



BIBLICAL ARCHAEOLOGY SOCIETY

SPECIAL REPORT

Jerusalem Forgery Conference

by
Hershel Shanks

With Abstracts by Conference Participants:

Shmuel Ahituv	André Lemaire
Gabriel Barkay	Alan Millard
Chaim Cohen	Ronny Reich
Aaron Demsky	Amnon Rosenfeld & Howard R. Feldman
David Noel Freedman	Hershel Shanks
Edward Greenstein	Andrew Vaughn
Avi Hurwitz	Ada Yardeni
Wolfgang Krumbein	

Photo Analysis of James Ossuary Inscription

Full Text of Comments by Gabriel Barkay

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REPORT

**JERUSALEM
FORGERY
CONFERENCE**

HERSHEL SHANKS

P R E F A C E

On January 16, 17 and 18, 2007, the Biblical Archaeology Society convened a conference in Jerusalem to consider matters relating to the numerous inscriptions that have been recently alleged to be forgeries. Among these inscriptions are the James Ossuary Inscription that reads “James, son of Joseph, brother of Jesus”; the Ivory Pomegranate Inscription that reads “(Dedicated) to the Temple of [Yahwe]h, Holy to the Priests”; the Yehoash Inscription that purports to be a royal Judahite inscription describing repairs to the Temple as also described in the Bible; the recent Moabite Stone published by Shmuel Ahituv and displayed in the Israel Museum; and the so-called Moussaieff Ostraca, one a receipt for a contribution of three shekels to the Temple and the other a widow’s plea for an inheritance from her deceased husband.

In attendance were leading scholars from England, France, Germany, the United States and, of course, Israel. The invited scholars who attended some or all of the sessions were:

Shmuel Ahituv	Israel
Gabriel Barkay	Israel
Chaim Cohen	Israel
Aaron Demsky	Israel
Israel Ephal	Israel
Hanan Eshel	Israel
Edward Greenstein	Israel
Martin Heide	Germany
Avi Hurwitz	Israel
Wolfgang Krumbein	Germany
André Lemaire	France
Alan Millard	England
Bezalel Porten	Israel
Ronny Reich	Israel
Christopher Rollston	USA
Andrew Vaughn	USA
Ada Yardeni	Israel

A word about who did not attend:

Frank Moore Cross and David Noel Freedman, two old friends who grace the pinnacle of scholarship in many of the areas involved, declined the invitation to attend for reasons of health but were very supportive. Kyle McCarter was to have attended but was stranded by a snowstorm in Oklahoma and could not get back to Baltimore to catch his plane to Jerusalem. Othmar Keel, too, was supportive but was celebrating his wife’s special birthday with his family, as long planned.

Joseph Naveh, generally considered Israel's most distinguished paleographer, declined the invitation, but he and I had a wonderful hour-and-a-half discussion at his apartment about the matters to be considered by the conference.

Emile Puech, the honored paleographer of the Ecole Biblique et Archeologique Francaise in Jerusalem, was considering attending but was confined to bed with the flu during the conference.

Yuval Goren of Tel Aviv University and Avner Ayalon of the Geological Survey of Israel (GSI) did not respond to my invitation. They are the material scientists on the committees who largely determined that the Ossuary Inscription, the Ivory Pomegranate Inscription and the Yehoash Inscription are forgeries. Because of this, I invited only the two GSI material scientists who had originally authenticated the Ossuary Inscription, the Ivory Pomegranate Inscription and the Yehoash Inscription (Amnon Rosenfeld and Shimon Ilani) and one other material scientist (Wolfgang Krumbein). Many material scientists have criticized the work of Goren and Ayalon and reached different conclusions from them (people such as James Harrell of the University of Toledo, Edward J. Keall of the Royal Ontario Museum and Howard R. Feldman of the Museum of Natural History in New York). To have invited more material scientists, however, could have led to a charge that I was creating a badly imbalanced variety of views. In short, I could not find anyone else to defend the position of Goren and Ayalon.

As things turned out, Amnon Rosenfeld, now retired from the GSI, could not attend because he was in Argentina with his family. However, Rosenfeld and Feldman did prepare a special paper for this conference, which is appended hereto. It is in addition to the other items Rosenfeld has published in connection with these inscriptions. The case of Shimon Ilani is more interesting. He is still an employee of the GSI. He originally accepted the invitation, but at the last minute he was told by his employer that he should not attend.

Another interesting case concerns a younger Israeli scholar, Haggai Misgav. He, too, had accepted my invitation to attend, but later withdrew at the request of the prosecutor in the ongoing forgery trial in which some of the items being discussed at the conference are alleged to be forgeries. The reason for this request from the prosecutor is that Misgav was scheduled to be a witness for the government. This is especially interesting because, as discussed in more detail below, the government had no objection to Chris Rollston's attending the conference even though he, too, was to be a witness for the government. The difference is that the Biblical Archaeology Society was paying Rollston's airfare to come to Jerusalem to attend the conference. If he decided not to attend our conference, the government would have to pay his expenses in coming to Jerusalem if it wanted to offer him as a witness. In these circumstances, the prosecutor decided it was OK for Rollston to attend the conference.

INTRODUCTION

In addition to considering whether a particular inscription was likely to be a forgery or authentic, the participants at the conference also considered more general questions, such as whether unprovenanced inscriptions should be published, the biases of scholars, the development of protocols for identifying forgeries, etc.

My understanding of the overall judgment of the conference participants regarding particular inscriptions that were considered is as follows (in the discussion below I will

also include the opinions of participants who disagree with my understanding, my interpretations and my judgments)

1. The Moabite Stone is authentic.
2. The James Ossuary Inscription is very probably authentic.
3. The Ivory Pomegranate Inscription is quite probably authentic.
4. The Yehoash Inscription produced a deep division of opinion. Some said it was a forgery. Others said they could not make this judgment. Some material scientists suggested it was authentic.
5. The Moussaieff Ostraca (the Three-Shekel Ostrakon and the Widow's Plea Ostrakon) are probably forgeries.
6. The Moussaieff Ostrakon presented by Martin Heide is very probably authentic, although most of the participants saw this ostrakon for the first time at the conference.

When I first issued the invitations to attend the conference, I noted that this would be a private conference, as it indeed was, and "off the record" so that participants would feel free to express themselves. As the discussion at the conference developed, it seemed that everyone felt that some report should be issued. We discussed the report that I would write. Although I would write it (based on a recorded tape of the conference as well as written submissions), the participants stated that they wanted to review my quotations (and statements of their positions) to assure that any quotations (or summations) were balanced and accurate. I, of course, agreed. I also said that I would not include quotations that, during the discussion, they indicated were private and not for quotation or public disclosure.

After the conference I received an email from one of the participants, Christopher Rollston, stating that, in light of my initial letter that the conference would be "off the record," he did not want what he said at the conference to be cited in my report: "I do not want to see my name appear in BAR for any reason (i.e., nothing cited from the conference recordings and nothing cited from the abstract [that he submitted])."

I immediately replied that I would of course comply with Chris's quite proper request and asked whether he would prefer not to be publicly listed as one of the participants. He replied, "I am okay with you mentioning my name as being a participant in the conference. In fact, if you don't mention it, people might accuse you of blocking me from the conference and in my opinion that wouldn't be fair to you...and I don't want that to happen."

I go into this for two reasons: First, to explain that in the report below, when I state something about the participants—that they agreed or disagreed with a certain view, for example—this judgment of mine *excludes* consideration of Chris Rollston's expressed view at the conference. Thus, I may say that all agreed to a judgment that Chris disagreed with—and vice versa.

Second, I do quote some of Chris's views below. But these quotations are based on his public record, either as published in his articles or, more particularly, as he testified in court. In connection with the forgery trial that is now ongoing in Israel (for nearly two years), the government prosecutor contacted Chris and asked him if he intended to be in Israel at any time in the near future. (Chris testified to all this in court.) He replied that he would be attending our conference. The prosecutor then asked him if he would agree to testify for the government. Chris contacted me and asked whether I had any objection. I

replied that the decision was entirely up to him. So he agreed to testify and did so on January 23, 2007.

I attended that session, which was open to the public. I have advised Chris that I would feel free to refer to his testimony in my report on the conference. Incidentally, the court session at which he testified opened promptly at 9:00 a.m. and did not finish until nearly 11 o'clock that night, when he was released.

I. THE JAMES OSSUARY INSCRIPTION

Ada Yardeni, whose distinguished reputation as a paleographer extends far beyond Israel, shared with other participants her analysis of the James Ossuary Inscription. She is of the view that it is clearly authentic. Her views are set forth in the attached abstract. She there explains that her judgment is based on both paleography and circumstantial evidence regarding the ossuary and its owner, Oded Golan. Her drawing of the inscription is appended to her abstract. In summary, as she stated at the conference:

"I am sure that it is no fake, unless Oded [Golan] comes and tells me he did it. So he's a genius. But I don't believe it."

Bezalel (Buzzy) Porten of Hebrew University agreed with Ada's analysis.

André Lemaire, France's leading Semitic paleographer, has long studied the James Ossuary Inscription and continues to be of the view that it is authentic. His views are contained in his publications concerning the inscription,ⁱ as well as in his abstract attached hereto.

Gabriel Barkay spoke of his respect for Lemaire's judgment as well as his own examination of the inscription:

"It is true that one has to suspect everything. But still my assumption *a priori* is that if André Lemaire, a very sharp-eyed and knowledgeable scholar, has some observations about the Ossuary Inscription, I accept it because of his knowledge, his expertise and his honesty. But still I'm going to check the object myself. I went to see the ossuary. I went to touch it myself. I went to the Rockefeller Museum. My impression is that the inscription is genuine. And my feeling is also that of a very well-known expert in Jewish script, Ada Yardeni."

Joseph Naveh has expressed no view as to the authenticity of the James Ossuary Inscription on the occasions when I have discussed it with him. He says he has not seen it. I hope I may be forgiven for inferring from this that he may well suspect it is authentic. I say this for a number of reasons: First, he is not usually reticent about these things; he is known for (and his publications demonstrate) his general skepticism. For example, he has expressed doubts about 49 inscriptions in Nahman Avigad and Benjamin Sass's *Corpus of West Semitic Seals*.ⁱⁱ Indeed, participants at the conference praised Naveh for his willingness, as a senior scholar, to express publicly his doubts concerning the authenticity of unprovenanced inscriptions, thus serving as a caution to less experienced scholars who might otherwise be misled.

This silence speaks especially loudly because Professor Naveh was named as a special consultant to the Israel Antiquities Authority committee that judged the James Ossuary Inscription to be a forgery. The committee report identified Naveh as "an authority on ancient Hebrew writing of international repute" who was available to the committee. Apparently the committee chose not to consult him.

That Emile Puech also believes the inscription is authentic seems clear, as will be discussed below, from his belief that both parts of the inscription are by one hand (and his several publications on the inscription arguing that it does not refer to Jesus of Nazareth, but without suggesting even the possibility that it may be a forgery).ⁱⁱⁱ

Frank Cross originally expressed the view that the inscription is authentic, but later changed his mind. He still finds no problem with the inscription paleographically, as he reaffirmed in a conversation with me since the conference, but what, in his words, put him “on the fence” is the fact that the rosettes on the back of the ossuary have badly weathered while the inscription on the other side has not and the inscription is still clear. Further reference is made to this issue below.

No one at the conference claimed that the inscription raises any paleographic problem. The only one who has ever raised a paleographic problem with the inscription is an independent scholar named Rochelle Altman, whom nobody in the field of Semitic epigraphy had ever heard of. She arrived at her conclusion by examining a photograph and failed to determine correctly whether the inscription was incised or excised. *The New Yorker* magazine (April 12, 2004) published an article on this inscription; in its effort to find a paleographer who would support the contention that it was a forgery; the magazine was successful in locating only one so-called paleographer: Rochelle Altman. This led to a great deal of short-lived notoriety for Dr. Altman. When her book on what she calls “writing systems” was published, it was reviewed in *Maarav*.^{iv} It was a damning review (it accused her of making a “bizarre assertion,” proceeding “by free association” and of using words “like no one before her”). She has not been heard from since.

In short, no one at the conference or otherwise has raised any question about the paleography of the inscription.

But this does not prove that the inscription is authentic. There is no way to prove that any inscription is authentic and not a forgery: The forgery may be perfect. Or the scholars and scientists have not yet found the test that will unmask the forger. This is true even of a professionally excavated artifact. We can never prove that it has not been forged and then “salted” in the excavation. (A recent artifact supposedly excavated at Ein Gedi was presented by a worker as a find, but it was actually an easily recognizable forgery.)

Therefore, we are always talking about probabilities. All the factors that we discussed at the conference increase or decrease the probability that an artifact or an inscription is authentic.

It was in this context that a number of other factors were raised at the conference regarding the James Ossuary Inscription, as well as other inscriptions.

Ronny Reich of Haifa University and a leading archaeologist specializing in the archaeology of Jerusalem observed that if this inscription had read “James, son of Jesus, brother of Joseph” instead of “James, son of Joseph, brother of Jesus,” no one would ever have raised a question about the authenticity of inscription, just as no one has raised any question about the authenticity of the hundreds of other unprovenanced ossuary inscriptions. Everyone agreed with this. That the inscription may refer to the New Testament Jesus, however, makes it more suspicious.

This raised the question of the validity of the “Too Good To Be True” test.^v Is this inscription just too good to be true?

Andrew Vaughn, who is credited with articulating the test in recent literature, explained that it was not originally enunciated with respect to forgeries, but with respect to reconstructions of incomplete inscriptions. It simply means, in either event, as he explained, that “We should be skeptical. This skepticism does not mean that an unknown inscription is a forgery. But it means that we should be cautious in just the same way one is cautious in making a restoration of an [incomplete] inscription.” In short, “When one has an inscription like this, it warrants a greater level of suspicion than when one doesn’t have an inscription like that.”

André Lemaire responded: “We have to be careful, of course. We have to be skeptical. But at the same time we must be precise. We have to present arguments [not just suspicions]. If there is no argument [with reasons] to detract from the authenticity, how are we helped [by the increased suspicion or the “Too Good To Be True” maxim]? You need to be suspicious when you check the inscription, but when you check it—and very carefully—and there is no serious argument, [the suspicion should evaporate]. Suspicion is usual at the beginning, but you have to do your work, not just stand there and say, ‘It is suspicious. It is suspicious.’”

In the end, Lemaire’s arguments seemed to reject the idea that a hunch, even of an expert, is meaningful unless it is backed by reasons. Later in the discussion Vaughn explained that he had originally adopted the phrase only to be cute and that he feels we should come up with a better motto. He said he has been “trying not to use it for the past year.”

Too many have come to this James Ossuary Inscription, said Lemaire, with what he called “*a priori*” conclusions. We must study it and analyze it, he said, and base our conclusion on this study and analysis, without having started out *a priori* with the conclusion that it is suspicious or too good to be true.

Another subject of discussion was whether the James Ossuary Inscription was by one or two hands. Was the first part (“James son of Joseph”) by a different hand than the second part (“brother of Jesus”)? This is the theory of the forgery indictment, which alleges that only the second part was added by the forger and that the first part is authentic.

This position has been firmly rejected by Ada Yardeni, André Lemaire and Emile Puech. In a post-conference telephone conversation, Frank Cross discussed whether the first and second part of the inscription are by different hands. He said the idea was “absurd.” As Yardeni states in her abstract: “As for the claim that the two parts of the inscription were made by two different hands, I insist on my opinion that this is not the case. If there are differences between the forms of certain letters, this is a natural phenomenon in hand-written inscriptions, as can be seen in numerous ossuaries.” Here she cites ten examples from the Rahmani catalog^{vi}. I will not repeat the remainder of the extensive discussion in her abstract, which I find convincing.

In the past, Kyle McCarter has taken the position that the two parts of the inscription may be by different hands. However, he neither denies nor asserts that either part is a forgery, although he does not exclude that possibility either.

Andy Vaughn, too, felt the two parts might be by different hands. Aside from this, no one else at the conference argued for the possibility that the inscription was by two different hands.

Andy Vaughn also suggested that the depth of the incisions of the inscription appeared to be different in the first part and the second part. Ada Yardeni rejects this view,

suggesting that the claim is the result of “bias,” i.e., by someone who is biased against *any* unprovenanced inscription and is looking for some reason to question it. Quoting from Yardeni’s abstract:

“The claim that the depth of the engraving differs from the first to the second half of the inscription seems to me mistaken if not biased. I would like to know exactly how the depth was measured and the exact difference in depth. In such a small script, I doubt if a significant difference in the depth can be observed.”

Another similar issue regarded differences in kerning, which was defined as little tics on some letters. Vaughn believed there were differences in kerning between the two parts of the inscription. He called attention to a *yod* in the second half that has a little tic that is not in the *yod* in the first half, adding, however, that he does “not think this was a problem, that there were two different hands.”

Gaby Barkay commented about the conditions under which ossuaries were engraved, suggesting that looking for these minute deviations in an inscription was not meaningful:

“[These inscriptions] were written in the dark, sometimes inside a cave. They were written by family members, not professional scribes. They were written haphazardly and not pre-planned. They were not measuring the characters, and they were not measuring the tics or measuring the different kerning of the characters. They did it as it came. One has to think about the circumstances under which these inscriptions were written. They are meant to identify the bones within the ossuary, and they are not meant to be an exercise for future paleographers.” [laughter]

Bezalel Porten said he was coming to this issue “from the vantage point of having worked with some 1,800 fourth-century B.C.E. Idumean ostraca [all unprovenanced but unquestionably authentic, like the Dead Sea Scrolls and the Samaria Papyri from the Wadi Daliyeh, which everyone at the conference conceded were authentic]. In a single text you can find a letter made in different ways.” Porten pointed out that the *yod* in James (*Yaakov*) was different from the *yod* in Joseph (*Yosef*); both are in the first part of the inscription. He also drew our attention to the same phenomenon in the latter part of the inscription. Conclusion: The fact that the so-called kerning is different in the first and last part of the inscription “doesn’t mean anything.”

Martin Heide remarked: “If we are going to take the kerning issue seriously, we ought to make the division after *bar* [and before *Yosef*] because the *yod* in *Yosef* already has no kerning,”

Wolfgang Krumbein explained that he had 17 little black personal notebooks and that the handwriting changed with his mood, where he was sitting, etc.^{vii} On ossuaries the inscriptions, especially the depth of the letters, could vary depending on the relative hardness at that point in the stone. As to kerning: “We should avoid the kerning issue. It is not a good criterion.”

Alan Millard commented that even if the two parts are by different hands, this means nothing:

“Gaby Barkay described how these ossuaries were inscribed by lamplight in tombs, perhaps with a nail. The depth of the lettering is not of great significance. If we want to make a difference between ‘James son of Joseph’ and ‘Brother of Jesus,’ it could be a matter of five minutes: Somebody wrote ‘James son of

Joseph,' and one of the other members of the family said, 'You should add brother of Jesus because a Jesus is in the next ossuary.'"

At one time, it was urged that the latter half of the inscription was a forgery because the first part was in lapidary script and the second was in cursive script. This argument was not considered seriously at the conference because André Lemaire had already demolished the argument in a BAR article^{viii} that has apparently been accepted by all mainstream scholars. The mixture of formal letters and cursive letters is found in the first part as well as the second part of the inscription. As Lemaire stated in his abstract: "This means that we have a mixture of formal and cursive shapes, a well-known phenomenon in ossuary inscriptions."

Ada Yardeni also addressed the charge that the alleged forger copied the *akhui* (brother) from its appearance in the only other ossuary on which it appears (No. 570 in the Rahmani catalog): "It is quite clear that the scribe of the so-called James ossuary has not copied the letters from any other ossuary inscription." She then specifically cites No. 570 in Rahmani's catalog.

In all of the quotations from Yardeni's abstract, I have omitted the extended reasoning which the reader should consult. Similarly, Lemaire's abstract deals with a number of issues I have not mentioned here.

We turn now from the humanistic sciences to the so-called hard sciences, the material sciences. After that, we will consider what I call the human factors.

The relative importance of evidence from the hard sciences as opposed to evidence from the humanistic sciences was addressed by Gaby Barkay. In his opinion, "Inscriptions are cultural [products]. The final word should be with the epigraphers, the humanities, not the sciences." I saw Wolf Krumbein shaking his head in agreement. This surprised me. But Krumbein, the material scientist, confirmed that indeed he agreed with Barkay.

On that issue, Yuval Goren, a petrologist from Tel Aviv University, whose analysis led to the IAA's condemnation of the James Ossuary Inscription as a forgery, had testified at the forgery trial just days before our conference. His testimony surprised almost everyone. On cross-examination, Goren seemed to admit that he saw some *original ancient* patina in the *ayin* of Yeshua and perhaps elsewhere in the inscription. This is especially significant because the indictment charges that only the second part of the inscription is a modern forgery. Everyone agrees that the ossuary itself is authentic and ancient, so obviously, if there is original ancient patina in the letters, the inscription (or at least those letters) must be authentic. Unfortunately Goren did not attend the conference, so it was impossible to discuss this recently-made admission with him.

Wolf Krumbein gave us a short course in geology, stone patina, biopitting and other such things that I am incapable of fully explaining. Attached hereto is his abstract. In addition, his lengthy report has been published on the BAR website ("External Expert Opinion on Three Stone Items").^{ix}

At the outset, it must be noted that Krumbein has been retained by the defense in the forgery trial. Because of this, the IAA gave him full access to the ossuary even after the case had been filed. No one else has had this advantage. He also had available to him many photographs of the ossuary and the inscription taken before the ossuary went to Toronto for a museum exhibit; other photographs taken in Toronto; still other

photographs taken on its return; and photographs taken after the ossuary had been confiscated by the police.

As is well-known, he found that the ossuary and its inscription had been contaminated after it had been confiscated by the police. Comparing photographs that were taken in Toronto with photographs taken after the ossuary had been confiscated, he found granulation on the ossuary in the earlier photographs, but not on the photographs taken after the police confiscated the ossuary. “After custody, I was the next person to study it,” Krumbein stated. “These grains were completely [gone]. 100 percent not anymore existing ... Grains are coming and going during the period that the ossuary is under custody ... [In the new photographs] there is a lot of red material ... You see this reddish material [showing us a picture on the screen] ... There is a lot of red material ... Again you see this reddish material that was never there before 2005.”

Krumbein questioned the custodians of the ossuary in the Rockefeller Museum in Jerusalem about the reddish contamination. They said it was “done by the police” when it was in their custody.

Krumbein concluded that “Somebody has deliberately added something and deliberately taken away something from it.”

On the positive side, Krumbein was able to say that the ossuary has been out of a cave environment for at least 150 years. The ossuary had been covered with a kind of biochemical patination that takes that long to develop and could not have developed in a cave atmosphere. The evidence that demonstrates this is observable on the surface of the ossuary and in the inscription. It may have been sitting in damp soil, which would account for the biopitting near the bottom of the ossuary. “I cannot imagine that the ossuary came directly out of the cave to the [antiquities] dealer and from the dealer to [Oded] Golan.”

