goren et al. (2004), page 5-6. [↑](#footnote-ref-83)
84. Goren et al. (2004) page 6, lines 33-34. [↑](#footnote-ref-84)
85. See the geologic time scale on Wikipedia: <https://en.wikipedia.org/wiki/Geologic_time_scale>. [↑](#footnote-ref-85)
86. See page 17, line 16 of Goren et al. (2004). [↑](#footnote-ref-86)
87. See page 38, col. 2, lines 13-14 in Goren (1996); Cohen-Weinberger and Goren (2004), page 73, col. 1, line 15. [↑](#footnote-ref-87)
88. Porat and Goren (2002), page 474, col. 2, line 20. [↑](#footnote-ref-88)
89. Porat and Goren (2002), page 471, col. 2, line 5. [↑](#footnote-ref-89)
90. See page 47, column 1, line 2 in Goren (1996). [↑](#footnote-ref-90)
91. See Cohen-Weinberger and Goren (2004), page 73, col. 2, line 19. [↑](#footnote-ref-91)
92. See Cohen-Weinberger and Goren (2004), page 73, col. 2, line 21. [↑](#footnote-ref-92)
93. See Cohen-Weinberger and Goren (2004), page 76, col. 2, lines 46-50. [↑](#footnote-ref-93)
94. See Cohen-Weinberger and Goren (2004), page 76, col. 2, lines 21-22. [↑](#footnote-ref-94)
95. See Cohen et al. (2004) p. 62, line 3 and p. 109, line 34. [↑](#footnote-ref-95)
96. See Goren et al. (2004) p. 61-62. [↑](#footnote-ref-96)
97. See page 4, paragraph e under Clay Production Systems in Goren et al. (2004) . [↑](#footnote-ref-97)
98. See page 4, lines 17-18 of Goren et al. (2004). [↑](#footnote-ref-98)
99. See page 4, lines 16-17 of Goren et al. (2004). [↑](#footnote-ref-99)
100. See page 4, lines 21-23 of Goren et al. (2004). [↑](#footnote-ref-100)
101. Cohen-Weinberger and Goren (2004), page 79. Col. 2, lines 5-6. [↑](#footnote-ref-101)