Shmuel Ahituv asked Krumbein if he examined the other side of the ossuary—the side with the weathered rosettes, the basis by which Frank Cross concluded that he was “on the fence” with respect to the authenticity of the ossuary inscription. Krumbein replied that he did indeed examine the side of the ossuary with the rosettes. “One explanation [for the differential weathering] could be that it was standing next to a wall somewhere and the other side was exposed to different atmospheric conditions. The other option is that it was standing in a garden under different conditions—one on the rain side and the other on the sun side.”

Perhaps as important, the rosettes were drawn with the turn of a pointed stylus and the inscription was made with a metal chisel.

Continued Krumbein: “The ossuary has been cleaned and scraped all over, crisscross, crisscross. They go right through the inscription. They must have used a steel brush. They could have used a cleanser.” He observed a cement-like coating, which could be “the remains of cleanser.” Speaking from the viewpoint of the person doing the cleaning: “If I clean an object and I realize there is writing on the object, then I will clean more seriously in the place of the writing.”

Where the ossuary sat for the last 150 years could also explain the flawed results of the IAA examination based on oxygen isotopes. This experiment ultimately depended on a finding that water temperature from which a coating on the ossuary was formed was made with water hotter than is commonly found in nature in the Jerusalem area. (Goren called this coating the “James bond.”) Goren admitted in his report to the IAA committee that this covering could be the result not only of a fake patina made with hot water, but

also the residue from cleaning with a cleanser. But, according to Krumbein, if the ossuary sat in a garden or a balcony of a home for 150 years, exposed to the sun, this covering could well have been formed naturally from hotter water than normally flows in the area and resulted in the oxygen isotope ratios Goren relied on to conclude that the inscription was a forgery.

The reason for Goren's failure to analyze the limestone ossuary and its inscription properly may be because Goren was operating outside his specialty. He is a *clay* petrologist,^x not a stone petrologist. He understands ceramic petrology, but not stone petrology.¹

From the material sciences, we now turn to the human factor. New circumstantial evidence was presented to the participants in the form of a forensic study of a photograph owned by Oded Golan (both Krumbein and I had obtained copies of the report). The report on the photograph was written by Gerald B. Richards, an American expert who formerly served as the chief of the Special Photographic Unit of the FBI in Washington, D.C. (his findings appear as an appendix to this report). Richards was retained by Oded Golan to study the photograph. The subjects of the picture are some ordinary looking ancient artifacts on a shelf. But the photographer took the photograph vertically instead of horizontally; as a result, the photograph includes parts of the shelving above and below the shelf that is the subject of the photograph.

Richards examined the paper on which the photograph was printed. It was Kodak paper stamped "EXP[osed] 3/[19]76. He found that this kind of paper was not manufactured by Kodak after the late 1970s/early 1980s.

On the shelf below the subject of the photograph there appears part of an ossuary with an inscription of which only the last part is captured in the photograph. It is clearly the end of the James Ossuary Inscription. If the picture was taken in March 1976, the inscription was already on the ossuary at that time.

On the shelf above the subjects of the photograph are several books. The FBI analyst (he still occasionally works for the FBI) was able to enlarge the photograph and identify the books. One is a 1974 Tel Aviv telephone book. Another contains marks indicating it has been withdrawn from the Technion library—where Golan says he was studying at that time.

Another item on this shelf is a small photograph that Golan claims is a picture of his then-girlfriend, who, now more than thirty years later, is, he says, prepared to identify herself as the young girl clearly seen in the photograph.

If all this is true and if Golan is the forger of the inscription as alleged in the criminal indictment, Golan must have forged the inscription at least a quarter century ago and then let the ossuary lie unpublicized in his collection with no attempt to sell or exhibit it—surely unlikely behavior.

Andrew Vaughn raised another possibility, however. At the conference, I misspoke, reading "Expires March 1976" (referring to the date on the photographic paper) instead of "Exposed March 1976." Vaughn noted that from his own experience as a photographic darkroom buff, he knows that long outdated photographic paper can still be used to print pictures. There thus remains the possibility that Golan recently created this

¹ Goren has reacted to this statement by providing me with his 13-page resume that cites 102 articles and chapters in books. Based on their titles, I counted 53 citations involving clay, 2 that involved clay and stone, and 5 that involved stone (not counting the papers on the recently alleged forgeries).

photograph by obtaining the long-outdated paper and using it to print a recently-taken photograph. In this scenario, he could have obtained a 1974 telephone book, the book withdrawn from the Technion library and the picture of his old girlfriend, all of which he used as props in this doctored and forged photograph. Vaughn remains skeptical about the authenticity of the inscription.

This skepticism is intensified by evidence that the police confiscated from Golan's apartment, including a dentist's drill, dirt from various archaeological sites, drawings of seals that were supposedly to be made by Golan and half-made seals, all suggesting that he is a forger.

On the other hand, even if he is a forger, it does not mean his entire collection of the thousands of pieces are all forgeries. As one participant stated, "Oded Golan may be a crook, but he's not an Einstein, not a genius."

Other human factors are reiterated in the abstracts of Ada Yardeni and André Lemaire. Yardeni notes that even she did not immediately recognize the possible significance of the inscription because of the appearance of these names and their relationships in this order in the New Testament. ("It never occurred to me that this inscription referred to the brother of Jesus.") This tends to buttress Golan's claim that he, too, failed to understand the possible significance of the names, but learned this only from Lemaire. Golan says that he did not even know that Jesus, the Son of God, could have a brother. Moreover, an Israeli would have difficulty associating *Yaakov* on the ossuary with "James" in English, especially since the same Hebrew name is translated as Jacob in English versions of the Hebrew Bible.

II. THE IVORY POMEGRANATE INSCRIPTION

Fortunately, the two paleographers on the IAA and Israel Museum committee that condemned the Ivory Pomegranate Inscription as a forgery were both in attendance at the conference. By agreement with Shmuel Ahituv, the case for the forgery was presented by Aaron Demsky.

Professor Demsky said that the decision concluding that the Ivory Pomegranate Inscription was a forgery was driven by four factors^{xi}:

- (1) The artifact was unprovenanced. It came from the antiquities market. "The onus of proof is on the inscription."
- (2) The pomegranate itself (as opposed to the inscription) is very probably from the Late Bronze Age. The paleography of the inscription, however, indicates that it was Iron Age II (late eighth century/early seventh century B.C.E.), about 400 years after the pomegranate itself was carved from a hippopotamus canine tooth. "It's a little hard to believe that a minute object lies around for three or four hundred years and is then dedicated to the Temple ... It's possible, but not too probable."
- (3) A third of the grenade of the pomegranate is broken off. All agree that it was broken off in two stages. Part was broken off in antiquity, and some was broken off in the modern period. If the inscription is authentic, it must have been written before the ancient break, because it would be very peculiar to dedicate a broken pomegranate to the Temple. Obviously, in the eighth century, the owners of the pomegranate (perhaps the Temple authorities) would not have used a broken pomegranate to be inscribed with a dedication to the Temple. [Besides, the

inscription is incomplete and could have been complete only before the ancient break.—HS]

The modern break (or breaks), on the other hand, probably occurred as a result of the forger's going into the old break and thereby creating a new break. Viewing the inscription through a high powered microscope, it appeared that in several letters, the engraver stopped short of the edge of the old break. Had he continued to complete the letter, he would have gone into the break. He was afraid of breaking off more by going into the old break. If this is the case, the inscription is clearly a forgery. (Everyone agreed that the failure to go into the old break would be a clear sign of forgery—if the natural shape of the letter would have extended into the break.)

- (4) Finally, one must note the use of spaces as word dividers in this inscription, which is a relatively late development. In ancient northwest Semitic alphabetic inscriptions there were three ways that words were written in a sentence. Depending on the time and the national script, three different methods developed: (1) the earliest method was inserting a dot or slash as a word divider. This was the usual practice among Hebrew scribes in the Iron Age; (2) without spaces or dividing marks between the words, called *scriptio continua*, a method practiced by Phoenician scribes; and (3) leaving a space between the words, as we do today. Spaces used to divide words from each other were seemingly introduced by Aramaic scribes in the employ of the Neo-Assyrian Empire in the seventh century B.C.E., and was adopted by Hebrew scribes at a much later period, perhaps in Second Temple times. The Ivory Pomegranate inscription, written in the Hebrew script supposedly in the late eighth century B.C.E. by an Israelite scribe contradicts this understanding of the phenomenon. The use of spaces for word dividers in this Hebrew inscription at this time “seemed to Shmuel [Ahituv] and me to be the smoking gun. The modern forger didn't realize that there was that type of distinction at this period of early Hebrew inscription” and that spaces for word dividers were not yet used in the Hebrew national script.

Demsky noted that the *cumulative* effect of all these factors led both him and Ahituv to conclude that the inscription was a forgery.

Demsky did not mention paleographic considerations as a basis for concluding that the inscription is a forgery. On the contrary, he notes that “Shmuel [Ahituv] and I were brought in to look at the letters and we were pretty well satisfied. After all, two great paleographers, Nahman Avigad and André Lemaire, had looked at it [the inscription] and pronounced it good.” The inscription “is perfectly fine from a paleographical point of view.”

In his abstract (appended hereto), Demsky recognizes that he has not made a 100 percent case: “The truth is that I can never be sure.”

André Lemaire believes the inscription is authentic and rose to its defense. With regard to the first issue, Demsky takes a cautious position. According to Demsky, the burden of proof in the case of any unprovenanced inscription is on the one who asserts its authenticity. In Rollston's words, “Epigraphists must operate with the *a priori* assumption that there are currently individuals with the ability, motivation, materials and economic resources to produce good epigraphic forgeries, that is, forgeries that ‘pass all the tests’ (or at least pass them to the satisfaction of many).”^{xii} In short, “Superb forgeries will be difficult to detect in all cases.”^{xiii}

Lemaire's view is different. He, too, starts out with suspicion, but the final determination, he asserts, must be made on the basis of facts and reasons, not on the simple basis that it is unprovenanced—or because it is important and dramatic and therefore especially suspicious. For Demsky, however, the fact that it is unprovenanced remains part of the decision-making process.

As to Demsky's second issue: The 400-year difference between the date of the pomegranate and the date of the inscription. The pomegranate was created in the Late Bronze Age and not inscribed until Iron Age II. Lemaire believes this could well have occurred: "I admit that it is not very usual, but we have very good parallels." He cited a treasury of ivory from the Late Bronze Age that was found in an Iron Age II temple at Tel Miqne/Ekron. He also cited other parallels. "I admit it is unusual, but it is not a problem." Demsky asks: But were they inscribed with dedicatory inscriptions in the latter period?

As to the third issue raised by Demsky, Lemaire agrees that "if the stroke [of a letter] does not go as it should because the engraver is afraid to go to the edge, then it is a forgery. I agree completely with this principle, which was already known to Professor Avigad." However, Lemaire examined the Pomegranate Inscription under a Stereoscopic Electron Microscope and found this *not* to be the case. The failure to go to the edge of the old break was apparently the argument of Professor Yuval Goren, the critical member of the committee in this regard. Goren's observation swayed the others, including Demsky and Ahituv. But Goren is not an epigrapher. Lemaire suggested that Goren does not know whether the letter *should* continue to the ancient break. Upon Lemaire's examination of the inscription, he found no instance in which a letter *should* have extended into the break, but did not. Lemaire had just published an article to this effect in the most recent issue of the *Israel Exploration Journal*.^{xiv} "For me," said Lemaire, "it is very clear, but I invite you to check it by yourself." Speaking of the *yod*, which Goren says exposes the forger by his failure to go into the break, Lemaire stated at the conference: "I agree that this stroke is not going as far as the old break. It stopped before. There is a very good reason for that. If you know paleography, when you have a *yod*, you need to turn it until you have the very beginning of the vertical stroke. When you look at it very carefully, it's clear enough." In short, the *yod* did not go into the old break because the top of the *yod* properly curved at this point.

"If you don't know anything about epigraphy, about letters, you cannot interpret properly [why a letter stopped short of the ancient break] ... I think Professor Goren has to think a bit more, especially when he is going against two people who have worked with the same inscription but who know a little bit about epigraphy. He doesn't know anything about epigraphy, so he doesn't know how to interpret what he's seeing ... In this case, why is he working if it's not in his field? This raises a moral question."

Professor Lemaire said he has tried to discuss the matter with Professor Goren, but Goren would not discuss it with him.

As the conversation developed, it became clear that the crucial question related to whether some letter or letters in the inscription stopped just short of the old break (betraying the forger's fear of going into the old break) or whether, on the other hand, there were paleographical reasons why the letter or letters stopped short of the old break.

But there was more: Did one or more letters actually go into the break, in which case that would be very strong evidence that the inscription was engraved before the ancient break? If a letter or letters went into the break and the inscription was engraved before

the ancient break, then the inscription must be authentic. Lemaire felt sure he had seen that in the case of two letters. The discussion was a very friendly and respectful one.

Finally, Lemaire addressed the problem of the spacing between words. He argued that one of the spaces was due to the fact that it was a two-part inscription, like the two-line inscriptions on seals and bullae. He had also measured the spaces between letters and found that this did not support the contention that spaces separated the words. On the contrary, the space between the words varied from between 1.25 mm and 0 mm.

Though not convinced by Lemaire's argument, Shmuel Ahituv, speaking for himself and his colleague Aaron Demsky, said that he hoped Yuval Goren would "go back to look again" in light of what Lemaire had said. As a matter of fact, Goren, Demsky and Ahituv have already planned to recheck the pomegranate. As for Lemaire's measuring the exact distance between the letters, Ahituv remarked that due to the different shape of the various letters and the relation between them, not the actual distance counts but the optical illusion.

After the conference, I wrote to Israel Museum director James Snyder to arrange a date when Yuval Goren, André Lemaire, Aaron Demsky and Shmuel Ahituv (and others) could look at the pomegranate under a Stereoscopic Electron Microscope and have professional pictures taken of what can be seen. I said I would come back to Jerusalem for the occasion. At least theoretically, we should be able to observe quite clearly one of two possibilities: (1) a letter (or letters) improperly stops before the ancient break; or (2) there should be a v-shaped groove observable from the side where the inscription goes into the ancient break. The result of this re-examination should be well-nigh conclusive. At this writing, I am working with the head archaeology curator, Michael Dayagi-Mendels, to fix a mutually agreeable date. Both Demsky and Ahituv said they would approach the new investigation of the pomegranate "with an open mind."

Amid some laughter, Shmuel Ahituv promised to talk to the editor of the *Israel Exploration Journal* about publishing the results. (He is the editor of the journal.)

III. THE YEHOASH INSCRIPTION

The discussion of the Yehoash Inscription was especially interesting not simply because of the diametrically opposing views, but also because of the methodological issues these differences exposed.

The discussion regarding the Yehoash Inscription initially focused on the competing philological evaluations of the text by Edward Greenstein of Tel Aviv University (with whom many philologists, including Avigdor Horowitz of Ben Gurion University of the Negev, Frank Cross, Kyle McCarter and Israel Ephal agree) and Chaim Cohen, also of Ben Gurion University of the Negev. Greenstein and Cohen have previously debated these issues and know each other's arguments. And each conceded the philological expertise of the other. Ed Greenstein has issued several statements quoted by the Israeli and American press on the subject, and published one article in *BAR*.^{xv} Historian Israel Ephal was the first to write in a scientific publication (*IEJ*) that the inscription is a forgery. His position is based on linguistic and textual arguments. Chaim Cohen has also written on the subject and submitted a 70-page paper defending his position at the conference. This will be published in as "Biblical Hebrew Philology in the Light of Research on the New Yeho'ash Royal Building Inscription," in Meir Lubetski, ed., *New Seals and Inscriptions* (Sheffield: Sheffield Phoenix Press, 2007), which is currently in press.

The discussion at the conference, however, was particularly valuable because it exposed not so much philological differences as methodological differences.

This inscription purports to be a royal Israelite (actually Judahite) inscription from the ninth century B.C.E. Ed Greenstein finds a host of philological errors, some of which he calls howlers. He finds no justification for these forms in ninth-century Hebrew. Chaim Cohen agrees that these forms are unattested in the inscriptions we have from the Bible or from other Hebrew texts and inscriptions from the same period. This is an enormous area of agreement between the two contending scholars.

Cohen, however, looks for comparative material to see if the forms could have been used in early Hebrew, even though we do not (yet) have their attestation. He looks at Hebrew from other periods or a parallel development elsewhere in Hebrew. He considers possible parallels from Mesopotamia, especially Akkadian, royal building inscriptions (including those dealing with temple renovations like the Yeho'ash Inscription). Moreover, Cohen does not contend that the inscription is authentic. He says only that it might be, leaving open the possibility that these unattested philological usages may later turn up.

Each scholar uses a different methodology, which determines whether the answer is "forgery" or "possibly authentic." Herein lies the methodological battlefield.

First of all, Greenstein proceeds from the fact that the Yehoash inscription is unprovenanced. In his view, this makes the inscription suspicious (although he does not use this as an assertion in support of its being a fake); in other words, the unprovenanced nature of the inscription guides him as a philologist in choosing one of two general methods of approach to its interpretation. "I begin," he tells us, "with a text that's suspect. It simply appears on the market."

This issue seems to arise again and again in the scholarly discussions. For some (Demsky, Vaughn), that the inscription is unprovenanced is an independent and continuing factor to be weighed on the scales when deciding whether the inscription is or is not a forgery. For others (Lemaire, Cohen), this initial suspicion stemming from the unprovenanced nature of the inscription calls for a particularly intensive examination. But once this examination is undertaken, the unprovenanced nature of the inscription is no longer a factor to be considered in deciding whether or not the inscription is a forgery. According to these scholars, decision must be based on what we find in this intense examination. As Cohen says in his abstract: "Labeling [the Yehoash Inscription] in advance as a forgery and then proceeding to demonstrate how the assumed forger constructed his masterpiece by pointing out all the similarities is surely begging the question."

Greenstein finds a host of philological problems. Because the text is suspect, he measures those problems against "standards of what already is known," not against theoretical possibilities based on distant parallels. Comparative material must be used "guardedly." "In this case, we have to measure the suspect text against standards of what is already known from the literature, the linguistic phenomena that are supposed to characterize this text." This inscription purports to be a royal inscription. We know what to expect in a royal building inscription. "You expect perhaps some deviation in all texts, [but] you don't expect the number of deviations from established Hebrew practice that we find in the Yehoash Inscription." While expressing the "utmost esteem for Chaim's philology," in the next sentence Greenstein accuses him of "perform[ing] philological summersaults in order to explain this or that deviation from what we know to be standard."^{xvi}

Greenstein discusses a number of specific examples (see his abstract). One of these involves a blessing at the end of the inscription that is an “oddball.” But it is attested in a recent find from Ekron, although superficially similar, he states. At first I thought this would support Cohen. But Greenstein considered this proof of forgery: “The Yehoash forger decided that he would make an inscription that is unattested anywhere in the ancient Near East, but is supported by the new inscription from Ekron.”^{xvii}

Greenstein also emphasizes the cumulative nature of his argument. Perhaps one of the problems he sees would be explainable, but not the many, many instances of problems he finds in this inscription. A theory that requires a series of *ad hoc* explanations should not be preferred over a simple explanation that explains the many oddities in this inscription. Here there is a simple explanation that accounts for all the philological problems: Someone who knows modern Hebrew forged the inscription. The forger was not an expert in Biblical Hebrew; he simply “copied from some Biblical passages and put them together.” This theory explains all the problems.

Chaim Cohen studies each of the supposed philological problems and finds parallels that not only suggest that the usage in the Yehoash Inscription may well be correct, but that also demonstrate extraordinary scholarly knowledge—if it is indeed a forgery. That is why he concludes that either the inscription is authentic or the inscription is a “brilliant, brilliant forgery.”

Cohen rejects Greenstein’s view that the unprovenanced nature of the object is a factor in concluding that it is a forgery. “As a philologist, the only difference in the way I treat an inscription without known provenience is that I will consider the possibility of a forgery.” But he will not use that as “the” or even “a” basis for his conclusion. As a philologist, Cohen says he is “neutral.”

As noted above, for Cohen, if this is a forgery, the forger must have been extremely knowledgeable and brilliant in ancient Hebrew as well as in a variety of other ancient languages. This is similar to the view expressed by Ronny Reich and Gaby Barkay. They actually broaden Cohen’s point because the alleged forger would need to be, they say, extremely knowledgeable in a number of other disciplines and crafts. (If it were a conspiracy by a number of participants, then there would have been a leak, Reich and Barkay suggest. To pursue this possibility and to perhaps create a leak among the conspirators, BAR has offered a reward of \$50,000 for information leading to the arrest and conviction of the forger(s) of the Yehoash Inscription.^{xviii})

Greenstein disagrees that the forger would need to be knowledgeable and brilliant: “This [forger] is not someone who knows Biblical Hebrew. This is not someone with a higher degree in Biblical Hebrew. This is someone who knew how to copy material from lots of different places, who will potentially make millions of dollars by writing this inscription. For this reason it was worth looking [for the new inscription from Ekron] or to re-construct [something] from the second century B.C.E. book of Ben Sira ... [This forgery] is not brilliant, it’s bad. He makes mistake after mistake.”

“Do you have any doubt, Ed?” I asked him. After all, some physical scientists have authenticated it.

Greenstein: “If there was clear evidence that this inscription was authenticated by the consensus of physical scientists, I might take Chaim’s approach and say, ‘Let’s see how we should understand this inscription,’ although I still would say it’s not very good to start a series with seven objects with one that doesn’t work.”^{xix}

Paleographically, there was somewhat of a consensus concerning the Yehoash Inscription. All agreed there were paleographic problems with it. The difference arose from the different conclusions scholars have drawn from this fact. Some (Frank Cross, Kyle McCarter) say that on this basis, the inscription is a forgery. Others say there are problems, but they are not ready to declare it a forgery.

Ada Yardeni and Chris Rollston (based on his courtroom testimony) may be considered emblematic of these two differing positions—one ready to declare it a clear forgery and the other unwilling to declare it a forgery. Attached to this report is Yardeni’s detailed analysis of the script. She concluded at the conference, “There is not enough epigraphical material from that period to enable a definite conclusion.” In more colloquial language: “I’m not sure.” She thinks both Ed Greenstein and Chaim Cohen made “beautiful presentations.” She sees both sides. “It’s so difficult to say.”

Rollston, on the other hand, has “no doubt,” as he testified, that it is a modern forgery. He is not at all concerned that other eminent scholars may disagree with him. He bases his conclusions on “evidence, not authority.”

Rollston presents himself as a specialist in Iron Age inscriptions only, which he defines as the period between 1200 and 500 B.C.E. “Very few people in the world” specialize in the paleography of this period, he testified. He is one of them^{xx}.

Rollston is much more sure of his conclusions than Yardeni^{xxi}. He can identify “with certainty” scripts of the eighth, seventh and sixth centuries B.C.E., clearly distinguishing them “with precision.”^{xxii} Moreover, he can distinguish Hebrew from that of its Semitic neighbors: “We have a distinctive Hebrew script; we have a distinctive Phoenician script; we have a distinctive Aramaic script.” Features of one are “not mixed” with features of another. “No paleographer would confuse an Old Hebrew inscription with Phoenician script.”

By contrast, in her abstract for the conference, Yardeni states that “in this period [of the Yehoash Inscription] it is still difficult to distinguish between Aramaic script and its ancestor, the Phoenician script.”

On cross-examination Rollston was questioned about the famous Gezer Calendar, which he identified as being written in Phoenician script, although it is often considered a Hebrew inscription of the tenth century B.C.E. Although it seemed clear to him, he admitted there was “some discussion” among scholars about the matter. He was then asked why, if the difference was so clear, there was this discussion. Apparently, not everyone was up on the latest scholarship.

Rollston operates from a “hermeneutic of suspicion.” Another way of characterizing this outlook is extreme skepticism. He told the court that if a forger makes mistakes, as he abundantly did in the Yehoash Inscription, he is unmasked as a forger. But if he does not make mistakes, it may still be a forgery, just an undetected one. As he has written, “Forgers have all the tools needed to produce a nearly impeccable forgery. Fortunately, of course, forgers often make mistakes (and these can be detected), but it is imprudent to assume that this is always the case.”^{xxiii}

That is why, Rollston explained to the court, he will not use unprovenanced finds in his work. Indeed, he announced at the outset that he will study only inscriptions that have been recovered in a professional excavation. As he has written: “Because the potential for forgery is consistently present, scholars must begin to relegate non-provenanced data to a secondary or tertiary position at the very least, and must be disinclined to base conclusions regarding history, religion, language, epigraphy, etc. upon such data.”^{xxiv}

Wolf Krumbein disagreed with such an idea: “Everyone interested in studying cultural heritage should include objects that have ‘not a clean origin.’”

Rollston’s attitude of extreme skepticism is different from many other scholars. I particularly like the articulation of this by a scholar who was unable to attend the conference, David Noel Freedman. He expressed it this way:

“Two favorite mantras of mine are ‘Be skeptical,’ which you have to be if you’re a scholar, and ‘Be especially skeptical of the skeptics,’ because skepticism is too easy a position to assume. If someone says of every new discovery, ‘It’s a fake,’ then they dismiss it and it’s over for them. They never have to change their minds or consider new ideas. The fact is, every new discovery may open a door we didn’t even know was there ... The fact that we have found something we haven’t seen before or don’t understand doesn’t necessarily indicate a forgery.”^{xxv}

Even Andrew Vaughn, who has “no doubt” that the Yehoash Inscription is a forgery, added: “Would I stake my life on it? Of course not.”

As with the Pomegranate Inscription, so with the Yehoash Inscription, there was a question about whether two letters had improperly stopped short of the break on the top and side of the inscription, thus revealing the inscription as a forgery. Ada Yardeni presented her drawing of the inscription with the two letters marked.



The two letters involved are a *heh* and a *shin*. André Lemaire reported that he checked the stone itself, and there is no question about the *heh*; it was OK. No one could speak to the *shin*. Unfortunately, the stone itself is in IAA custody and therefore unavailable to scholars. But clearly this should be examined if we can get permission from the IAA to do so.

What do the material scientists have to say about the Yehoash Inscription? Several material scientists have examined it and come away with a very different view from the many philologists and paleographers who are confident that it is a forgery. Wolfgang Krumbein is one of the material sciences. He explained how a stone like this would be prepared for engraving. The last step, to smooth the surface, involves an interaction between the object used to smooth the surface and the plaque itself that modifies the surface of the stone. It takes considerable time for the effect of this process to

disappear. So either the inscription is good or “a very, very skilled man or woman has changed the surface to look much older than freshly polished.”

Krumbein identified the surface changes in detail: blackish deposits in the sub-millimeter range that are sometimes thicker, sometimes thinner, sometimes pustular, and, finally, “at some places it is not a blackish deposit, but it is brownish or yellowish varnish ... This irregular coating goes down through the writing and comes up here again with all its irregularities ... It is incredibly difficult to imitate a naturally growing black, brownish, uneven, hilly landscape on such a rock surface. But,” he added, “let us not forget this is not from an authentic excavation, so we have to be suspicious. I agree with that.”

The stone also contains clear evidence that it has been cleaned. This complicates the analysis still further.

Krumbein also addressed the fact that small gold globules appeared on the stone. “This kind of gold particles are difficult to get. It is difficult to produce. And it is really difficult to explain how you could mix this with the other material except by fire—heavy, serious, strong fire.”

Professor Krumbein referred to a recent study demonstrating how silver was absorbed into Jerusalem pottery, but only in the wealthier part of town.^{xxvi} “The rich people had a lot of silver.” In one pot with silver coins in it, it was shown that the level of silver in the pottery was higher at the bottom part of the pot than in the upper part. In other words, the pottery absorbed minute amounts of the silver simply by proximity. This could be the source of the gold globules in the Yehoash plaque.

Naturally, one thinks of the Babylonian burning of the Temple in 586 B.C.E. and the gold it contained. Could this plaque have been hanging in the Temple when it was destroyed by the Babylonians? Or is it simply a poor and obvious forgery as many philologists and paleographers claim?

Krumbein was permitted to examine the plaque because he has been retained by the defendant Oded Golan as an expert witness. He spent two days in the Rockefeller Museum. His work was interrupted, however, because he was “accused of theft by the IAA. A policeman came and searched my bags for four hours, interrupting my work not only literally but psychologically.”

Those material scientists who have found the Yehoash Inscription authentic (Wolfgang Krumbein, Amnon Rosenfeld, Shimon Ilani, etc.) have made their views known in writing. (In addition, a special paper for this conference was prepared by Amnon Rosenfeld and Howard Feldman and is appended hereto.)

The chief counter-view, by Yuval Goren, could not be vetted at the conference because Goren was unwilling to attend.

There was some additional discussion regarding Goren’s expertise. Most basically, he had misidentified the stone on which the Yehoash Inscription was engraved. He called it greywacke and suggested it came to Israel from North Syria or Cyprus. In fact, it is common arkosic sandstone found in abundance in such places as Timna in the Negev and in the Sinai.^{xxvii} Aryeh Shimron, described at the conference as “an excellent field geologist and stone petrographer,” is reported to have said that it is easy to understand Goren’s error “because rock is not like clay and obsidian rock is a very complex material.” Krumbein was clear as to Goren’s error, however: “Yes, the rock is misidentified [by Goren].”

Krumbein also addressed Goren's oxygen isotope studies, the basis of which Goren found the inscription to be a forgery. In Goren's view, the temperature of the water to make the patina he found on the stone was too hot to be found in nature in the Jerusalem area; it must have been hot water used by the forger to mix the fake patina. Krumbein described these tests as "double and triple erroneous ... If the stone went through a fire, then naturally these data are confused."

The crack that goes (or went) vertically through three lines of the inscription (the plaque has now broken at the crack line) was also discussed at the conference. It was accepted by all that the crack was ancient. Either the inscription was there before the crack (and the inscription was therefore ancient) or the forger was so skillful that he could engrave across the crack.

I pursued the matter with Krumbein:

Shanks: You just said that when you look at a section where the letters are separated by the crack, that all the evidence is that there is no separation and the engraver could not have come to the crack and gone to the other side without leaving some evidence that he did that. Is that correct?

Krumbein: That is correct.

Shanks: And you did not find that evidence?

Krumbein: I didn't find that evidence and nobody of the GSI or IAA has talked about any kind of evidence of that ... Your statement is new to me. It's a very smart question, Hershel. Is there potential to go to a more sophisticated analysis? In this case, it would be possible to make a very large blow-up photograph with very modern microscopy to see whether there is an indication that the writing is post-crack. [If it is] that would go very much to forgery. If the writing has no signs of post-crack and is apparently pre-crack, then we have to explain. It might be that the forgery is [very ancient].

Ronny Reich commented: "If the letters went across at the crack [and were therefore post-crack], the rock would have opened, would have split [at the crack] because this is a point of weakness. The chisel would have met an open space ... I think the letters preceded the crack. Otherwise, we would have had a concentration of small splits, small flakes coming off exactly where the line of the letters hits the crack.

Ed Greenstein reported that the plaque broke at the crack "when it [the plaque] was laid down. It wasn't handled roughly. It was just laid down—and cracked. That may have been from the trauma of incising letters into the tablet where it was already cracked and the incising of the letters would have produced this."

André Lemaire: "I saw the inscription before the break [in two]. I handled it. I took it in my hands. I carried it a little bit to see it in light and so on. It was not broken." So the idea that the plaque broke as a result of the forger's engraving the letters is not an explanation.

One of the things that detract from Krumbein's testimony is that he also finds that a stone oil lamp decorated with the seven species of Succoth [the Jewish holiday of Tabernacles] is authentic, while it is widely believed by others to be a forgery. [Scientists at the GSI also believe the oil lamp is good.] As Hanan Eshel said at the conference, "I can tell you I'm positive it [the oil lamp] is a new thing that was produced recently by someone who read Varda Sussman's book on oil lamps. I am sure about this ... I don't

have any problem to go into any court and say under oath that I'm 100 percent sure that the oil lamp is a forgery." Eshel was also certain the Yehoash Inscription was a forgery: "If you compare the Yehoash Inscription to any other archaeological artifact that was found, there is nothing good in it. Nothing. It's not the stone, it's not the letters, it's not the philology, it's not the size of the letters. There is nothing good there that fits what we know about other Iron Age inscriptions. So it seems to me we're wasting our time."

Responding to Hanan Eshel, Krumbein stated: "Wasting our time or not, I'm a peaceful person. Over two days there has been constant doubt as to my expertise."

In Krumbein's judgment, the easiest of the alleged forgeries for a forger to forge would be the James Ossuary Inscription. Next would be the Yehoash Inscription. The most difficult would be the decoration on the stone oil lamp. "That is somehow contradictory to the paleographers and archaeologists," he commented. Eshel said he agreed with Krumbein that the oil lamp was the most difficult to forge. But for him, that just proves what "wonderful technology" forgers have.

Eshel also believes the Yehoash Inscription is not simply a forgery, but, in the words of Robert Deutsch, a "disgusting forgery." Eshel cited Deutsch, a leading paleographical scholar who is also a leading antiquities dealer and who is one of the two defendants remaining in the forgery trial (originally there were five defendants). "We have to give much more credit to the people who are in this business; they know exactly what to do." On the Yehoash inscription, Eshel said, "I think Deutsch is right."

This may be compared to Krumbein: "I have a very high tendency to believe it is authentic."

Alan Millard gave his assessment of the Yehoash Inscription in these words: "While the engraving is not demonstrably wrong for the ancient Near East, it seems to me strange and arouses suspicion." Millard raised several possibilities. One of course, is that it is a modern forgery. Another is that it is an ancient fraud. Or it could be an ancient revision of a more ancient text. Or it might be an accurate ancient copy of a more ancient text. "The last possibility is that the document is authentic and adds to our knowledge of the script and language of the time." His final verdict: *Dubitante* in view of the geophysical evidence. He is unable to arrive at a clear conclusion at this time. Although "I'm very much inclined to agree with Ed Greenstein," he cautioned, as have so many others, including David Noel Friedman, "We have so little material from ancient Israel to make comparisons." By contrast, "We have thousands of texts from Hammurabi's time, including quite a number of royal inscriptions; and we have many, many texts throughout the history of Babylonia, so we know the history of the language, its development, its vocabulary, its syntax, its grammar, all in considerable detail, almost century by century. This is something we cannot do for ancient Hebrew."

IV. THE MOABITE INSCRIPTION

The unprovenanced Moabite Inscription recently published by Shmuel Ahituv^{xxviii} and displayed in the Israel Museum was the subject of some discussion, but almost none of it concerned the question of its authenticity. Avi Hurwitz seemed to reflect the consensus when he stated that "my feeling in that this inscription is genuine. I have no doubts about it."

There was no discussion of why this was so, however. Why was no question raised about its authenticity? And why was such a question raised about the ossuary

inscription? Is the ossuary simply a case of “too good to be true?” All seemed to agree that if the three names had been re-ordered in the Ossuary Inscription, no question would have been raised as to the authenticity of this inscription.

But what of the Dead Sea Scrolls? They are also “too good to be true.” What should the rules be? Can they be articulated?

V. THE MOUSSAIEFF OSTRACA—The Three-Shekel Ostrakon and the Widow’s Plea Ostrakon

Hanan Eshel: “I think they are 100 percent a forgery.” There was general agreement that at the very least they were suspicious. Because of his doubts to their authenticity, André Lemaire had declined to publish them.

[Chaim Cohen’s addition: Not all scholars are convinced that the two Moussaieff Ostraca “are 100 percent a forgery.” Several well-known scholars have published their opinions concerning this issue and in at least three cases, the verdict has been approximately that the two inscriptions are probably authentic. The following is the opinion of Prof. Dennis Pardee in W. W. Hallo et al. eds., *The Context of Scripture*, Volume III (Leiden: E. J. Brill, 2002), p. 86 (immediately preceding his translation of “The Widow’s Plea” [Text 3.44]): “I continue to believe ... that the text is authentic. To believe the opposite requires the hypothesis that the forger was a master epigrapher, a master grammarian ..., a master of biblical law, and a master chemist...”. This is also the published opinion of Prof. Elisha Qimron (in his Hebrew article in *Leshonenu* 61 (1998), especially p. 185. Finally, this is also my own published opinion. See my Hebrew article in D. Sivan et al., eds., *Yaakov Bentolila Jubilee Volume* (Jerusalem: Mosad Bialik, 2003), especially pp. 239-240 and n. 4.]

VI. THE BROADER ISSUES

After considering all of these specific inscriptions, the discussion turned to the broader issues. Most insistently:

What do we do, as scholars, as an academic community, when artifacts or inscriptions surface on the antiquities market, especially because, if they are not forgeries, they were probably looted?

At one extreme are scholars like Chris Rollston, as expressed in his courtroom testimony. He will not work with unprovenanced inscriptions.

The consensus of the scholars at the conference, however, was overwhelmingly that these artifacts and inscriptions must be published.

On the other hand, to a great extent, the leading American professional associations—the American Schools of Oriental Research (ASOR) and the Archaeological Institute of America (AIA)—agree with Professor Rollston. They will not permit unprovenanced artifacts that come from the antiquities market to be published in their journals.^{xxix}

Andrew Vaughn, who is serving as executive director of ASOR, told us that ASOR has been struggling to arrive at the appropriate balance or compromise regarding this policy. Under the present ASOR policy, although an unprovenanced artifact cannot be published in its distinguished journal, *BASOR* (*Bulletin of the American Schools of*

Oriental Research), it may be discussed in BASOR once it has been published elsewhere. Vaughn recognized that there is a certain “inconsistency” here.

Vaughn recounted his own conflict in doing the research on ancient Israelite seals for his doctoral dissertation. He met with various collectors because he wanted to work on their seals. He wanted to see some of the seal impressions on their jar handles. Some of these enabled him to decipher seal impressions from Lachish, Beth Shemesh, Tell el-Nasbeh and elsewhere, inscriptions that had previously been unreadable. He wanted to publish some of his results in BASOR. To do so, he had to jump through various “hoops.” First, he had to publish the inscriptions somewhere else. Then he could publish his larger, more comprehensive article in BASOR.

On the other hand, he understood that there were two sides. “As a scholarly community,” he asked, “are we promoting looting if we go and talk to people who collect antiquities? Should we talk to people who come to us and want to talk about some antiquity they have purchased?” It depends on each case, Vaughn concluded.

But Vaughn would not let the scholars avoid the moral and ethical questions just because they were committed to knowledge—even knowledge from looted objects. “As scholars, we cannot say that it’s not our job to ask ethical and moral questions. We do have to make ethical and moral choices. We’re scholars. To separate ourselves from these moral obligations I feel is wrong. As scholars, we have a responsibility for what we do. The problem is not black and white ... Most of our choices,” Vaughn concluded, “will be in the gray area.”

On the other hand, many participants stressed the importance of learning as much as we can, even from items that come from the antiquities market. Alan Millard equated the failure to publish important inscriptions with their destruction: “If they’re not published, they are virtually destroyed.”

Wolf Krumbein: “I think publishing has to be done. Publicity is always good.”

Hanan Eshel: “As scholars, our job is to learn as much as possible about the past.”

Even some of the most conservative people would make an exception for the thousands of looted cuneiform tablets that are now coming out of Iraq.

A number of scholars expressed the view that it was important to publish even artifacts and inscriptions thought to be forgeries. “If you think that an object is a fake, what is your obligation?” asked Hanan Eshel. “I think we should write about our doubts ... Sometimes I think, ‘Why should I waste my time to do it?’ Because it’s important that senior scholars like Cross and Naveh and Lemaire tell people.” Eshel: “I think that it’s important for senior scholars to publish those doubts, because if they won’t publish those doubts, at the end bad inscriptions will contaminate the whole field.”

“It’s naïve to boycott [unprovenanced inscriptions] like most Americans do,” said Shmuel Ahituv. “Things will be published sooner or later by charlatans or second-raters, so we have to publish them and then put them on the shelf as suspected [forgeries].”

André Lemaire stressed the importance of retaining even objects that are considered forgeries. Perhaps some day we will change our mind. But even if not, forged documents have something to teach us. Millard mentioned the famous Shapira strips, which Moses Shapira touted in 1883 as authentic fragments of an ancient Torah scroll. They were displayed in the British Museum when Charles Clermont-Ganneau noted little pinholes in the strips; they were the margins of a modern Torah scroll that had been sewn together.

Shapira was exposed as a forger. He fled to Rotterdam, rented a hotel room and shot himself in the head. The strips have disappeared. Even if they are forgeries (although it might be nice to review the question after all these years), “we should still like to have this material at hand,” said Lemaire.

“Let the reader pass judgment,” said Avi Hurwitz. “We cannot suppress it and hide it from the public.”

Conference participants also acknowledged that scholars’ own prejudices and biases can influence their conclusions. Andrew Vaughn was among those who recognized this: “I think it’s impossible to say that we don’t have suspicions or don’t make assumptions before we start. I don’t think any of us are able to do that. I agree we have to try to be neutral. I try. But I think it’s important for me to acknowledge I am suspicious and I have my doubts, and they have become more and more keen over the last few years. That’s an important thing to acknowledge, rather than try to pretend that that’s not how I feel.”

For Vaughn, his suspicion or skepticism is a factor in arriving at his conclusion: “In general I’m a suspicious person. I’m even more suspicious when faced with something that talks about Jesus [the James Ossuary Inscription], or talks about cultic worship [the Ivory Pomegranate Inscription] or talks about a Judahite king [the Yehoash Inscription].

Aaron Demsky agreed with Vaughn’s view regarding the existence of scholarly bias and the importance of trying to suppress these biases: “We cannot escape our own predilections at the way we look at things. Whether we suspect them or not, we cannot come with a completely objective approach. It would be very, very difficult to do. I think that’s the fact that should be emphasized. We should be aware of our own limitations. That is very important.”

Several participants favored the development of protocols that would offer suggestions as to how the scholarly community should treat unprovenanced artifacts and inscriptions. One of them was Andrew Vaughn: “We should establish protocols. This would be a very worthwhile outcome of this conference. We could recommend to the field protocols that people should use when encountering unprovenanced objects and inscriptions.” He added that that would be a very good start as a basis of an exchange of views, what he called a conversation, with ASOR and other professional societies. He also added that we should “avoid being imperialistic” about these protocols. We should not be telling independent countries or societies what they must do. “I’m not sure how to solve it, but it’s something that has to be thrown into the mix.”

Wolfgang Krumbein seconded the idea: “I want to very much favor the idea of Andy Vaughn [regarding protocols].”

I charged right in with the obvious response. Andy Vaughn was appointed the chairman of our Protocol Committee and Wolf Krumbein became the first member. To this was added Alan Millard, thus comprising a three-person Protocol Committee.

One of the protocols that everyone seemed to agree on was the flagging of unprovenanced inscriptions. But I sensed a buried difference, which was also reflected in the discussions regarding the specific inscriptions. While all agree that there should be some indication that an inscription is unprovenanced, some participants regard this as a kind of “Mark of Cain,” a way of condemning the inscription from the very start. André Lemaire, while supporting the flagging of non-provenanced inscriptions, does not believe this flagging should be the Mark of Cain: “There is no problem to indicate a fact. It is a fact. When we have an unprovenanced inscription and when we give it a number, we would add an apostrophe. It is very simple. It is a fact. But it is not to judge, not to make

a judgment *a priori* whether it is good or not. It is just to tell them clearly that this inscription is unprovenanced.”

On the matter of caution and skepticism regarding unprovenanced material, Lemaire tells us: “My problem is that we have to be cautious both ways. This is very important to be skeptical both ways.” In other words, we need to be just as cautious in declaring an inscription a forgery as in finding it authentic. “If you start with an *a priori* position that something may be a forgery, then you can always find an explanation consistent with forgery. In my view, you have to start with the facts and then see where they lead.”

This argument was highlighted with respect to some bullae (seal impressions) that Andrew Vaughn concluded were probably forgeries. He was critical of Robert Deutsch’s “supermaximalist” approach with regard to the authenticity of bullae. Vaughn quoted Deutsch:

“Is it reasonable to ask whether they [bullae] could be fakes? The universal answer of all experts in the field is no. It is simply impossible to fake them. The wet clay bullae were not made at the time they were imprinted, but dried upon the document they sealed. They hardened only in a fire that destroyed the documents the bullae sealed. For this reason, they are very fragile. All have worn down during the last 2,700 years. All have small cracks and surface corrosion, and under a microscope we see small crystals in the cracks and on damaged edges and surfaces. None of this can be duplicated.” So Deutsch.

Vaughn says Deutsch is “too dogmatic. I have problems when people say it is impossible ... Why couldn’t someone fake these features if the incentives were high enough ... On the other hand, Deutsch is correct that it would be difficult to fake a bulla.”

Some people favored the creation of an institute that would study and evaluate whether unprovenanced inscriptions are authentic or forgeries. Of course, some participants were against the idea.

I objected to forgery vs. authenticity committees in which the members based their votes not on their own expertise, but on the expertise of other committee members. This seems to have been the case in the IAA committee that declared the Ossuary Inscription and the Yehoash Inscription to be forgeries and in the IAA and Israel Museum committee that declared the Ivory Pomegranate Inscription to be a forgery. Members of the committees, I said, are appointed not as a jury to listen to the evidence, but for their particular expertise. How they vote should depend on their own expertise. If their expertise does not tell them how to vote, they should refrain from participating in the decision. I have expanded on this argument in an appended paper entitled “Judging Forgeries—The Difference Between a Jury and a Committee of Experts.”

Finally, I call attention to Gaby Barkay’s talk, in which he presents ten points for discussion. Some of these points have already been the subject of some discussion in this report. Others have not. They all are significant, however. At the conference, Gaby expanded on each of these points extemporaneously and brilliantly; the full text of his talk appears later in this collection and a synopsis appears in the Appendix.

ⁱ André Lemaire, “Burial Box of James the Brother of Jesus,” *BAR* November/December 2002, p. 24; André Lemaire, “Israel Antiquities Authority’s Report Deeply Flawed,” *BAR* November/December 2003, p. 50.

ⁱⁱ Nahman Avigad, Benjamin Sass, *Corpus of West Semitic Seals* (Jerusalem: Israel Exploration Society, 1997), p. 12.

ⁱⁱⁱ Emile Puech, “A Propos de L’Ossuaire de Jacques, le Frere de Jesus,” *The Polish Journal of Biblical Research*, December 2002, pp. 7–22; “Une inscription controversée,” *Le Monde de la Bible*, no. 149, March

2003, pp. 62–65; Emile Puech, “James the Just, or Just James? The ‘James Ossuary’ on Trial,” *Bulletin of the Anglo-Israel Archaeological Society*, vol. 21 (2003).

^{iv} Peter T. Daniels, “Review of *Absent Voices: The Story of Writing Systems in the West*,” *Maarav* 11.1 (2004), p. 103.

^v Andrew Vaughn and Christopher Rollston, “Epilogue—Methodological Musings From the Field,” *SBL Forum*, March 2005.

^{vi} L.Y. Rahmani, *A Catalog of Jewish Ossuaries* (Jerusalem: The Israel Antiquities Authority, 1994).

^{vii} But see Christopher A. Rollston, “Scribal Education in Ancient Israel: The Old Hebrew Epigraphic Evidence,” *BASOR*, No. 344 (2006) p.47 at p.51: “Modern analogies of variation for a modern script series are of negligible value, as much more script variation is tolerated within the modern period.”

^{viii} André Lemaire, “Israel Antiquities Authority’s Report Deeply Flawed.” *BAR* November/December 2003, p. 50.

^{ix} http://www.biblicalarchaeology.org/bswbOOssuary_Krumbeinreport.pdf

^x The first item listed in his fields of research in his resume on the Tel Aviv University website is: “Provenance and technological studies of archaeological ceramics ...” None of his fields of research involves stone. Aside from his articles on the alleged forgeries considered by the conference, only one of his scientific articles involves a stone artifact.

^{xi} See Yuval Goren et al., “A Re-Examination of the Inscribed Pomegranate from the Israel Museum,” *Israel Exploration Journal*, vol. 55 (2005), p. 3.

^{xii} Christopher A. Rollston, “Non-Provenanced Epigraphs I: Pillaged Antiquities, Northwest Semitic Forgeries, and Protocols for Laboratory Tests,” *Maarav*, vol. 10 (2003), p.135 -136.

^{xiii} Christopher A. Rollston, “Non-Provenanced Epigraphs II: The Status of Non-Provenanced Epigraphs Within the Broader Corpus of Northwest Semitic,” *Maarav*, vol. 11 (2004), p. 57-71.

^{xiv} André Lemaire, “A Re-Examination of the Inscribed Pomegranate: A Rejoinder,” *Israel Exploration Journal*, vol. 56 (2006), p. 167.

^{xv} Edward L. Greenstein, “The Linguist: Hebrew Philology Spells Fake,” *BAR* May/June 2003, p. 28.

^{xvi} Cohen emphasizes, however, that the literary genre of royal building inscriptions is not extant in the Bible or in extra-Biblical Hebrew inscriptions. The ancient Near Eastern Semitic literary corpus in which this genre is best represented in Biblical times is the Akkadian royal building inscriptions from Mesopotamia.

^{xvii} In Cohen’s opinion, not only does the new Ekron inscription provide at least a partial precedent for the blessing of the land (and by logical extension, the people of the land as well) at the end of a royal building inscription/dedication (the genre of the Ekron inscription), but the emphasis on the blessing of the people (rather than of the king himself or even of the land) at the end of the Yehoash Inscription could well represent priestly influence on this royal building inscription in accordance with the very influential position of the High Priest Yehoyada` and his special relationship with King Yehoash (2 Kings 11–12; Jeremiah 29:26; 2 Chronicles 22–24; see especially 2 Kings 12:3)

^{xviii} See “BAR Offers \$50,000 Reward,” *BAR* May/June 2007, p. 18.

^{xix} A particularly dramatic moment in this debate between Cohen and Greenstein occurred when Cohen challenged Greenstein to translate lines 10-14 of the Yehoash Inscription according to his understanding of the way the assumed forger used the phrase VA’A`AS ‘ET BDQ HBYT based on its meaning in Modern Hebrew. Cohen claimed that the two words BDQ HBYT represent the first of seven direct objects of the verb VA’A`AS in lines 10-14 of the YI and the meaning of VA’A`AS in this context must be “(Then) I renovated” (as translated by Cohen in his translation of the entire Yehoash Inscription provided in his handout). Cohen’s translation precludes the possibility of Modern Hebrew influence in this case. Greenstein’s response was that he saw absolutely no need to provide a rational literal translation of the Yehoash Inscription, because he doesn’t think that the assumed forger was capable of composing such a rational inscription.

^{xx} By contrast, he testified he was a “non-expert” in inscriptions from the Second Temple period.

^{xxi} Rollston has elsewhere stated: “The innumerable intricacies of the morphology of the letters of a horizon of a script series contain enormous amounts of data that can be analyzed and documented in an empirical manner by a trained palaeographer.” Christopher A. Rollston, “Scribal Education in Ancient Israel: The Old Hebrew Epigraphic Evidence,” *BASOR*, No. 344 (2006), p. 47-51.

^{xxii} Elsewhere Rollston admits that sometimes inscriptions cannot be dated “with precision”: “The script of [the Ophel] inscription exhibits several hallmark features of the eighth century, and so I assign it to that century ... Naveh has dated it broadly to the seventh century.” Rollston, “Scribal Education in Ancient Israel: The Old Hebrew Epigraphic Evidence,” p. 52. Similarly in the case of the Gibeon jar handles; Rollston dates them to the eighth century B.C.E. and Cross dates them to the seventh century B.C.E. Hence, “*Pace Cross*,” *id.* at 54.

^{xxiii} Rollston, “Non-Provenanced Epigraphs I,” p. 139.

^{xxiv} Rollston, “Non-Provenanced Epigraphs I,” p. 136.

^{xxv} David Noel Freedman (and Pam Fox Kuhlken), *What Are the Dead Sea Scrolls and Why Do They Matter?* (Grand Rapids: Eerdmans, 2007), p. 72.

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- ^{xxvi} David Adan-Beyewitz, Frank Asaro, Robert D. Giauque, "The Discovery of Anomalously High Silver," *Archaeometry*, vol. 48, 2006, pp. 377–398.
- ^{xxvii} See "IAA Geologists Repeatedly Err," *BAR*, March/April 2004, p. 47.
- ^{xxviii} Shmuel Ahituv, "A New Moabite Inscription," *Israel Museum Studies in Archaeology*, vol. 2 (2003).
- ^{xxix} Unless the object's ownership can be traced to 1972 or earlier.

FORGERY CONFERENCE REPORT

APPENDIX

**ABSTRACTS BY
PARTICIPATING SCHOLARS**

The New Moabite Inscription

Shmuel Ahituv

The stone is of course unprovenanced. However I believe it to be authentic.

1. There is no problem with palaeography. The unique feature in the set of letters is the typical Moabite *mem* and *nun*.
2. The broken octagonal piece of column is a novelty, but we have also the 12-sided *miqtar* (incense altar) excavated in Khirbet el-Mudeyineh.
3. While the faces of the column are smooth, its upper and lower sides are roughly dressed. The stone was redressed and used as building material. It still has signs of dirt and mortar.
4. One may argue that a modern forger used the broken column and inscribed on it his invented text. I doubt it for the following reasons: (a) The letters of the top line are cut diagonally, thus obliterating the upper part, or more, of letters that were formerly chiseled in the stone with much care. (b) The same goes for the last line and the break in the lower part of the stone. (c) There are more defective places in the stone. I cannot imagine that someone invested time and toil to incise words in the hard stone and then will intentionally smash the stone and jeopardize his investment.
5. The text has new words, and a still unknown toponym: *Beth-harosh*. It has affinities with the Mesha Stele, but it does not follow it servile. If I was to invent a text I would not write *bny mn* with a *yod*, but will follow the Ammonite spelling *bn mn*.

ABSTRACT

BAS Forgery Conference

Gabriel Barkay

I will present 10 points for discussion concerning the subject of recently published objects regarded by some scholars as forgeries.

1. The importance of experience, deep knowledge and status of experts. One should honor the experts' views, and not dismiss their judgment.
2. The matter of ancient inscribed objects is part of the culture of past civilizations. The subject should be dealt with by epigraphers, palaeographers, linguists, historians and archaeologists. The contribution of scholars from the sciences--geologists, chemists, etc. is secondary.
3. The authentication of ancient inscription is done in scholarly publications, in articles *pro* and *con*. Those who will be most convincing shall tip the balance.
4. The authentication is not a legal matter, and the court of justice is not the place for such matters.
5. The principle of "Too good to be true" is not an argument that should be used; would we use this argument if a new Dead Sea Scroll is surfacing in the antiquities market?
6. Controlled excavations and archaeological context are of utmost importance. But even in archaeological excavations there are surprising objects which do not fit their context. One should not automatically reject objects without proper context. The obligation of scholars is to enhance knowledge about past civilizations and not to catch robbers. Scholars have studied and must study any ancient object from any source. That is their obligation.
7. Linguistic and palaeographical anomalies exist in well known and authentic inscriptions. An anomaly is not a reason to reject the possibility of an inscription to be authentic. The famous Siloam Tunnel inscription and the Moabite Stone have also linguistic anomalies.
8. A priori we should accept the integrity and honesty of scholars, unless proven otherwise. Accusations and insinuations about scholars' integrity should be rejected.
9. If we assume that some or all the recently published inscribed objects are fakes, then he who faked them has to be an expert in a multiple of disciplines. He has to be a first class expert in ancient history, in paleography, in linguistics, in Biblical studies, in chemistry and in geology, in addition to the technical ability of engraving the inscriptions. There is no such person! If the faking is a result of a team's work, then there had to be some leak out of information; it would be impossible to keep such secrets in such a small community.
10. Time is very important, we should give things a certain duration of time. The Dead Sea Scrolls and the Moabite Mesha inscription were thought to be forgeries for some ten years after they were first published. In due time a consensus emerges.

The Proper Evaluation of Philological Evidence as Part of the Determination of the Status of Authenticity Regarding the Unprovenanced Yeho'ash Royal Building Inscription (Yehoash Inscription)

Chaim Cohen (Ben-Gurion University)

While philology is not the only factor in determining the status of authenticity of an unprovenanced inscription (obviously the physical and geological aspects as well as the paleography of the entire inscription must also be thoroughly investigated), the relative importance of the philological aspect is surely enhanced when dealing with such a relatively long inscription as the Yehoash Inscription (16 lines).

The only reasonable method of philological inquiry when dealing with such unprovenanced inscriptions (which certainly could be the product of modern-day forgery) is to concentrate on those aspects (lexical, grammatical, syntactical or orthographic) which appear to be unique) (or nearly so) vis-a-vis the extant ancient corpus in the language of the inscription (in the case of the Yehoash Inscription, unique vis-a-vis Biblical Hebrew). This is because the similarities with respect to that corpus could reflect *either* the natural tendency of the modern forger to “play it safe” and imitate as much as possible from the extant ancient corpus, *or* (no less likely) the natural tendency of ancient authentic texts to be very similar in nature and sometimes even serve as sources for one another. Thus as regards the Yehoash Inscription, similarities with Biblical Hebrew should not be used as decisive evidence for or against authenticity. Labeling the Yehoash Inscription in advance as a forgery and then proceeding to demonstrate how the assumed forger constructed his masterpiece by pointing out all the similarities is surely begging the question.

It must first be investigated if any of the unique differences could be attributed to the modern language of the potential forger (e.g. modern Hebrew in the case of the Yehoash Inscription).

Such items (so-called “smoking guns”) could surely serve as *prima facie* evidence of forgery *if philologically validated beyond any reasonable doubt*. On the other hand, unique differences that philologically cannot be so attributed to the modern-day dialect of the potential forger, but can be shown to be proper ancient usage on the basis of comparative philology, may potentially serve as evidence to the contrary, namely in favor of authenticity. The more sophisticated the level of philological (and especially comparative philological) knowledge that would have been required by a potential forger in order to properly reconstruct such ancient usage, the less likely that the inscription in question is a forgery. Such unique aspects of legitimate ancient usage could be reflected either by the proper usage of unique lexical items or by the display of explicit knowledge and strict adherence to rules of grammar, syntax and orthography that were hitherto unnoticed or misunderstood (even if it becomes apparent in retrospect that they were frequently utilized in the ancient corpus).

Such an intense philological investigation requires the use of all the tools at our disposal and may involve many hours of comparative philological research. When the literary genre of the unprovenanced inscription is not represented in the extant ancient corpus of texts in that language, this is even further justification for such comparative philological research. In the case of the Yehoash Inscription, the literary genre is clearly “royal

building inscription,” a genre without representation in the extant Biblical Hebrew corpus, but of course well-attested to in ancient Mesopotamian literature. It thus stands to reason that important comparative philological evidence in this case will be forthcoming from the many extant Akkadian royal building inscriptions, many of which deal specifically, as does the Yehoash Inscription, with the renovation of temples and other sacred buildings.

In my article, I deal with eight such items (one of which is based on my reconstruction of the text) occurring in the first two sections (lines 1-14) of the Yehoash Inscription (my complete reading of the text together with a full English translation is also included in this article). In each case, it has first been clearly shown that the discussed item provides absolutely no philological evidence in favor of forgery. But the main purpose of this comprehensive article was to highlight the important contributions to Biblical Hebrew philology that have come to light as a result of my extensive research on the Yehoash Inscription. These contributions define the sophisticated level of knowledge that the alleged forger would have needed to possess concerning new rules of Biblical Hebrew grammar and syntax, as well as special technical usage of First Temple Hebrew building terminology (the latter based especially on the comparative philological evidence of extra-Biblical sources). This then leads to my final conclusion that *either* the Yehoash Inscription is a completely authentic inscription from the First Temple period *or* it is a most brilliant forgery!

In my current presentation, I intend to briefly review the eight items extensively discussed in the attached article and add to them two more widely discussed items from lines 15 (׳*dt* “witness, testimony”) and 16 (׳*mw* “his people” [in the singular with consonantal *waw* as alternative 3ms. pronominal suffix]). As with the aforementioned eight initial items, I will here, too, first attempt to prove that these two items provide no evidence whatsoever in favor of forgery. But the main purpose will be to demonstrate the contributions to Biblical Hebrew philology that have come to light as a result of my research on these two items, making the required sophisticated level of knowledge of the alleged forger that much higher.

Reading NW Semitic Inscriptions

Aaron Demsky

As I mentioned to Hershel [Shanks] in the planning stages of this conference, we as colleagues who have collectively devoted so much time, talent and effort to deciphering epigraphic texts and improving difficult readings should take this opportunity to share, in a relaxed atmosphere, our cumulative experience clarifying methods of research. Reflecting on our science and especially how decisions are made should be helpful in assessing the veracity and importance of the specific texts under discussion. The conference should be a learning experience more than an academic debate.

This conference gives me an opportunity to put into writing some of my thoughts on formulating a methodology for epigraphic study. While this is a very personal account, I do this with the hope that other participants in the field will add their comments. Perhaps this note will be the basis of a joint venture setting down the guidelines for the next generation of practitioners, a sort of checklist for further research. Because of time constraints, I cannot survey the valuable contributions made by all of you to North West Semitic (NWS) epigraphy. My apologies.

The study of ancient NWS inscriptions has been a major part of my understanding of the history of the ancient near east. I have been fortunate in studying these texts with different teachers representing complimentary disciplines. As an undergraduate student, I was introduced to NWS epigraphy by Professor Nahum Sarna, who viewed these texts in relation to the biblical narrative and Hebrew language. After coming to Israel in 1965–66, I studied the subject in three separate classes, one in paleography with Prof Nahman Avigad, NWS languages with Professor Yehezkel Kutcher, and as a source of biblical history with Professor Hanoah Reviv. At the time, I was amazed how the very same texts could be taught with almost no overlap. I was even more amazed that there were no other students sitting in any two of the three classes. After that year, I was convinced that the only way to fully understand these texts was to apply an integrative approach.

How I look at epigraphic texts:

1. The first distinction is whether they were discovered in **provenance** or not. It is obvious, that a document purchased on the antiquities market is suspect. If it was found in an archeological context, one should note whether it was found in primary (Achish inscription) or in secondary use (Tel Dan inscription). Of course texts that were found on a tell but not in a secure archaeological context present certain problems of exact dating (e.g. the Gezer calendar). However, I would not dismiss a document from consideration if it was non-provenanced.

2. I take note of the **writing surface**. For instance, an inscription engraved on stone can be typed: a) a monumental inscription engraved on a prepared surface of natural stone (Siloam inscription), or the burial inscription (Royal steward), or b) on a stone building block (Shifitbaal from Byblos; Achish from Ekron), or c) a shaped stele (e.g. Mesha, Tel Dan), or d) a (soft) stone tablet (Gezer calendar), e) Seal and bullae are another sub-field of study.

Another writing surface that has turned up is plastered walls, inscribed in ink with a text of a religious nature, probably a foundation inscription telling of the sanctity of the place and meant to be a display copy of a scroll (Kuntilat Ajrud; Balaam; see Deuteronomy 27:2–8; cf. Joshua 8:30-35; Daniel 5:5).

Another surface is the one used on occasional inscriptions written in ink or engraved on an ostrakon or perhaps chiseled on pottery, before or after firing.

Perhaps it is a votive inscription inscribed on an artifact made from a precious metal or ivory (Pomegranate inscription)? Whatever the case, there seems to be a definite relationship between the writing surface and the content and form of the inscription.¹

Other physical aspects of the inscription that should be noted: was it engraved or written in ink? In late antiquity, there are mosaic inscriptions with their specific problems.²

3. Paleography is the main stay of epigraphic studies.

Especially when studying Proto-Canaanite inscriptions, one must take into account the direction of writing and stance of the letters.³

There is a basic distinction between lapidary and cursive styles that might be contemporary in a given society or scribal tradition. This distinction has bearing on the form of the letters and their evolution; those engraved on stone might be more lapidary than contemporary inscriptions written in ink in a cursive hand on parchment or papyrus. We cannot compare linear evolution of letter forms evolving from one type of writing to another. In fact, we may even find different scripts, engraved or in ink, in use in the same archaeological context.⁴

Then again, from the ninth century B.C.E., there are developing national scripts that have to be considered.

Letters have to be analyzed as to their stance, incline and measurements. However, it is common that there are slight differences between the same letters in the same inscription. Of course if we are looking at late Second Temple Herodian times there are alternate forms of the same letter (e.g. open and closed *mem*) appearing in the same inscription (Uzziah)⁵.

Several letters like *r/d y/w/z, k/b* tend to be confused in both the Old Hebrew and in the Jewish scripts. The comment about Rabbi Eleazar ben Arakh (BT Shabbat 147b) is enlightening in this context. This illustrious scholar forgot all his learning to the point that he misread the verse *החדש הזה לכם* (Exod. 12, 2), as if it was *החרש היה לבם*, confusing the paleographically similar *resh/dalet, yod/ zain, bet /kaf*.

Sometimes the context will solve or decide the reading of certain letters. For instance, the Gibeon handles record the word *gdd/r*. In half the cases, the last letter can be read equally as well as a *resh* or as a *dalet*. Cross and Avigad read the last letter as a second *dalet*, while Pritchard and Albright favored the *resh*. Other sources have to be brought to bear in deciding the issue. In this case, the Gibeon genealogy (I Chr 8: 31; 9:37) and the

¹ Demsky, "Writing and Writing Materials in the Biblical Period," *Encyclopaedia Judaica* vol. 16, (1971), cols. 654-665.

² Demsky, "The Permitted Villages of Sebaste in the Rehob Mosaic," *IEJ* 29 (1979), pp. 182-93. (A shortened Hebrew version appeared in *Qadmoniot* 11 (1978), fascicles 42-43, pp. 75-77.)

³ Demsky, "A Proto-Canaanite Abecedary Dating from the Period of the Judges and its Implications for the History of the Alphabet," *Tel Aviv* 4 (1977), pp. 14-27. Reprinted in M. Kochavi et al., *Aphek-Antipatris 1974-1977 The Inscriptions*, Tel Aviv University Institute of Archaeology (Tel Aviv, 1978), pp. 47-60. [A slightly different Hebrew version appeared in *Sefer Bar-Ilan*, vol. 14-15, pp. 45-57.]

⁴ A. Maier and A. Demsky, "Tel Gath Inscriptions" (in preparation).

⁵ Demsky, "The Trumpeter's Inscription from the Temple Mount," *Eretz Israel* 18 (N. Avigad Volume) 1985, pp. 40-42 (Hebrew); English summary, p. 66*.

proximity of the village of Judeira convinced me that the disputed letter was a *resh*.⁶ Similarly, it was the biblical term לחל קדש בין להבדיל as used in Mishnah Sukkah 5:5 listing the trumpet blasts in the Temple service, that was the basis of my reading *lehav[dil]* in the Trumpeter's inscription and dismissing the reading *lehakh[riz]*, i.e. reading a *bet* for a *kaf*.⁷

Another point to consider is the relationship between the words in the inscription whether separated by a word divider (dot or dash) as in Old Hebrew inscriptions, or with spaces between the words as introduced seemingly by Aramean scribes in the seventh century B.C.E., or as *scriptio continua*, without spaces, as in Phoenician inscriptions.⁸

Furthermore, one should note other significant signs like numbers⁹ borrowed from Egypt or Phoenicia and sometimes adapted with different values.

4. Linguistic features. Identifying the NWS dialect is not always easy, to wit the Balaam inscription (Aramaic or Gileadite?), or the Gezer calendar (Canaanite or North Israelite?).

Another linguistic feature to be considered is the use of *matres lectionis*, whether infixes or suffixed as well as orthographic features in general. Are texts consistent in this matter? Compare Mesha stele *b(y)t* in lines 7,23,27,30 (defective), and in line 25 (plene)? Can this feature be a basis for dating a text? There is a difference of opinion in this matter between Cross/ Friedman and Y. Kutscher.

After deciphering the script which may be fairly easy, then read the text.

A difficult word, i.e. a *hapax legomenon*, or a damaged text or an unusual letter form, can sometimes be explained contextually (e.g., *zdh* in Siloam inscription). Sometimes we have to search for cognates in the NWS languages (dialects) starting from the closest. Methodologically, I maintain that the closest dialect to Biblical Hebrew is Late/Mishnaic Hebrew.¹⁰ Where there is no linguistic tradition we have to look at synchronic material. For example, I identified the Moabite crux *kl ršh w's'h* (MS line 21) with biblical Hebrew *nasa' roš*.¹¹ However, I explained the Phoenician *`lt* in Tabnit and Eshmunazor burial inscriptions, as an early cognate of eastern Talmudic Aram: *`ilaita* "coffer/coffin".¹² The term *šemen raúaş* in the Samaria ostraca meaning "bath oil" is supported by a Ugaritic literary text.¹³

Onomastics is an important aspect of the linguistic content of an inscription, with consequences regarding the cultural/ethnic identity of the person mentioned. For instance, Yahwistic names in the biblical period will almost always indicate an Israelite and can be dated from ca. 1200 B.C.E. (Numbers 13:16). Compare the name *AVY[H]* in the colophon of the Gezer calendar. Sometimes an unexpected personal name turns up

⁶ Demsky, "The Genealogy of Gibeon: Biblical and Epigraphic Considerations," *BASOR* 202, (1971), pp. 16-23.

⁷ Op.cit. Demsky, "The Trumpeter's Inscription".

⁸ A.R. Millard, "Scriptio Continua' in Early Hebrew: Ancient Practice or Modern Surmise", *JSS* 15 (1970) pp. 2-15; J. Naveh, "Word Division in West Semitic Writing" *IEJ* 23 (1983), pp.206-208; See also S. Ahituv and A. Demsky in Y.Goren, et al, "A Re-examination of the Inscribed Pomegranate from the Israel Museum", *IEJ* 55, p. 14.

⁹ Demsky, "'Dark Wine' from Judah," *IEJ* 22 (1972), pp. 233-234.

¹⁰ Demsky, "Dark Wine"; Demsky, "A Note on *yyn 'šn*," *Tel Aviv* 6 (1979), p. 163.

¹¹ Demsky, "King Mesha Takes a Census", *Eretz-Israel* 26 (F.M. Cross Jub. Vol.) 1999, pp. 43-44 (Hebrew); English summary, p. 228*.

¹² Demsky, "An Unnoticed Phoenician - Aramaic Cognate for Coffin and Coffer", *Shnaton* 12 (2000), pp. 195-198 (Hebrew); English summary, pp.13-14.

¹³ Demsky, "Shemen Rahaz -Bathoil from Samaria", in S. Gitin Fs (forthcoming)

like Muwaqqir or Gibbeaú (MPQD ostrakon). My advice is to check for a misreading or a nickname.¹⁴ In a community where there are linguistic and cultural ties to the Greek world and where the king bears a name like Ikausu, reflecting that culture, it is more likely that his goddess is called by the common Greek term *potnia* “Lady” which becomes her personal name (cf. Donna) in this Philistine setting. It seems to me to be the logical choice rather than inventing a goddess by the name of PotaGea.¹⁵

5. Literary structure. The epigraphic material can be analyzed according to its literary genre from the most elementary abecedary¹⁶ to the more structured and rich monumental royal inscription. As I said above there seems to be a relationship between types of writing surfaces and the literary genre of the inscription. For instance NWS monumental royal inscriptions engraved on a stone stele are usually of considerable length of 20-30+ straight lines with few spelling errors if at all.¹⁷ It tends to follow the basic form: a) introduction, beginning with 1st person sing (*anokh[ij]/ ana*), noting the name of the king, his patronym + lineage, the name of the god to whom the inscription is dedicated, the king’s piety and answer of the god; b) description of the military duress from which the king was saved; c) his good deeds and public works; d) curse formula. The curse formula is independent of the historic events described in the earlier parts of the document. It is a literary genre in its own right that sometime can be traced to scribal tradition of many centuries. It can be restored by comparing it to contemporary items.¹⁸

Similarly, the royal votive stone inscription has its characteristic structure: The name of the object dedicated (temple, statue) by the royal donor to a god /goddess, followed by a request for a divine blessing, usually long life (Byblian royal inscriptions, Ekron; see 1 Kings 3:11).

But there are monumental inscriptions lacking the characteristic structure. A case in point is the Siloam Tunnel inscription, which Levi della Vida identified as an excerpt from the Chronicles of the king of Judah (2 Kings 20:20).

6. The historic message. After deciphering the inscription one should then view it as an historic source, which may illuminate the areas of education, finance, and social structure, military and political history. Even the most occasional text like one of ownership may lead to new insights into the larger issues of literacy in ancient Israel.¹⁹

Of particular interest is the possibility that an epigraphic text describes an historic event which contradicts the biblical narrative. The Mesha stele is a case in point. The Moabite king describes in detail his revolt and fortification of border cities in anticipation of the imminent Israelite attack on his kingdom while II Kings 3 describes the joint Israelite Judean campaign against Moab. Similarly the Tel Dan inscription might give us a

¹⁴ Demsky, “The MPQD Ostrakon- A New Reading” , *BASOR* (2007), pp 1-6

¹⁵ Demsky, “The Name of the Goddess of Ekron: A New Reading”, *JANES* 25 (1997), pp. 1-5.

¹⁶ Demsky, “Abecedaries” in W.W. Hallo (ed.), *The Context of Scripture-Vol. 1: Canonical Texts from the Biblical World* (Leiden, 1997), pp. 364-367.

¹⁷ Demsky, “On Reading Ancient Inscriptions: The Monumental Aramaic Stele Fragment from Tel Dan”, *JANES* 23(1995), pp. 29-35.

¹⁸ Demsky, “Mesopotamian and Canaanite Literary Traditions in the Ahirom Curse Formula,” *Eretz Israel* 14 (H.L. Ginsberg Volume) 1978, pp. 7-11 (Hebrew); English summary, p. 122*.

¹⁹ Demsky, “Literacy”, in E.M. Meyers (ed.), *The Oxford Encyclopedia of Archaeology in the Near East* Vol. 3 (New York, 1997), pp. 362-369.

different view of the death of Jehoram king of Israel,²⁰ providing more than one perspective of biblical history (compare the Shishaq victory document).

All of the above points should come into consideration when deciphering an inscription.

* * * *

Hershel asked me how sure am I that the Pomegranate inscription is a forgery. My answer at the time was 80-90 percent. The truth is that I will never be sure. But reviewing the above list of points, a cumulative picture is formed.

1. The pomegranate is non-provenanced, which makes it suspect.
2. The writing surface is hippopotamus tooth, which was used for ivory carving in the Late Bronze Age, several hundred years prior to date of the form of the letters and the message of the text as belonging to the First Temple. This leaves us with a glaring discrepancy between evidence from material and palaeography.
3. As noted by Ahituv and myself the words are separated by spaces, a scribal device found in the contemporary Aramaic scribal tradition, but probably not used by Hebrew scribes at the time.
4. Add to these points, which cumulatively argue against authenticity, the observation seen with the help of a high powered microscope, that some of the letters were engraved after the pomegranate was initially damaged in antiquity!

For the sake of discussion, I will argue against this negative position:

1. The unique term *qdš khnm* does have a Late Hebrew parallel in *úerem shel kohanim* (Mishnah *Nedarim* 2: 4).
2. Spacing between words IS found on another eighth-seventh century non-provenanced Hebrew inscription--the "Dark Wine" decanter. Similar to the pomegranate, this inscription (written by a different hand) is engraved on its shoulder. Does this mean that spacing words already caught on in Hebrew scribal circles and thereby negates or weakens our above observation? Perhaps we are looking at another example of the work of the same "school", which adopted the Aramean scribal practice. The unanswered question is whether it is an ancient or a modern school of scribes?

²⁰ Demsky, "On Reading Ancient Inscriptions: The Monumental Aramaic Stele Fragment from Tel Dan", *The Journal of the Ancient Near Eastern Society* 23 (1995), pp. 29-35.

The (Almost) Perfect Fake And/Or the Real Thing

David Noel Freedman

The discussion following is about the two most notorious inscriptions that have turned up in recent years: The Proclamation of King Jehoash of Judah (ninth century B.C.E.) and the Ossuary of Jacob (= James) the Son of Joseph and the Brother of Jeshua (=Jesus) (first century C.E.). The former is written in Classical (=Biblical) Hebrew, while the latter is written in Palestinian Aramaic of the period. Following the publication of each of these inscriptions, a storm of controversy has arisen over the genuineness of each of them. Generally speaking, the same scholars who have questioned the genuineness of one have also questioned the genuineness of the other, while those who defend the genuineness of one, have also defended that of the other. But, there may be exception: A number of scholars have limited their pronouncements to one of the inscriptions and withheld comment on the other. Since the evidence and arguments are not the same for the two inscriptions, we should deal with them individually. But first, a few remarks about the basic issue Fake or Real --False or Real?

By definition, a perfect fake is undetectable, meaning that even the best and most skeptical experts will be taken in by it. Is either of these in that category, or are there any such in the whole inventory of ancient Near East inscriptions? Maybe some, maybe none, because perfection is hard to attain, even when the market is rich, the incentives are great and the necessary skills and techniques are available. Again, by definition, or major implication, such fakes would pass muster, at least a few, maybe more. Even if it were not perfect, what would it look like? The perfect fake would not be perfect itself, by which I mean that it would not be flawless. There would be an occasional novelty in wording, form or grammar and syntax. It might even have an error or two, depending upon the length of the inscription, since most of them would have been carved by stonecutters not scribes, artisans not artists. In other words, they would look very much like The Real Thing and be indistinguishable from authentic inscriptions by all the standard criteria.

Also within the definition, or implied by it, is the fact that whatever such an inscription may contain, it does not add to or substantially subtract from what we already knew or know about the subject matter. It may add a few details, but will not depart sharply from known data. It won't add appreciably to our current knowledge or subtract from it. It is simply there in all its false glory, to be admired and appreciated for being something other than what it is, an artifact from a later time. At the other extreme is a real thing, with real information from ancient times, and therefore important in its own right, even apart from the message that it bears, the materials of which it is made and the other details relating to its manufacture and installation. It belongs to real history and contributes to the recovery of it, as well as to our understanding of that time and place.

Where in this spectrum of FALSE and TRUE do these two inscriptions come? Right now, in my considered judgment, in spite of confident assertions from both sides of the argument, we really don't know and can't be sure. For each piece of evidence and for each category of discussion, there are opposing arguments, but none seem conclusive. For the purposes of the following discussion, we will call those who hold inscriptions to be modern fakes, THE FAKERS, while those who consider them to be real artifacts of the ancient past, THE ANTIQUARIANS. For example:

1) Linguistic Data and Argumentation: In regard to the Jehoash Inscription, the Hebrew is manifestly that of Biblical times, and corresponds to the period of the contents and the paleography (ninth century B.C.E.). The Fakers would test the inscription against a model for that time and place, and depending upon the individual scholar, would claim that the inscription doesn't fit within the range of authenticity, because it had too many novelties and anomalies, along with mistakes, or on the other hand, too few. In the prior case, the evidence would point to a clumsy modern forger, who simply substituted modern Hebrew expressions with which he (or she) was familiar and otherwise betrayed his contemporary workmanship through conspicuous lack of skill. At the other end, if there were very few departures from the ancient standard, that would show that the modern forger simply copied verbatim or blended materials drawn directly from the Bible or other ancient sources. And if the inscription fell somewhere between these extremes, the Fakers would argue that that only meant that the forger was even more expert at imitating reality. In other words, wherever the evidence weighs in, and whatever the number or percentage of proportion of variations and deviations from the norm, a fake is a fake, and a good faker can (and did) produce an inscription that would fit into the graph at any point from one extreme to the other. I would agree, that if the inscription is a fake, then the faker could make it fit anywhere in the spectrum we have sketched out. But this does not prove or even support the claim, which is presumed or assumed, and then justified or explained.

The Antiquarians, for their part, would proceed from the data on which both sides agree, and then argue that splitting the difference between the Fakes at both ends of the spectrum, claim that the linguistic features of the inscription fit comfortably between the extremes, and therefore there is no reason to question its authenticity. This is not proof either, whether for or against, and the debate will remain at an impasse, until and unless we find more evidence or make better arguments on either side of this debate. Both sides can claim high ground, on the selective use of some of the evidence in support of their position, while using ingenious and countervailing arguments to dismiss or explain away the opposition's views.

What has been said of the Jehoash Inscription applies in large part to the Ossuary of Jacob (commonly but wrongly called "James") the son of Joseph and the brother of Jeshua. The questions concerning genuineness and significance are both simpler and more complicated as the different sides of this controversy have emerged and joined battle. So far as I am aware, all those engaged in the discussion and debate agree that the ossuary itself is an authentic artifact of the first century C.E. and belongs to the latter part of that century, much like thousands of others from this region and from the first two or three centuries of the Common Era. This ossuary bears an inscription, as do many others from the same period. Most of the latter have not attracted much attention because names are common in this period, and further identification is generally lacking. That might have been the fate of this inscribed bone box, except for the collocation of three otherwise unremarkable names, and the specified relationship among the three persons mentioned: Jacob, Joseph and Jesus are three of the most important figures in the New Testament, and the combination of parentage and brotherhood is specifically true of Jesus, his brother Jacob and their father, Joseph. So the questions of authenticity and applicability become centrally important, and naturally and inevitably the arguments begin.

Even if we are ultimately satisfied that not only the Ossuary but also the Inscription on it are genuine artifacts of the first century C.E., what can be said about the combination of names? While Joseph and Jesus and James figure prominently in the New Testament,

the names themselves are quite common in the Jewish community before, during and after this period. The combination of the three names and the relationships specified in the inscription might have no historical connection with the members of the Holy Family in the New Testament. There would be no way to prove or disprove this particular hypothesis. It would remain an interesting speculation, nothing more, nothing less. Demographic figures and arguments might be used to support one conclusion or the other, but could hardly constitute proof, certainly not beyond a reasonable doubt. So that particular question should not be brought to bear on the other more basic question of authenticity. Alternatively, if the inscription were a fake, then the combination of names would be an obvious choice, but that observation would have to come after the determination of the question of authenticity. If the latter cannot now be settled, fitting the other pieces together won't settle the matter either.

When it comes to the wording, including reference to a brother, the inscription is unusual but hardly unique, and that applies as well to the Aramaic expression adding the pronominal suffix to the word for "brother;" it is also unusual, but not the only example. So the language could pass muster. On the other side, if it were a fake, we would expect a good faker to be careful to stay within the bounds of traditional linguistic usage in matters of this kind, especially in writing on gravestones, or in this case, an ossuary.

2) Much the same may be said of the results of *paleographic scrutiny and evaluation*. While the latter is a well-established science, and over the years admirable progress has been made in sorting out the genuine from the fabricated, and more precisions has been achieved in accurate dating (within a century in many cases), still, neither exactness nor unanimity have been achieved in either of these fundamental categories. Legitimate differences of opinion persist, and questions about some, if not many, of the most important ancient Hebrew and Aramaic inscriptions remain unresolved. In the case of the Edict of King Jehoshaphat of Judah, the jury is still out. I would say that it fits well within the first range between FALSE and TRUE. It might be what it claims to be, a proclamation of the ninth century B.C.E., or it might be a clever fake of the 20th or 21st century C.E. (depending on whose story you believe about its manufacture) or almost anything in between, in terms of the time span or the mode and process of its making. The conclusion would be that since no single item in the inscription has been shown beyond a reasonable doubt to be impossible, then the possibility that it is an authentic artifact must also be allowed.

About the paleography on the Ossuary: Some argue that the writing fits well with the period of the Ossuary itself, namely in the range from the middle to the end of the first century C.E., whereas others hold that some of the letters or words are anomalous, and argue further that more than one hand can be discerned, and that precisely the link Jeshua has been added by a later hand (very recently). In the end, all the anomalies taken together may only reflect the fact that one or more stonecutters actually did the job, based on sketches or drawings, and their own inadequate knowledge of the language and the content of the inscription. In other words, unusual features are proof only of unusual features, and these may or may not be compatible with a determination of authenticity or fakery, but are insufficient in themselves to make that determination. This debate has raged for some time now, and I don't believe that, as a group or individually, we really know enough or can settle this argument by the available evidence of force of reasoning. Not now, and maybe not ever. A confession (or two) might help, but as we all know only too well, even "unforced" confessions do not always have probative value, in court or out of it. As matters stand, we come out pretty much where we came in, with the verdict that the Scottish people hold dear: "NOT PROVEN!" On the

one hand, we can't affirm that the inscription is authentic, although it may be. On the other hand, we may not dismiss it as a proven fake.

By way of summary, let us say that we already knew the following about the two inscriptions:

1) That King Jehoash collected money for the Treasury of the Temple in Jerusalem in order to make repairs and renovations in the building, and issued an edict when the money had been collected, and the work was authorized and in progress (or even completed).

2) That whether or not the Ossuary is the receptacle of the remains of Jacob the son of Joseph and brother of Jeshua (of the New Testament), we already knew that Jesus had a brother named Jacob, and they were known as the sons of Joseph. Most of us believe that this was common knowledge of the time: That Jesus and Jacob were the sons of Joseph, and that Jacob later served as the leader of the Jerusalem community of Christians.

We would say that if the inscriptions are fakes, then the information provided hardly adds anything new or striking and nothing that would mislead scholars in the future. If authentic, they would not add much to the knowledge we already have. So in the end, whether real or fake, they don't seem to make much difference.

Some Methodological Principles in Determining that the So-Called Jehoash Inscription Is a Forgery

Edward L. Greenstein (Bar-Ilan University)

This abstract presents Professor Greenstein's remarks at the conference in a more pointed, expanded version. It does not contain footnotes or references. Those will be supplied in the event that he later publishes these remarks in a scholarly article.

The philological determination that an inscription such as the so-called Jehoash inscription is authentic or not should be based on at least the two following fundamental principles. First, if the text has been excavated in controlled conditions, it should be presumed to be authentic. That means that the philologist studying and trying to interpret the inscription should use every tool imaginable in order to read and understand it. Analogous usages and constructions from the entire linguistic spectrum, possible though unlikely forms, figurative and creative usages, as well as errors, should be considered in making sense of previously unencountered phenomena. If a text has not been found in a controlled excavation, its authenticity cannot be presumed. Second, when interpreting words and constructs, comparative evidence should be used in order to establish possible yet unattested usages in the text at hand. Comparative evidence should be used guardedly, however, when a particular usage is actually attested and is contravened by a form found in a given text. In other words, if we know how to say X in the language we are dealing with, one should be chary of suggesting that there is another way to say X based on a hypothetical analogy from another language.

The so-called Jehoash inscription was not uncovered in a controlled excavation. It has no presumption of authenticity, which means that the philologist must measure it against known forms and not do somersaults in order to make sense of it as written. Such an inscription is not a secure source of authentic data and therefore a prime text for philological analysis but rather a suspect specimen for philological assessment. As has been demonstrated independently by a number of scholars, the Jehoash inscription violates the norms of ancient Hebrew language and writing as they are known in over a dozen instances. That is more than enough to make the likelihood of its authenticity virtually nil.

The peculiar words and constructs that are found in the Hebrew of the so-called Jehoash inscription conflict with what is known from ancient Hebrew. Arguments from other Semitic languages, or from later forms of Hebrew, contending that a form in Jehoash may be analogous to a form in some language other than ancient Hebrew, are therefore inappropriate. In the end, it does not matter how many forms in the suspected inscription are possibly correct. It only matters that with so many forms contrary to the norm, and the fact that the inscription has no presumption of authenticity, the inscription should not be reasonably considered authentic. The only reasonable conclusion is that it is not. Once one entertains the possibility that the inscription was forged, all the oddities can at once be explained—including an unexpected reference (to the “wilderness”) that has been shown (by Eph'al) to be based on a misreading of a Biblical text (2 Chron. 24:9).

Close to 75 percent of the language of the inscription (and more if one counts substitutions like *'asa bedeq habbayit* for *hizzeq bedeq habbayit*) is virtually identical to language found in the Biblical text. This fact alone suggests that the language of the inscription may have been lifted directly from the Hebrew Bible.

The so-called Jehoash inscription is formulated as a royal inscription. Scores of royal building inscriptions are known from the ancient Semitic world, including first millennium inscriptions in Moabite, Phoenician, and Aramaic. The genre of royal inscription, whose overall structure is remarkably uniform over geographical and chronological distances, is one of the last literary types in which one would expect to find deviations from the norm. By my most conservative count, there are well over a dozen errors or problematic expressions or writings in Jehoash.

Here I shall review only a few of the most outstanding deviations from literary and linguistic norms that one finds in the Jehoash text. Mesopotamian royal building inscriptions tend to conclude in one of three ways: (1) with a reference to the building itself (e.g., the Old Babylonian inscription of Samsuiluna 8 [references are to the Royal Inscriptions of Mesopotamia series]; Hammurapi 7, 12, 17); (2) with a blessing for the king, the builder (e.g., Samsuiluna 7; Hammurapi 2); or (3) with a curse upon anyone who would do damage to the building and/or the inscription (e.g., Hammurapi 11, 1001; Yahdunlim 1 [the cone], 2 [the disc]). West Semitic royal building inscriptions are similar. Unfortunately, both the Moabite inscription of Mesha and the Aramaic inscription from Tel Dan are broken off at the end. However, Phoenician and/or Aramaic inscriptions attest the same three types as the Mesopotamian inscriptions, for example:

1) Barrakab (Old Aramaic), Pyrgi (Phoenician, ca. 500 BCE), Tema (mid-5th century Aramaic), Umm el-'Ahmed (near Tyre, 222 BCE);

2) Yehimilk, Abibaal, Elibaal, Shipitbaal, Baalshillem (all Phoenician);

3) Ahiram, Cyprus grave, Kilamuwa, Azatiwada, Yehawmilk (Phoenician); Zakkur and possibly Panamuwa (Old Aramaic).

The Jehoash inscription concludes with an invocation of a blessing for the people from the Israelite god YHWH. It differs from every single known royal building inscription. The only ancient Semitic royal building inscription that includes a blessing for the “land” (ארץ)—but not, as in Jehoash, for the “people” (עם)—is the Tel Miqneh-Eqron inscription. One must be suspicious of this coincidence: the only other inscription from the ancient Near East in which one finds a blessing for the land or population is one that was only recently discovered. Even so, the Tel Miqneh inscription only invokes a blessing for the land after invoking a more elaborate blessing for the royal builder. In other words, the Tel Miqneh inscription does not deviate from the overall structure of royal building inscriptions; only Jehoash does.

Moreover, the language of the blessing by which the Jehoash inscription concludes is incorrect in its syntax (see section 4 of the handout at the end of this paper). The syntax of the inscription is: “May YHWH command his people with a blessing” (line 15). Not only is the sentiment nonsensical (How can God command people to be blessed?); the syntax distorts the sensible formula that is found three times in Biblical Hebrew (Leviticus 25:21; Deuteronomy 28:8; Psalm 133:3). The proper syntax is: God commands a blessing to the people. The phrase “with a blessing” is found in the Bible only once (Psalm 109:17), and not in connection with a blessing formula. Where do we find the phrase “with blessing” (בברכה) used as a commonplace? In modern Hebrew (see the handout and compare the formula for concluding a letter, in the sense of “Yours truly”). This fact will shed light on the origins of the inscription, if it can be conjoined with other linguistic data found in the inscription that would seem to support a similar conclusion.

The deviation from known Hebrew that has been most often observed in the Jehoash inscription is the replacement of the Biblical expression for repairing a cracked building or foundation—“to strengthen the fissure of the building” (חזק את בִּדְק הַבַּיִת)—with the phrase “to make the fissure of the building” (עֲשֵׂה אֶת בִּדְק הַבַּיִת; see handout section 1). The phrase [emphasized by Avi Hurvitz at the conference to function as a technical term] occurs throughout the Biblical passage concerning the repairs to the temple performed under King Jehoash (2 Kings 12) and is used in the report of King Josiah’s effort to repair the temple as well (2 Kings 22:6). The phrase found in the so-called Jehoash inscription, “to make the fissure of the building,” is deviant.

My learned colleague and friend Chaim Cohen argues that “to make the fissure” can in Biblical Hebrew refer to “rebuilding” the fissure, adducing usages from Hebrew *’aša* and from Akkadian *epēšu* in support. This is not the place to give a comprehensive critique of Cohen’s argument. I find it entirely misconceived, and I will offer here a few representative criticisms. First, Cohen wants to compare the Jehoash text to Mesopotamian, rather than West Semitic, inscriptions. Since, as I have shown above, the Jehoash text deviates in form in a major way from ancient Semitic—including Mesopotamian—building inscriptions, the basis for a strong comparison is weak. In West Semitic inscriptions, the concept of “rebuilding” is expressed by *bana* (בני) and not by *’aša* or *pa’al*, which are used only of making something in the first place (see, e.g., the Phoenician inscriptions of Yehimilk and Shipitbaal and the Old Aramaic inscriptions of Zakkur and Panamuwa). In the handout (section 2) I give several examples from the Moabite inscription of Mesha. There the rebuilding or restoration of towns and buildings is expressed consistently by *bana* while the construction of something new, like a reservoir or a road, is conveyed by *’aša*. This is made clear in the example from Mesha lines 26-27, where a high place said specifically to have been in ruins (כי הרס הא) is repaired by using the verb “(re)build” (בני) and not *’aša*, the verb in Jehoash that Cohen wants to render as “rebuild, repair.”

In Akkadian the verb *epēšu* does not alone indicate the making of repairs, as Cohen suggests. In the text of a Neo-Assyrian inscription adduced by Cohen in his paper, the verb *epēšu*, “to make or do,” is combined with the object *dullu*, “work,” whereas the fissure that needs to be repaired as part of this work—*batqu*, cognate to Hebrew *bedeq*—is not “made” but “bound together,” using the verb *kašāru*. In other words, in Akkadian, as in Hebrew, one never “makes” a fissure as an act of repair; one “strengthens” it (in Hebrew) or “binds (it) together” (in Akkadian). The act of “doing the work” is indicated in the same Biblical texts where one finds the repair of the fissures (2 Kings 12 and 22) with the phrase עֲשֵׂה מְלָאכָה, the proper semantic equivalent of Akkadian *dulla epēšu*, “to do the work.”

Hebrew *’aša* does not by itself indicate “to repair, renovate,” as Cohen claims. Consider his case of Jeremiah 3:16, for example. There the verb *’aša* in the Nif’al (passive) conjugation is taken by Cohen to mean “to (be) reconstruct(ed).” Cohen’s interpretation is not generally held. Both the New Jewish Publication Society *Tanakh* and the New Revised Standard Version render “to make”—to construct something that does not currently exist (the Ark), and not to reconstruct an existing one. This usage does not, therefore, provide an analogous usage to the one alleged to be found in the Jehoash inscription.

Cohen also claims that the phrase “to make a fissure” (*bedeq*) can be assumed to be good Biblical Hebrew because the cognate verb *badaq* means “to repair” in 2 Chronicles 34:10. If the verb *badaq* can mean “to repair,” so the argument goes, then the cognate noun *bedeq* can mean “repairs” and the phrase “to make *bedeq*” can mean “to make

repairs.” The argument does not hold water because it is only in idiomatic English translations (and in the leading Biblical dictionaries, which render idiomatically) that *badaq* means “to repair.” Literally, *badaq* in the Chronicles passage means “to go into the fissure,” which is the first step in the act of repairing the fissure. Chronicles contains the sequence: “to go into the fissure (לבדוק) *and* to repair (lit., strengthen, ולחזק) the building (הבית).” A careful examination reveals that in Chronicles, as in Kings, the verb that is used of repairing the fissures is “to strengthen,” the verb that should have been employed in the Jehoash inscription but tellingly was not. (See the examples from Chronicles in section 1 of the handout.)

Finally, Cohen pulls a sleight of hand in adducing the evidence of Nehemiah 6:1. There we find the phrase “(re)build a wall” (בנה חומה) in proximity to the noun פרך, “breach (in the wall),” which has a meaning similar to that of *bedeq*, “fissure.” Cohen wants to create the illusion that in Nehemiah we have the expression “to construct a breach” in the sense of “to repair a breach,” and wants to argue, by analogy, that if one can say “to construct a breach” in Biblical Hebrew and mean “to repair a breach,” then one can say “to construct a fissure” in Biblical Hebrew and mean “to repair a fissure.” The argument is false because the key phrase “to construct a breach” in the sense of “to repair a breach” does not exist in Nehemiah or anywhere else.

Here, as in many of Cohen’s other arguments, he constructs a hypothetical parallel and claims to find an analogous form in the Jehoash inscription. Hypothetical parallels are no more than unattested possibilities. They exist in theory but may never have existed in reality. I find it entirely improper to prefer a hypothetical parallel to actual evidence. That is exactly what Cohen is doing in arguing for the acceptability of “make a fissure” in the sense of “repair a fissure” in the Jehoash text. We know how to say “repair the fissure” in First Temple Hebrew: one says “to strengthen the fissure,” to tighten it up. It is attested with Jehoash and with Josiah. There is no evidence at all, in any ancient Semitic language, of an expression “to make a fissure” in the sense of “to repair a fissure.”

To take another of Cohen’s examples: he justifies the form גרעה, allegedly “recess,” in the Jehoash inscription even though the attested Biblical form is מגרעת. He argues that since there are some other Biblical nouns having two forms, one with preformative *mem* and one without, there is no reason that ancient Hebrew could not have had a גרעה alongside a מגרעת. Again, the argument is purely hypothetical. Many things that are possible never occur. There is no reason to think that in the case of מגרעת there would have also been a form גרעה.

Here is another case in point. In the Jehoash inscription, lines 13-14, the phrase “as testimony, as a witness,” is לעדת—*le’edut*. The problem is that in early Biblical Hebrew, the Hebrew that the Jehoash inscription is supposed to reflect, the word *’edut* never means “testimony”; it only means “covenant”—it is more or less synonymous with the term *berit*. Thus, the Ark of the Covenant is known here as *’aron habberit* and there as *’aron ha’edut*; the two tablets of the Covenant are known here as *luhot habberit* and there as *luhot ha’edut*. There are no exceptions. The only way to convey the concept of “testimony” in First Temple Biblical Hebrew is by using the term for “witness” in an extended sense, either in the masculine (*’ed*) or in the feminine (*’eda*).

Cohen argues that the abstract noun *’edut* could have existed in early Biblical Hebrew because the same abstract form of the noun is used for the term “testimony” in Akkadian: *šībūtu* from *šībū* “witness.” Again, this argument by hypothetical analogy is inappropriate. First, Akkadian employs the abstract ending *-ūtu* very broadly, while it is rare in early Biblical Hebrew. For example, in early Biblical Hebrew one finds very few

reliable instances of *malkhut*, “kingship,” while one finds this form dozens of times in Late Biblical Hebrew. The morph *-ut* is characteristic of later Hebrew, not earlier Hebrew. Second, Cohen is again pitting a hypothetical form against an attested form and favoring the former. If the Jehoash inscription were provenanced, then one would conclude that we have in it the earliest usage of *‘edut* in the sense of “testimony” in Hebrew. However, since it is not provenanced and must accordingly be treated with suspicion, one should measure the usage of *‘edut* against what is known from ancient Hebrew. In ancient Hebrew the term for “testimony” is *‘ed(a)*. Since the Jehoash inscription has *‘edut*, we must suspect that it is inauthentic.

The arguments of Cohen and others in behalf of the authenticity of this or that usage in the Jehoash inscription are entirely ad hoc. That is, each problematic feature or form is given a unique explanation, unrelated to the explanation of any other feature or form in the text. This type of argumentation runs against accepted norms of scientific method since at least the Middle Ages and the famous razor of William of Occam. In general, a theory is to be preferred if it explains a variety of phenomena in a single, simple, economical way.

Here are some of the facts concerning the language and form of the Jehoash inscription as I understand them. The inscription deviates from all other building inscriptions known from the ancient Semitic world. The inscription’s language is at least 75 percent identical to language in the Hebrew Bible. There are over a dozen deviations from known expressions and writings in ancient Hebrew, and these in a text type that is supposedly as conventional as they come—a royal building inscription. While each one of these deviations might have been possible, the cumulative weight of even a fraction of them makes the authenticity of the inscription virtually nil. In each of the cases of linguistic deviation I have mentioned, and in others I could have mentioned, the form we find in Jehoash resembles expressions that are known from later, and sometimes only modern, Hebrew. Thus, *bedeq habbayit* in the sense of home repairs is not attested until at least the Hellenistic period and is very familiar from modern Hebrew. The term *‘edut* for “testimony” is late Hebrew and routine in modern Hebrew. The phrase *bivrakha* “with blessing” is commonplace in modern Hebrew.

One theory alone can explain all of the unusual phenomena that have been enumerated, alongside the numerous resemblances between the language of the inscription and the material of the Hebrew Bible. Someone who knows modern Hebrew and does not know Biblical Hebrew in a trained or controlled way has composed the inscription by making extensive use of the Bible. This theory is of a piece with Eph`al’s explanation of the odd reference to collecting tribute to the temple in the wilderness, which is based on a misreading of 2 Chronicles 24:9, which does not say that temple donations were collected from the wilderness but rather that Moses had collected donations from the Israelites while they were in the wilderness. Philological examination both establishes that the Jehoash inscription is inauthentic First Temple Hebrew and suggests that the text is a modern composition.

Handout

2 Kings 12:6: והם יחזקו את בֵּדֶק הַבַּיִת

2 Kings 12:7: לֹא יִחְזְקוּ הַכֹּהֲנִים אֶת בֵּדֶק הַבַּיִת

2 Kings 12:8: מִדְּוַע אֵינְכֶם מִחְזְקִים אֶת בֵּדֶק הַבַּיִת

ולבלתי חזק את בדק הבית: 2 Kings 12:9
וחזקו בו את בית ה': 2 Kings 12:15
כסף לחזק את בית אלהיכם: 2 Chron. 24:5
לחזק את בית ה': 2 Chron. 24:12
לחדש (את) בית ה': 2 Chron. 24:4, 12
ואעש את בדק הבית: Yehoash 9-10

ואעש הבמת זאת: Mesha 3
ואבנ את בעלמענ ואעש בה האשוח: Mesha 9-10
ומלכ ישראל בנה את יהצ: Mesha 18-19
אנכ בנתי קרחה חמת היערנ וחמת העפל ואנכ בנתי שעריה ואנכ בנתי מגדלתה ואנכ...
בנתי בת מלכ ואנכ עשתי כלאי האש[ו...]. ואמר לכל העם עשו לכמ אש בר בביתה...
אנכ בנתי ערער ואנכ עשתי המסלת בארננ אנכ בנתי בת במת כי הרס הא...: Mesha 26-27

הגל הזה עד ביני ובינך: Gen. 31:48
והיה לעד ביני ובינך: Gen. 31:44
כי את שבע כבשת תקח מידי בעבור תהיה לי לעדה כי חפרתי את הבאר הזאת: Gen. 21:30
למען תהיה לי השירה הזאת לעד בבני ישראל: Deut. 31:19
כי עד הוא [= המזבח] בינינו וביניכם...: Josh. 24:27
ביום ההוא יהיה מזבח לה' בתוך ארץ מצרים, ומצבה אצל גבולה לה', והיה לאות ולעד: Isa. 19:19-20
לה' צבאות בארץ מצרים
לא תענה ברעך עד שקר (שוא): Exod. 20: 16 (cf. Deut. 5:17)
אם טרף יטרף יבאהו עד: Exod. 22:12
והיה הימ הזה לעדת: Yehoash 13-14

וצויתי את ברכתי לכם בשנה הששית: Lev. 25:21
יצו ה' אתך את הברכה באסמיק...וברכך בארץ אשר ה' אלהיך נתן לך: Deut. 28:8
כי שם צוה ה' את הברכה...: Psalm 133:3
יצו ה' את עמו בברכה: Yehoash 15
ויאהב קללה ותבואהו ולא חפץ בברכה ותרחק ממנו: Psalm 109:17
בישראל מקבלים בברכה את וורן באפט: Contemporary Hebrew (www.google.co.il)

Examining the Authenticity of the Yehoash Inscription In Light of its Linguistic Profile

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One of the most interesting expressions to which particular attention has been paid in studying the language of the Yehoash Inscription [=YI] is *'asah + bedeq bayit*. *Bedeq bayit* is an architectural term attested 7 times in the book of Kings, always in descriptions relating to repairs and/or restorations carried out in the Jerusalem Temple. The word *bedeq* is generally understood as *breach*, *fissure* or *damage*; and these meanings of the Hebrew word are firmly established by corresponding forms in Akkadian.

The term *bedeq bayit* is governed in the YI by the verb *'asah* – in contrast to the corresponding story in 1 Kings, which in this connection consistently employs the verb *hizzeq*. The otherwise far-reaching agreement – in both contents and wording – between the inscription and the biblical account of the repairs/renovations of Yehoash in the Temple is regarded by those who maintain a First Temple Period dating of the inscription as decisive evidence that the version of the inscription is authentic. How, then, should one account for the striking deviation of the phrase *'asah + bedeq bayit* from the consistent biblical usage of the technical idiom *hizzeq + bedeq bayit* in the book of Kings? Any attempt to dismiss the interchange of *'asah* and *hizzeq* in terms of insignificant modification must be rejected, since we are dealing here with strict technical terminology which cannot be explained away as trivial variation.

The appearance of linguistic irregularities in newly discovered inscriptions is a well-known phenomenon in the world of epigraphy. Yet, at the same time, it is impossible to turn a deaf ear to linguistic anomalies which clearly violate the traditional norms of a well-documented linguistic usage. It is clear that *'asah + bedeq bayit* indeed constitutes such an anomaly whose antiquity is highly suspicious – all the more so since it is a common idiom in Modern Hebrew. This idiom, therefore, may well be taken as a “slip of the pen” by a forger whose native language is Modern Hebrew. Personally I believe, however, that only if and when the existence of additional potential cases of anachronistic nature are *definitely* established in the text – thus attesting to a significant *accumulation* of marked features indicative of later times – only then will we be safely entitled to reject the authenticity of YI on linguistic grounds.

Can Rock Patina Be Forged? How to Recognize Forgery?

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Yes, rock patina or weathering deposits and other traces and surface changes created by age and long periods of exposition to atmospheric environments can be imitated. Whether or not such adaptations of new material or fresh surfaces to the outcome of hundreds if not thousands of years of ageing can be recognized and differentiated from the true processes of ageing and patina formation is a wide open question. Many different scientific tools and discrimination techniques are needed in order to identify true ageing and separate it from later deliberate falsifications of surfaces. It is also easier to create a fresh patina on places or rock surfaces freshly hewn from new blocks of stone. Two examples will be shown. The first one shows old blocks with patina in architectural context with new blocks without patina. The second one demonstrates how vandalism (strong cleaning) can be hidden for the first glance by new pseudo-patina additions by restorers.

Recently the author had the opportunity to examine two inscriptions engraved in stone and a stone lamp, in an independent manner, using multi-disciplinary scientific techniques, and to assess when they were produced, especially on the basis of the examination of their patina (more specifically, whether they or at least parts of the patina were produced recently, or several hundreds of years ago). The tests and analyses were conducted in Jerusalem and at the University of Oldenburg, Germany. Samples were also re-tested in mineralogy laboratories in Hannover with advice from the Crystallography Institute of the University of Würzburg, both in Germany. A complete report was published in November 2005 and accompanied by documentation of these tests including photographs.

In summary: In contrast to organic materials (which can be dated using the Carbon-14 dating technique), or clay artifacts (which can be dated using thermo-luminescence techniques), there is no currently available technique to date the manufacture, engraving or processing of stone artifacts. Existing scientific tests may, at most, cast doubt on the authenticity of such items, or provide evidence that reinforces the probability that the items are ancient e.g. by multilayered and structured Patina grown with time. Tight adhesion and intergrowth with the substrate or loose contact to the substrate, however, is not an indicator of age (or forgery). Both types of patina can occur with natural long time growth or fast artificial patina production.

According to published reports, doubt arose about the origin of inscriptions and ornamentations on rock materials (ossuaries, tablets and lamps made of stone). It was even suggested that forgery action was involved. The arguments forwarded by the negative expertise are, however, not conclusive and at least partially inconsistent. Several arguments for and against serious later changes of these and other objects were critically discussed. However, as stated above no unequivocal evidence could be given on the authenticity of the inscriptions, the patina on them and on the ornamentations and their patina. Some arguments are given, how high age and authenticity of any given patina could be documented. The full lecture also suggests other techniques of analysis aiming at a full and convincing picture and differentiation between "old" or naturally grown patina and "new" or freshly (artificially) prepared surface changes.

Jerusalem Forgery Conference

André Lemaire

I. James Ossuary Inscription (A. Paleography)

Summary

The James ossuary inscription does not present any problem from the point of view of the paleography.

As far as I know them, the objections that were raised against the authenticity of the paleography of the inscription are not serious:

1. The first objection is that it was written in two parts because of the final shape of the P but it is well known that the shape of final letters does not mean that these letters are at the end of an inscription but at the end of a word.
2. The second objection is that the first part is written in formal script and the second part in cursive, but it is simply not true. If we use the abbreviation *f* for formal letter and *c* for cursive one, we have the sequence : *cffcffccffcfccccfcf*. That means that we have a mixture of formal and cursive shapes, a well known phenomenon in ossuary inscriptions.

Two other objections raised from the orthograph are also not serious :

1. The spelling Y'QWB (with waw) is already attested on, at least, three ossuaries (Rahmani 104, 396, 678)
2. The spelling 'HWY D is already attested in Rahmani 570 and in Qumran.

Furthermore, more generally, as is well known, the 'objection' TGTB ("Too good to be true") is also not a serious objection.

Finally, since there is no problem in the authenticity, reading and translation of this inscription. The only problem of this inscription is an eventual problem of identification. Is the person named on the ossuary the same as the homonym person known in the literary tradition (New Testament, Flavius Josephus, Eusebius of Caesarea...)? This inscription does not reveal the existence of a new historical personage and, in a way, it is not that important for an historian who already knows the New testament, Flavius Josephus... ! Furthermore, it is only a problem of probability.

This problem of identification should not be connected with the problem of authenticity of the inscription.

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II. Ivory Pomegranate Inscription (A. Palaeography)

Summary:

As far as I know, no serious objection has been raised against the *palaeography* of this inscription: "... there seems to be very little to add to the thorough discussion of Avigad... Almost all the letters can be compared to other ancient Hebrew Inscriptions of the eighth-seventh centuries BCE. The only problematic letter is the curious *mem* with the W-like head. This, however, might have been caused by a slip of the engraving tool on the hard surface of the pomegranate's shoulder, as well as by its small dimensions" (*IEJ* 55, 2005, p. 13).

In connection with palaeography, however, an objection has been raised because there is no word divider and the inscription seems to present spaces between the words (instead of word dividers). Actually we have here two different problems:

1. It is clear that there is no apparent word divider (a dot or a short vertical stroke), however the use of word-dividers is not absolute as recognized in *IEJ* 55, 2005, p. 14 "Some ancient Hebrew inscriptions inscribed on ostraca or engraved on seal overlooked the rule, writing *scriptio continua*". The exceptions to this 'rule' are so numerous, especially when the inscription is small and incised or engraved (see WSS 2, 8, 35...), that the absence of word dividers is not really a problem in ancient Hebrew epigraphy. Actually this lack of word dividers is frequent at the end of a line, as is the case after [YHW]H and after KHNM (see *infra* syntax), or, in the case of a construct state, as between BYT and YHWH (in any event, not visible because of the break!) and between QDSh and KHNM.

2. The second remark--that there are spaces instead of word dividers--is not true. If we put aside the larger spaces between KHNM and LBYT and between [YHW]H and QDSh because they are easily explained by the syntax (*infra*), the other spaces between letters seem to vary between 1.25 mm and 0 mm and such differences in spaces between letters are not unknown (see again WSS 2, 8, 35...): they do not mean that the larger spaces take the place of a word divider.

Another objection seems to have been raised from the 'syntax' of the inscription "The syntax of the inscription is awkward": *IEJ* 55, 2005, p. 13, even though, finally, a reading LBYT YHWH, QDSh KHNM is offered as an 'alternative'... which is exactly the syntax that I proposed in *RB* 1981, p. 236. Thus, there is no problem at all in the 'syntax' of this very short inscription.

From *IEJ* 2005, p. 9ff., the main problem raised against the authenticity would be that the engraving of certain fragmentary letters stops before reaching the old or new breaks. Unfortunately a new examination with a stereomicroscope revealed that it is not true and it confirmed the conclusion of the previous examinations by epigraphers (myself and Avigad). (For other points and a detailed argumentation, see *IEJ* 2006/2)

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III. Yehoash Inscription

Summary:

From the point of view of *paleography*, the Yehoash inscription is problematic under various aspects:

1. A general look at the inscription gives the feeling of a somehow hesitant and irregular engraving (Z small at l. 2 and normal at 8, *sadé* lines 5 and 8...), which is surprising in a royal inscription.
2. The writing is difficult to classify: some of the letters look Hebrew and other ones Phoenician or Aramaic: for instance ' , Y, P with 'round' and not sharp angles, B is slanting down towards the right (instead of vertical or leftwards).

The *spelling* of the third personal singular suffix is surprising since we expect rather 'MH instead of 'MW (last line). However -W would be possible if 'M is plural (but that is contextually unlikely) or, else, could be compared to R'W (sing. or plural?) in the Siloam inscription (lines 2 and 3).

From the *linguistic* point of view, several phrases look problematic: 'SH + BDQ; NML'H NDBT ...

The *general presentation* of the inscription with a so regular lower frame does not seem to have parallels among the IX-VIIIth c. monumental inscriptions. The engraving with a broad chisel looks also very rare (However see Ophel: *IEJ* 1982, p. 194 ... but on soft limestone ...).

How to explain all these problematic aspects? That is the question with several possible answers/scenarios:

1. The inscription is a genuine inscription from the end of the IXth BCE but engraved by an inexperienced engraver. The problematic aspects are to be explained by the fact that we have no contemporary monumental Hebrew inscription and only very few and short Hebrew inscriptions from the IXth c. (Cf. the Moabite inscription from Khirbet el-Mudeiyineh ZDPV 2000, pp. 1-13; if, appearing on the market, this inscription would probably have been declared a forgery by most of the epigraphers, included myself).

2. This inscription is an ancient copy of an original one (hence the 'mixed' script forms and linguistic features), eventually during the IVth-IIInd c. BCE, if this corresponds to the radiocarbon age of the patina.

3. This inscription is a modern forgery connected with the Shapira affair (c. 1870-1884). More precisely, it would have been the work of the association Shapira-Selim el-Qâri (the author of the famous *Moabitica* cf. Ch. Clermont-Ganneau, *Les fraudes archéologiques en Palestine*, Paris, 1885, pp. 107ff., 152ff. 159, 173) and Martin Boulos "by profession, a stone cutter and engraver of tombstone" (cf. *Fakes and Forgeries From Collections in Israel*, Eretz-Israel Museum, Tel Aviv, 1989, p. 20*).

4. This inscription is a contemporary learned forgery, eventually suggested by the publication of N. Na'aman's paper: "Royal Inscriptions and the Histories of Joash and Ahaz, kings of Judah", *VT* 48, 1998, pp. 333-349, esp. pp. 337-340, see p. 337: "... on the basis of an original inscription".

IV. Moabite Inscription

Summary:

From the point of view of *paleography*, there is no problem; it seems to fit exactly to the Moabite shapes of letters. The engraving seems clear without hesitation, following the model of a fluent scribe.

The presentation on an octagonal column seems original and without parallel for the royal inscriptions of this period but there is no reason why it would be a problem since it is to be read on only three faces + one letter on the fourth face.

Even though there are a few problems in filling the lacunae, there does not seem any *linguistic* problem and some of the sentences are similar to, but not the same as (not a copy) the ones read in the Mesha stele.

There is not reason to doubt that it is genuine.

One notes an interesting phenomenon:

J.A. Emerton wrote: "The text has no direct bearing on any person or event mentioned in the Bible. That fact, however, may perhaps be regarded as reassuring, when one remembers recent occasions when texts apparently having such references have been claimed to be forgeries--and such an allusion is likely to be an advantage to a forger wishing to sell his alleged discovery. In any case, I am not aware of any reason to regard this newly-discovered inscription as other than authentic" (p. 293).

I agree completely with the conclusion but, although nuanced, the remark about the absence of relation with the Bible as a sign of authenticity is disputable and only reveals the contemporary atmosphere of forgery mania.

Actually this inscription may well have some connection with a few enigmatic enough verses of the Bible: Hosea 10:14, 2 Kings 14:25.28, Amos 6:13-14 and this connection as well as an Assyrian text reveal that the author of this inscription is probably "Shalmân/Salamanu" (see Lemaire *CRAI* 2005).

Thus, now, the question is: should we consider now this inscription a forgery since it has some Biblical connection? Is the fact of proposing a Biblical connection for an inscription a sign that this inscription is a forgery? We are apparently faced with the same problem as with the James ossuary!

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V. Moussaieff Ostraca

From the point of view of *paleography*, the two ostraca published by P. Bordreuil *et al* are problematic:

1. Both ostraca seem to have been written by the same 'scribe'.
2. The general aspect of the writing does not give the feeling that it was written by a fluent scribe. It seems that there is some hesitation in writing, especially in ostracon 1, and each letter seems to have been written individually.
3. Several letters have strange shapes for a Paleo-Hebrew ostracon, for instance the rounded head Y, the head and the small size of the B, the H with the second or the third stroke longer than the other ones... the somehow flat Sh... the upper stroke of the P written diagonally...

Furthermore the *formula* of ostracon 1 is strange. It seems to be an order but without address ('L + P.N.). It begins with K'ShR, and this is apparently without parallel among Hebrew ostraca. Ostracon 2 begins with a blessing but without the name of the addressee. Although the following YShM' 'DNY HShR is parallel to the Mezaḏ Hashavyahu ostracon, one may have expected: YBRK YHWH 'DNY HSR BShLM...K...

How to explain these peculiarities, At least two explanations can be offered:

1. The ostraca are modern forgeries.
2. The ostraca are not true messages (or pleas) but exercises by apprentice scribes.

To choose between these two possibilities, one should like to get some help from the *material analysis*. Unfortunately the situation is not very clear: 1) the analysis of the surface seems to present strange or spurious aspects, and 2) the analysis of the ink seems in favor of the authenticity.

Since the surface of the ostraca, transmitted through various hands with unknown handlings, may have been contaminated and transformed through these various handlings, the result of the ink analysis seems to be more trustworthy (that is the reason why I proposed interpretation 2 in Lemaire 1999). However I am not an expert in the material analysis and I should like that this problem should be discussed in detail (with the detailed results of the various [five?] material analysis).

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VI. Methodology. Functions and Responsibilities of the Academy

Summary :

The problem of forgeries is not a new problem and it must always be considered seriously. Thus Renan already wrote in 1874:

"Les faussaires menacent de causer bientôt tant d'embarras aux études d'épigraphie et d'archéologie orientales qu'il faut placer au nombre des plus signalés services celui de démasquer ces sortes de fabrications".

There are various kinds of forgeries and one could propose a typology of the fakes.

On one hand, the real danger is with the forgeries that are published by scholars as genuine inscriptions.

On the other hand, there is also a danger of classifying as forgeries genuine inscriptions and, since each inscription is often unique, it may be something lost for historical research for ever.

A few orientations may help dealing with this problem:

1. Primacy of the inscriptions found during controlled excavations. As far as possible, the knowledge of *all* these inscriptions (published and sometimes unfortunately still unpublished) is important before dealing with non-provenanced inscriptions
2. As far as it is possible, it is important to try changing the status of non-provenanced inscriptions into a status of probable or certain provenanced inscriptions, either, for genuine inscriptions, with the organization of salvage excavations (cf. Qumran, Wadi ed-Daliyeh, Khirbet el-Qom 1967-71 ...), or, for forgeries, with the identification of the forger.
3. In the absence of any archaeological context, the inscription itself must be examined under its different aspects (paleography, linguistic, general presentation, content, literary genre...) and to compare it to inscriptions found in controlled excavations, taking into account the method of the series in epigraphy, but also the problem of copies/adaptations of genuine inscriptions.

4. As much as necessary, use material analysis to help evaluating the authenticity. However one should be aware of the fact that material analysis needs always to be interpreted for a problem of authenticity. Each method has to be used critically.
5. Generally this epigraphic and material examination by *experienced* and *specialized* scholars is enough to classify the inscription either as genuine or not.
6. There may still be a few doubtful or not completely clear cases in the present state of our knowledge. In these cases, it might be practical to publish them (eventually as “questionable”) with an adapted commentary. Actually it might be useful not using them at all as long as the matter is not clarified, eventually by new discoveries of inscriptions or by new methods of examination ...

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Jerusalem Forgery Conference

Alan Millard

The Jehoash Stone

Epigraphy—A Suspicious Symmetry

When you meet someone for the first time, you receive an immediate impression of them. Your mind places them in a certain category, taking account of various features which are apparent. If you come to know that person more closely, your first impression may change, it may be strengthened or it may be altered, or it may be completely changed—your first impression may turn out to be a false one. The same is true with many human artifacts, ancient or modern, such as paintings, sculptures, coins and antiquities. First impressions may be subjective, yet based upon experience in from studying the type of object and knowledge of other examples. Detailed analysis and scientific measurements may support the subjective opinions, or challenge them. We sometimes say, 'Second thoughts are best' and we should be ready to revise initial opinions if the case against them is strong.

When in September 1979 I saw the Tell Fekheriyeh Statue in Damascus Museum, my first impression was, Can this really be authentic? The statue is almost perfectly preserved and the inscriptions are in almost pristine condition. During the twenty minutes available before I was due to take the plane home, I concluded that it is a genuine antiquity. The Assyrian and Aramaic inscriptions convinced me; no one in the 20th century could have created those texts. The unique features of the Aramaic script and language do not point to a modern forger's hand, but to a hitherto unknown local fashion. I shall return to the Tell Fekheriyeh Statue for lessons and analogies more than once!

When the first photograph of the Jehoash Stone became available, my immediate impression was, 'Fake!' There is something about the script which does not ring true. It is not easy to quantify and perhaps giving too much detail will help a future forger to hoodwink us all! The first oddity is the forward leaning *beth*, an obvious anomaly in early Hebrew epigraphy, as several colleagues have remarked, but at home in Aramaic and Phoenician. In his palaeographical examination of several letters, Chris Rollston has pointed to shapes that should not appear in a late ninth century Hebrew monument but belong to later times, or, like the *beth*, to other script traditions.²¹ Here the famous seal of Jeroboam's servant may be noted, for it has two examples of the letter *beth*, one the expected Hebrew form, the other apparently Aramaic or Phoenician. However, it is almost certainly not to be treated as such, but rather as a result of the awkward space available to the engraver.

I have not made a comprehensive study of the script of the Jehoash stone, I only want to draw attention to one matter, the forms of the letter *he*. The examples of *he* in lines 1, 2, 3, and 5 caught my eye at first sight of the photograph. There are small variations, yet there is a far greater regularity than one sees in other inscriptions. The bars are almost exactly parallel; only the lower bar occasionally bends slightly upwards (6.1; 9.2; 12.3) or downwards (2.1; 12.2; 15.3) and the central bar is very slightly out of parallel with the other two in 2.1;10.1;10.2. And in 14.1 the lower end of the vertical was apparently added after the bars had been engraved. Compare the letter *he* in the Aramaic Tel Dan

²¹ C. Rollston, 'Non-provenanced Epigraphs I: Pillaged Antiquities, Northwest Semitic Forgeries, and Protocols for Laboratory Tests,' *Maarav* 10 (2003) 135-93, see 178.

stele fragments from about the same date. There is greater variety in the stance of the 'vertical' and in the length and angles of the 'horizontal' than on the Jehoash stone. In the Tell Fekheriyeh Text, Aramaic and engraved far from Jerusalem, a lengthy inscription on a hard, curved stone surface, there is also a greater fluidity in the stance of the letter *he*. The vertical is frequently almost truly vertical, often leans to the left, sometimes markedly so (3, 12, 16, 21). The bars vary in length, angle and spacing considerably within this one specimen. By contrast, on the Jehoash Stone, the *hes* seem very stilted; notice how the horizontal bars very exactly spaced at a virtual right angle to the vertical of each one. There are small variations, yet there is a far greater regularity than one sees in other inscriptions. The unexpected regularity of the *hes* is emphasized by putting them beside the *yodhs* which show much greater variation in shape, the stance is mostly right leaning, but some examples are almost, or fully vertical (10.1; 11.1; 13.1; 14.1). The angle at the top is almost a right angle in the latter, but a more acute angle in some others and simply a curve in 1.1 and 5.1. In my opinion, the engraver formed his *hes* with the aid of a template, a stencil, or a school geometry set! I do not see the flow of script that one expects in the work of an ancient scribe tracing the text on a stone surface for engraving by himself or by a mason.

The other aspect of the Jehoash Stone that has caught my eye is the engraving. What was the tool used to make the letters? In most ancient West Semitic inscriptions the strokes are less evenly cut. Usually the end of a stroke tapers slightly, as in the inscriptions from Tell Dan, 'Amman and Kerak, whereas the strokes on the Jehoash Stone are noticeably even, only a few end in a tapering stroke. From the photographs, they appear to have been made with a gouge of some sort, rather than a sharp point. The closest comparison seems to be with the fragment found in a Byzantine level on the Ophel area in 1982. In that instance, however, the letters were more deeply cut with a narrow, square-ended chisel.²² Notice, too, the variation in the form of the two examples of the letter *he* on that fragment. While the method of engraving the Jehoash Stone is not demonstrably wrong for ancient Hebrew, it is abnormal and so arouses suspicion.

My brief palaeographic observations may be of minor value; they were the first reasons that led me to doubt the antiquity of the Jehoash Stone and they suggest that it may be better termed Ye Hoax Stone.

The Jehoash Stone

The Question of the *mater lectionis*

The word *lwlm* has been attacked because of the spelling with medial *waw* as well as on linguistic grounds.²³ Frank Cross has stated, 'We would expect in ninth-century spelling *llm* without the internal *mater lectionis*. However, in the eighth and seventh centuries there are rare cases in which an internal *mater lectionis* is used in a one syllable word.'²⁴ While Cross' observation is true, the paucity of Hebrew inscriptions prior to about 800 B.C. is relevant, as D. N. Freedman has pointed out²⁵. No formal examples are available

²² J. Naveh, 'A Fragment of an Ancient Hebrew Inscription from the Ophel,' *IEJ* 32 (1982) 195-98; M. Ben-Dov, 'A Fragmentary First Temple Period Hebrew Inscription from the Ophel' in H. Geva, ed., *Ancient Jerusalem Revealed*, Jerusalem: Israel Exploration Society (1994) 73-75; J. Renz & W. Röllig, *Handbuch der althebräischen Epigraphik*, I (Darmstadt: Wissenschaftliche Buchgesellschaft, 1995) 266-67.

²³ In the light of E. Qimron, 'lwl and blwl', *Leshonenu* 38 (1974) 225-27, the word is clearly *blwl*. See M. Cogan, *I Kings*, Anchor Bible 10, New York: Doubleday (2001) 240.

²⁴ 'Notes on the Forged Plaque Recording Repairs to the Temple,' *IEJ* 53 (2003) 119-22, see 121.

²⁵ 'Don't rush to Judgement,' *BAR* March-April 2004 49-51.

prior to the middle of the eighth century, the Nimrud ivory, only graffiti, as at Kuntillet 'Ajrud, and ostraca. We have to ask if the surviving texts are sufficient to support the argument. The possibility of sporadic *matres lectionis* occurring in ninth century Hebrew texts becomes stronger in the light of their use in Old Aramaic. There *waw* and *yodh* are used in polysyllabic words. The Zakkur stele has *jwr*< (17), the Sefire Stelae of about half a century later have *rw*' (III 2), *byr*< (I B 34), *ymwt* (III 16) and *jybt* (III 24) and, more significantly, though more distant, the Tell Fekheriyeh Statue has several examples, such as *dmwt*<, *tβlwth*, *tnwr* and *prys* (1, 15; 5, 9; 22; 19). The noteworthy aspect for this discussion is the inconsistency of the scribes engraving these monuments. Zakkur has *jr* beside *jwr*< (10, 17), Sefire has many more cases of defective than plene spellings, but no cases of the same word spelt in two ways. Tell Fekheriyeh has <*lhn* beside <*lhyn*, *j>rn* beside *j>ryn*. Similar variations are evident later in Aramaic and also in Hebrew late in the eighth century (e.g. <*rr* in a cave near the Dead Sea versus <*rwr* in the Siloam Tomb epitaph) as well as in cuneiform texts of the second and first millennia B.C.²⁶ With virtually no comparable Hebrew inscriptions of the same date, the *mater lectionis* in *lwlm* on the Jehoash Stone cannot be a decisive factor in condemning it but it may supplement others.

The Jehoash Stone

The Question of the Apparently Anachronistic Expression

Suspicious are raised when an apparently ancient document includes a word or phrase, a grammatical or syntactic form which is not attested for the era when the document appears to have been written. Of course, that is the case with the expression *bdq hbyt* on the Jehoash Stone. I am not going to argue whether or not it is a pre-exilic Hebrew phrase; rather, I shall exemplify the possibilities that should be kept in mind when approaching such a situation.

There are five possible explanations:

1. The document is a modern forgery. Sometimes this is easy to demonstrate, as Joseph Naveh did so well with the 'Philistine Leather Documents' and other things. There are numerous examples of clay barrel cylinders bearing a cuneiform text of Nebuchadnezzar which are made from moulds that have been wrongly put together. (There were some on display in Cairo Museum years ago, supposedly found at Tahpanhes where Jeremiah forecast that Nebuchadnezzar would set up his throne - Jer. 43: 9, and one is in the Clark Collection in Jerusalem). Casts of cuneiform tablets have been made for many years, often with obverse and reverse wrongly aligned. All sorts of fake cuneiform inscriptions circulate in the antiquities market to trap the unwary. There are even fake tablets among those collected by C. J. Rich when he was Resident in Baghdad between 1808 and 1821! Frank Cross has shown decisively that the Paraiba Stone is a nineteenth-century concoction, on the basis of the anachronisms and errors in the script and the language.²⁷

2. The document is an ancient fraud. The British Museum owns an elaborately shaped inscribed stone known as The Cruciform Monument of Manishtusu. It is 22 cm high.

²⁶ A. R. Millard, 'Variable Spelling in Hebrew and other Ancient Texts', *JTS* NS 42.1 (April 1991) 106-15.

²⁷ F. M. Cross, 'The Phoenician Inscription from Brazil: A Nineteenth Century Forgery,' *Or* ns 37 (1968) 437-60, reprinted in *Leaves from an Epigrapher's Notebook*, Harvard Semitic Studies 51, Winona Lake, Ind.; Eisenbrauns (2003) 238-49.

Manishtusu ruled in the Old Akkadian period; he was a son of Sargon of Akkad who ruled Mesopotamia about 2269-2255 B.C. According to this text, he gave land and endowments to the temple of Shamash, the sun god, in the city of Sippar. The Cruciform Monument, however, is not from Manishtusu's time. It is a later concoction, which, one expert believes, 'seems to be based, at least in part, on M's original inscriptions, and thus it is not totally devoid of historical value'.²⁸ Another suggests, 'there may have even existed an Old Akkadian prototype of the Cruciform Monument. If so, then the Cruciform monument could be merely an expanded and colored version of the original inscription'.²⁹ He goes on to argue that the Cruciform Monument is a pastiche from Nabonidus' time, in the sixth century B.C. A major indication of the late date is the order of quantities and measures. Putting the number after the measure is only known in Late Babylonian, from the time of Nabonidus onwards. In the Old Akkadian and subsequent periods numbers the measures.

3. The document is an ancient revision of an earlier one. Babylonian tablets from the eighteenth or seventeenth centuries B.C. tell stories about the great kings of the Dynasty of Akkad, Sargon and Naram-Sin, who ruled some four hundred years earlier. There are even later copies of stories about those kings, too. The accounts those texts give of the adventures of those kings are clearly legendary in some cases – when enormous numbers of enemy hordes attack – yet in other cases they are evidently based on fact, for they agree with the original inscriptions of those kings. Although their value has been attacked, notably by Mario Liverani, further study and discovery gives them greater credibility, as I and other scholars have demonstrated.

4. The document is an ancient copy of an older one. This is a possibility that has been raised in regard to the Jehoash Stone, although I have not seen any real attempt to sustain it. There are examples in cuneiform of very well executed copies of earlier inscriptions, but the fact that they are copies is clear because the scribes who made them added their names and dates. I notice that the re-burial of the remains of King Uzziah in the Herodian period was not accompanied by an attempt to inscribe the tomb slab in archaic script.

5. The document is authentic and adds to knowledge of the script and language of its time.

Almost every example of an ancient West Semitic document discovered has expanded current knowledge of the language in which it is written and often of the script, too. The Moabite Stone and the Siloam Tunnel Inscription are prime cases for Hebrew, Zinjirli monuments for Aramaic and the Byblian inscriptions for Phoenician.

The simple presence of an apparently anachronistic expression cannot be used to prove a text is not what it claims to be without other, clear evidence.

²⁸ I. J. Gelb, *JNES* 8 (1949) 346-48; E. Sollberger, 'The Cruciform Monument,' *JEOL* 20 (1968) 50-70; P. Steinkeller,

Manishtusu, *RIA* 7 (1987-90) 335, cf. *BE* 1.13.

²⁹ M. A. Powell, 'Naram-Sin, son of Sargon: Ancient history, Famous Names and a Famous Babylonian Forgery,' *ZA* 31(1991) 20-30.

The Moussaieff Ostraca

The Matter of Shared Phrases

When Pierre Bordreuil, Felice Israel and Dennis Pardee published the two ostraca, which have come to be known as The House of the Lord ostrakon and The Widow's Plea ostrakon, they noted some phrases which have counterparts in biblical Hebrew and in other ostraca.³⁰ Israel Eph'al and Joseph Naveh listed those and added more.³¹ For Eph'al and Naveh 'such a high degree and frequency of similarity, which can hardly be regarded as accidental, raises serious doubts about the authenticity of the ostraca'.³² In response, Pardee has pointed out that 'the features singled out are like pre-exilic Hebrew because that is how speakers of pre-exilic Hebrew spoke!'³³ This is a cogent argument. When Eph'al and Naveh quote the opening request of the 'Widow's Plea' beside the opening request of the Mesad Hashavyahu ostrakon, 'May my lord the officer hear {the word of} his servant' as one of the similarities which leads to their unease, they do not remark on the biblical parallel, adduced by other commentators, in 1 Samuel 26: 19, 'Now let my lord the king listen to his servant's words'.

The recurrence of phrases in texts dealing with related topics in the same language is to be expected and can be demonstrated repeatedly across the corpora of texts in the various languages of the ancient Near East. We need only note the curses which are shared by the Tell Fekheriyeh Statue, the Sefire Stelae and the Bukan Stele. The Bukan Stele has the curses that seven cows fail to satisfy a single calf and that a number of women bake bread in one oven but not be able to fill it. Now both curses occur on the Tell Fekheriyeh Statue and the first one and, according to S. Kaufman,³⁴ the second one also, on Sefire Stele I, so if we follow the Eph'al and Naveh argument, then we might dismiss the most recently discovered example, the Bukan Stele, as a modern imitation – a conclusion which I do not suppose anyone would accept!

In the same way, the presence of a unique or unusual word need not necessarily be a sign of modern fabrication. The Mesad Hashavyahu ostrakon offers a verb from the base 'sm (line 5), from which only a nominal form is present in biblical Hebrew and the expressions *kyymm* and *zh ymm* (lines 5, 9) which are also novelties. The Tell Fekheriyeh Statue has yielded several words otherwise unknown to Old Aramaic, one, *qlqlt'*, only previously recorded in the language one thousand years later.

On the other hand, there may be good grounds for suspicion, if a text presents an unusual expression which has already become known from another document, such as the sentence in Lachish Ostrakon 4. 3,4, 'I have written on a *deleth* everything that you sent me'. Thus, the closing lines of the 'House of the Lord' ostrakon do fall under suspicion because they are so close to last words of the Tell Qasile ostrakon. The former has 'Silver of Tarshish for the house of the Lord 3 sh(ekels)', the latter has 'Gold of Ophir for the House of Horon, 30 sh(ekels)'. Without other examples of ancient Hebrew accounts dealing with payments in precious metals, it is impossible to show that this was a normal formula. With only the Tell Qasile sherd available, it seems likely that the Moussaieff ostrakon is a recent fabrication. The model the Tell Qasile sherd offers is too good!

³⁰ P. Bordreuil, F. Israel, D. Pardee, 'Deux ostraca paléo-hébreux de la Collection de Sh. Moussaieff,' *Semitica* 46 (1996) 49-76.

³¹ I. Eph'al, J. Naveh, 'Remarks on the Recently Published Moussaieff Ostraca,' *IEJ* 48 (1998) 269-73.

³² *Ibid.*, 271-72

³³ 'The Widow's Plea (3.44)' in *CoS* 3, 86.

³⁴ S. A. Kaufman, 'Reflections on the Assyrian-Aramaic Bilingual from Tell-Fekhariyeh,' *Maarav* 3 (1982) 137-75, see

Too Good to Be True or Too Odd to Be False?

Inscriptions that appear on the antiquities market or are brought to museums as chance finds may arouse uncertainty because they have strange characteristics which may bring doubt about authenticity. Yet care should be taken lest any unusual or new feature be dismissed too easily as the aberration of a modern forger. The Aramaic inscription on the back of the Tell Fekheriyeh Statue illustrates this well. The statue was not unearthed by archaeologists, a local farmer found it at the foot of the tell, he claimed. Is this a hearsay provenance, or one thought likely to please a buyer, like many that dealers give? The inscription is perfectly preserved. In discussing another text, known only from a drawing, Frank Cross observed, 'One of the striking and suspicious aspects of the inscription is its clarity. Every letter appears to be clear ... [so] the presumption ... [is] that the inscription was in a perfect state of preservation ... Such circumstances are exceedingly rare in the real world of Northwest Semitic epigraphy.'³⁵

The Script. Faced with a broken slab engraved with the same script, who among us would have accepted it as coming from the ninth century B.C.? The 'ayin with a pupil was already extinct; the inverted *lamedh* had not been seen for more than two centuries; the vertical *mem* was old-fashioned. As for the *waw* with a foot and the vertical *βade*, they are unique to this text. Could this be a concoction by a joker with some knowledge of the early West Semitic alphabet?

The Spelling. Many words have *waw* or *yodh* as *matres lectionis*, to a far greater degree than other early Old Aramaic texts. They appear in both native Aramaic words and in Akkadian loanwords: *dmwt'*, *tβlwth*, *'dqwr*, *gwgl*, *'lhyn*, *i>ryn*, *prys*. Let me quote Frank Cross again, '... twice...yod is used to mark the masc. pl. ending *-n*, a usage wholly unexpected in Old Aramaic, or, for that matter, in epigraphic Imperial Aramaic'.³⁶ Again, a fairly well educated student, aware of the use of *matres lectionis* in Old Aramaic, might think up these forms.

The Vocabulary. Some words make their first appearance in Aramaic epigraphy, e.g. *dmwt'*, *mjqy*, *m'dn*, and the Akkadian loanwords *gwgl*, *'dqwr*, *prys*. Are so many novelties acceptable?

The Grammar. Standard textbooks do not envisage the *pe'al* infinitive with prefixed *m* until Imperial Aramaic, the simple form being normal in other Old Aramaic inscriptions; the verbal form with infix *t* is not known in Old Aramaic, although it exists in Moabite; *zy* is used as a genitive particle in Aramaic renderings of Akkadian phrases in the seventh century B.C. and is common in Imperial Aramaic, and so considered a borrowing from Akkadian, yet it is clearly present on the Statue (1, 13*bis*, 17, 23). These features might be sufficient to condemn the text; its fabricator importing later forms into his supposedly early text, just as Mishnaic form might be introduced into a supposedly pre-exilic Hebrew writing.

The Common Curses. As we have already seen in discussing the Moussaieff ostraca, the curses on the Statue have parallels on Sefire Stele I and on the Bukan Stele. There are comparable curses in the Bible and in Assyrian texts. Several of them could have

³⁵ F. M. Cross, 'The Phoenician Inscription from Brazil: A Nineteenth Century Forgery,' *Or ns* 37 (1968) 437-60, reprinted in *Leaves from an Epigrapher's Notebook*, Harvard Semitic Studies 51, Winona Lake, Ind.; Eisenbrauns (2003) 238-49.

³⁶ 'Palaeography and the Date of the Tell FaΔariyeh Bilingual Inscription,' in In Z. Zevit, S. Gitin, M. Sokoloff, eds, *Solving Riddles and Untying Knots. Biblical, Epigraphic and Semitic Studies in Honor of Jonas C. Greenfield*, Winona Lake, Indiana: Eisenbrauns (1995) 393-409, reprinted in *Leaves from an Epigrapher's Notebook*, 51-60, see 56.

served as models.

Could someone living in the twentieth century have created the Tell Fekheriyeh text? Any one of the quirks I have mentioned could be good ground for supposing that, for treating it with scepticism, yet all now accept the authenticity of the Tell Fekheriyeh statue and its inscriptions as a major monument set up in the kingdom of Gozan, and most agree with the date proposed in the latter part of the ninth century B.C. There is no single feature that carries conviction; it is the total of all the features that is the proof: the sculpture, the cuneiform script and text with its specifically Assyrian features, the Aramaic text with its awkward engraving, its eccentric letters and its correspondences with the Assyrian. The place names (Guzan, Azran, Habur) and the historical and cultural contexts correlate well and adequately assure the provenance. This is not a fake.

Jerusalem Forgery Conference

Ronny Reich

On the Yehoash Inscription, I really don't know at this moment whether it is or is not a forgery. But something does bother me: If the Yehoash Inscription is a fake, then the person involved in it (the "brain," not the artisan) is of extreme interest to me. His interdisciplinary knowledge in Bible, ancient Hebrew (lexicography, syntax, orthography), paleography and some geology, geomorphology and perhaps another subject or two seems to be extraordinary. Add to this the daring to produce such a long text (two lines would have been sufficient) where one can "stumble" so many times ... If this was one person, we all most probably know and have met him. I can't imagine that it was a conspiracy of more than one person; if it involved more than one person, something would have leaked. So I am willing to listen to all the arguments that it is a forgery, but I wish someone would explain to me how these concerns of mine should be answered.

Archaeometric Overview of the Jehoash Inscription and James Ossuary

Amnon Rosenfeld, Geological Survey of Israel, and Howard R. Feldman, Division of Paleontology (Invertebrates), American Museum of Natural History, New York

A. The Jehoash Inscription, a black tablet bearing an inscription in ancient Hebrew composed mainly of quartz and feldspar minerals, is a fine-grained arkosic sandstone known from the Cambrian rocks found south of the Dead Sea, in the Timna area, and in southern Sinai (e.g., Serabit el-Khadem inscriptions). These rocks were readily available to stone engravers in Judea in ancient times.

The patina post-dates the incised inscription as well as a crack that runs across the stone, and cuts several of the engraved letters. The covering patina contains a rich assemblage of accreted particles that includes feldspars, clays, iron oxides, sub-angular quartz grains, carbon soot, microorganisms, and gold globules (1 to 4 μm in diameter). Radiocarbon dating analyses of the carbon particles incorporated into the patina yields an age of 2340 to 2150 BP.

Wind-blown dust derived from the weathering of Cretaceous marine carbonates exposed in the area as well as the limestone buildings of Jerusalem contribute large amounts of material to the local soil. Indeed well-preserved marine carbonate microfossils (foraminifers and coccoliths) were found within the patina supporting its authenticity.

The presence of microcolonial long-living black yeast-like fungi (Rosenfeld et al., 2005) inside the letters of Jehoash Inscription forming pitted embedded circular structures indicate slow growth over many years.

Depleted oxygen isotopic analyses indicate a thermal event in close proximity to the tablet. The occurrence of pure gold globules is evidence of melting, indicating a conflagration above 1000 degrees C. All this support the antiquity of the patina, which in turn, strengthens the contention that the inscription is authentic (Ilani et al., 2002).

Additional points contradicting the oxygen analyses that provided the “smoking gun” for the forgery:

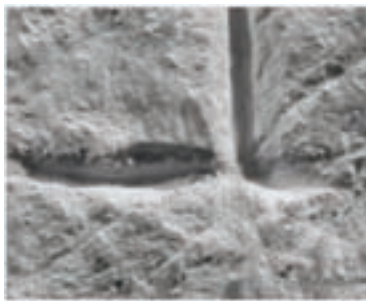
1. Dr. A. Shimron (GSI) examined the oxygen isotopic composition of ancient plasters, a major component in a tell environment. His results show very depleted isotopic oxygen values for ancient plasters, twice as much depleted as the cave deposition, similar to the JI “fake” results. Moreover, as Second Temple limestone that experienced conflagration exhibits similar depleted oxygen isotopic values to the patina found within the JI letters.
2. A recent oxygen isotopic study (Kolodny et al., 2005) deemphasizes the importance of oxygen isotopes as a palaeotemperature indicator both for lake and cave deposits. Their important conclusion is : “The dominance of the source effect in determining the oxygen isotopic composition of both speleothems (stalagmite, stalactite) and lake sediments in the Levant reduces the power of $\delta^{18}\text{O}$ as an independent climate indicator ...” The source (i.e. the rain) originates from different areas of the sea, and thus has a variable isotopic composition. This casts into doubt much of the paleoenvironmental work of Ayalon and his colleagues on the Beth-Semesh cave. These cave isotopic values were erroneously compared by Ayalon and Goren (IAA report) as a datum for all the buried antiquities!

3. A very important examination according to many archaeometric scientists is the UV illumination test. The UV test on the JI tablet detected no signs of fresh engravings.

B. The James Ossuary (JO), additional observations.

1. The brown patina “varnish” (denoted the “real patina” by the “material committee”) can be found inside the letters--accreting gradationally into the inscription (see Figures 1a,b and 2). The patina can be observed on the surface of the ossuary continuing into the engraving. The engraving clearly does not cut the patina, a strong proof for authenticity. This phenomenon can be seen almost in every letter of the inscription.

2. Notice the few scratches/fine lineaments (Figures 1a,b and 2) on almost every letter probably caused by falling roof rock in the cave during the past 2,000 years. These scratches occur both on the surface of the ossuary, moving into the letter and scratching it in the same direction. This is, in our opinion, another strong piece of evidence for authenticity.



**Figures 1-A
and 1-B (detail)**

Note the brown patina “varnish” in the letter *Beith* in *Yakob* (the letter from top to bottom is about 10 mm). The engraving does not crosscut it. The patina

goes from the surface of the ossuary into the engraving. There are some scratches on the surface of the ossuary that are oriented in the same direction into the engraved letter.

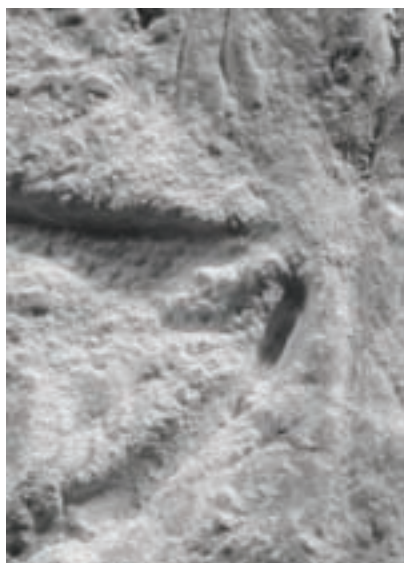


Figure 2

Note the engraving in the letter *Kof* filled with the white patina as well as a coating of the brown patina above.

Judging Forgeries—The Difference Between a Jury and a Committee of Experts

Hershel Shanks

Today in Israel, whenever suspicion arises that an inscription may be a forgery, it is becoming customary to appoint a committee of experts to decide the issue. This is a new procedure, but it has already been utilized in three cases--in the James ossuary inscription ("James son of Joseph brother of Jesus"), the Yehoash inscription (describing repairs to the First Temple) and the ivory pomegranate inscription ("Holy to the priests, (belonging) to the House of [Yahwe]h").

If American paleographer Christopher Rollston is heeded, the procedure is likely to be widely used in the future. According to Rollston's principles of extreme skepticism, "because the potential for forgery is consistently present, scholars must begin to relegate non-provenanced data to a secondary or tertiary position at the very least and must be disinclined to base conclusions regarding history, religion, language, epigraphy, etc. upon such data." Before Rollston would give an unprovenanced find a rating as "probably ancient," he would like to be "certain that laboratory anomalies are absent" from the object. "Methodological doubt and rigorous protocols are desiderata," he concludes.³⁷

A committee of experts is very different from a jury, however. A jury is picked precisely because it is not comprised of experts. Jury members are required to know nothing about the case before their appointment. If they have read about the matter in the newspapers, they are disqualified. They are to use reason, common sense, to decide the facts. They often hear the views of experts who try to explain to the jury in laymen's terms technical matters in arcane fields and the jury must decide who, even among competing experts, is correct.

A committee of experts is very different. Usually, as in the recent cases in Israel, the committee includes a variety of experts in different fields that may be relevant to determining whether the item is a forgery. The members of a committee of experts are expected to share their expertise, each contributing the expertise of his or particular field. It follows from this, however, that each member of a committee of experts must stick to his or her own expertise. Unlike members of a jury, the members of a committee of experts are not supposed to decide the validity of arguments or claims in which they are not expert.³⁸

It follows from this that a paleographer should not decide the matter on the basis of geology and a geologist should not decide the matter on the basis of paleography. But it appears that that is precisely what has happened in the recent cases in Israel.

The most egregious example is that of my good friend Ronny Reich in connection with the James ossuary inscription. He is one of the most distinguished and experienced archaeologists in Israel. He has studied innumerable inscriptions. He is prepared to comment on the archaeological, the historical, the linguistic, the paleographical aspects

³⁷ Christopher A. Rollston, "The Crisis of Modern Epigraphic Forgeries and the Antiquities Market: A Palaeographer Reflects on the Problem and Proposes Protocols for the Field," *The SBL Forum*, vol. 3, no.3.

³⁸ Of course a blue-ribbon committee may be appointed to investigate a matter, but then the committee takes testimony to inform itself. The recent Israeli committees are not blue-ribbon committees in this sense.

of the matter. But he is not qualified to comment on the geological aspects of the matter. However, that is exactly what he did. With his usual candor, he is quite explicit about it. On the basis of his variety of expertises, he could find nothing in the inscription pointing to its being a forgery. He wrote a long letter describing the condition of the inscription, the writing instrument used by the engraver, the location of the inscription on the ossuary, the position of the letters, the paleography, the language, the orthography, the nature of the content--nothing indicated to him that the inscription was a forgery. The Israel Antiquities Authority, however, wanted a unanimous decision--as is required from a jury. Reich was asked to reconsider his decision. Nothing in his own considerable expertise led him to change his mind. But in the end, he was "forced"--his word--"to change my opinion on the matter" on the basis of the geological study conducted by two hard scientists on the committee. Reich has since changed his mind again: At a banquet for participants in a Biblical Archaeology Society seminar in November 2004, Reich announced that he now regards the inscription as authentic, although not of archaeological significance.

The point, however, is that Reich should not have made a decision on the basis of someone else's expertise. By his own admission, he had only "some" knowledge of geology. He would not claim geology as his own expertise. In these circumstances, what he should have said was that using his own expertise, he was unable to find any basis for concluding that the inscription was a forgery, although some other experts, based on another expertise, may detect evidence of forgery.

I don't mean to single out my friend Ronny Reich. Many other members of the committee erred equally, but not quite so specifically. For example, Dr. Elisabetta Boaretto is an expert in radiocarbon, or C14, as it is known. No radiocarbon test was conducted on any aspect of the ossuary or its inscription. She has no expertise regarding the inscription beyond radiocarbon testing, yet with her reference to the patina "identified by Prof. Goren inside the letters," we are left with the impression that she concurs in the committee's decision. By contrast, Professor Shmuel Achituv frankly admits that "I do not see myself qualified to decide" whether the ossuary inscription is a forgery. Therefore, he leaves the matter to "his colleagues engaged in the physical aspects of the inscription." This, it seems to me, is the appropriate response to someone on the committee who is not an expert geologist.

But the IAA wanted to give the impression of unanimity. At the press briefing, where it announced the inscription to be a forgery, committee chairman Uzi Dahari stated, "We, the committee members...conclude that the patina on both items [the ossuary inscription and the Yehoash inscription] is forged." The fact is that the committee was far from unanimous. Yes, some concluded on the basis of their expertise that the inscription was a forgery. Others concluded that on the basis of their expertise there was no reason to believe it was a forgery. But for others, they could express no opinion based on their expertise.

The committee appointed by the IAA (and the Israel Museum) to determine whether the pomegranate inscription is a forgery is subject to even greater criticism because the committee includes people who have no expertise that might be useful in determining whether the inscription is a forgery. Michal Dayagi-Mendels is an excellent museum curator. But she has no expertise for determining whether the pomegranate inscription is a forgery. Similarly, with Dr. Uzi Dahari from the IAA, who is an archaeologist specializing in early Christianity. He has no known expertise that would help him to decide whether a First Temple inscription on ivory is a forgery. Did these two members of the committee concur that the pomegranate inscription is a forgery? Without

appropriate expertise, it was inappropriate for them to do so. Yet the article comprising the committee's final report will be published under the authorship of the entire committee.

If we are going to decide by committee whether an object or inscription is a forgery, as appears to be the current trend (replacing the former method of scholarly discussion in professional journals), then we should be clear how these committees are to function. If they are to function properly, each expert must opine only on his or her own expertise. A committee of experts is not like a jury of non-experts who are told to reach a unanimous verdict based only on evidence presented to it. All members of a committee of experts must decide what he or she can on the basis of his or her own expertise, not somebody else's. In short, Shoemaker: Stick to your last!

Jerusalem Forgery Conference

Andrew G. Vaughn

I. Are there forgeries? It seems likely that forgeries do exist.

a. See (among others) C. Rollston's articles in *Maarav*, jointly authored (Rollston and Vaughn) essay in NEA, Metzger's article reprinted in SBL Forum.

b. See Vaughn and Dobler's statistical articles in A. Mazar's Festschrift:

"Differences between the groups of known and unknown provenance support the probable conclusion that some of the examples in the unknown groups may be forgeries" (769) ... "The findings presented in this essay suggest that there may be bullas and seals of unknown provenance that are not authentic. These conclusions necessitate that researchers must exercise extreme caution in relying on inscriptions of unknown provenance in their studies." (770).

II. It seems likely that many inscriptions known from the antiquities market are authentic.

a. Dead Sea Scrolls often cited as banner example.

b. Other inscriptions such as Moabite Stela are likely authentic.

c. I have published seal impressions from jar handles that are identical to seal impressions on jar handles that are known from excavations, and these were obviously authentic.

d. If both forgeries and authentic inscription exist, the question is whether one should presuppose or presume that an inscription of unknown provenance is authentic or not.

III. Specific clarification / rebuttal (re: Hershel Shanks' email to conference participants about my recent article).

a. Hershel Shanks sent an email to the participants in which he quoted my article (co-authored with my colleague Carolyn Pillers Dobler). He quoted me as writing the following: "Finds that originate on the antiquities market may be glamorous, but their importance is minimal....[I]t seems prudent to us to assume that every (sic) inscription of unknown provenance is a forgery (sic) unless proved otherwise."

b. Hershel's skill as a lawyer is evident here because these quotes make me say something that I do not mean to say (and I don't believe is found in the article)—the quotes take a nuanced argument and make it overly simplistic.

i. The first quote is the final sentence of an honorific paragraph on p. 756 that describes the importance of Professor A. Mazar's work as an archaeologist as being much more important than finds from the antiquities market. The article deals with seals and seal impressions, so the sentence is intended to refer specifically to the topic of the paper. I do conclude that artifacts from excavations are more important than artifacts of unknown provenance, but some inscriptions of unknown provenance can be of importance.

ii. I was not able to find the second quote sent by Hershel in my article, but I did find a similar quote on p. 770 (14 pages after the first quote): "In light of this situation, we deem it prudent to recommend cautions and to assume that such seals and bullas are not

authentic unless proved otherwise.” As you can see my statement applies only to seals and seal impressions, but it raises an important question for other inscriptions. Of course the issues are different with other inscriptions.

iii. My statement on p. 770 does not mean that all seals and seal impressions of unknown provenance are without value, but I conclude that caution should be exercised before drawing conclusions solely on these artifacts.

IV. Focused investigation about particular bulla and bullae:

a. My statistical article only showed that it was likely that forgeries existed, but it did not say anything about a particular bulla.

b. My statistical article was written more than a year before the Israeli indictments were known, so I did not know about any particular bullae or seals that were included.

c. The question remains if specific questions can be raised about bullae that mention biblical figures. In the following, I raise these suspicions on palaeographic grounds.

i. In BAR 28:2 (2002), Robert Deutsch published numerous bullae from the time of Hezekiah. In the paper that I will present at our conference, I will argue that at least several of these bullae are likely forgeries because of palaeographic inconsistencies with bullae of known provenance and those of unknown provenance.

ii. It should be emphasized that my conclusions (if correct) do not say anything about whether Robert Deutsch is implicated—he may not have known that the bullae were forgeries (again, if I am correct), and I also need to examine the bullae in person to make sure of my conclusions based on published photographs (thanks go to Hershel Shanks for making the photographs available to me).

iii. The specific palaeographic questions relate to the letters *he* and *waw* in several of the bullae. These letters are some of the few diagnostic letters for Hebrew seals that allow a person to date the seal to either the horizon of the eighth century or the late seventh/early sixth century.

1. *He*:

a. In the horizon of the late eighth century, the letter most commonly exhibits 3 horizontal, parallel bars and a vertical shaft. Sometimes the bars are not parallel with each other and sometimes that top bar extends past the vertical shaft. However, both traits do not occur together in seals and seal impressions of known provenance (see Vaughn BASOR 313 [1999]:43–64).

b. In the horizon of the early sixth century/ later seventh century, we find the opposite (the horizontal bars are often not parallel and the top bar cross the vertical shaft). The ductus also appears to change in the later period.

2. *Waw*: More developed with separate strokes in the later period (again, see discussion in my BASOR article).

3. Ductus: Here I must be more tentative because it is hard to determine the ductus without in-person examination of the specimen. The photographs suggest that this should be investigated further.

4. Bullae from Hezekiah’s time period published in BAR 28:4 (2002). Several of these bullae exhibit the later forms of the *he* and the *waw*. I will present my drawings during the conference and solicit feedback. These conclusions (if I am correct) indicate that

several of these bullae are probable forgeries. These conclusions do not indicate that all of the bullae from the article are forgeries (it is possible, but the known data do not allow me to comment).

V. General conclusions:

- a. As I have argued in my co-authored article with Chris Rollston (in NEA and SBL Forum), we need to exercise caution and not assume that seals and bullae are authentic. I concur with the suggestions that Chris Rollston included in his abstract.
- b. I think that this caution should extend to other inscriptions (other than seals and bullae).
- c. The question remains as to whether one should assume that a bullae or seal from the antiquities market is authentic until proven otherwise.

Jerusalem Forgery Conference

Ada Yardeni

My View Concerning the Yehoash Inscription

1. The history of the events:

My first connection with [Oded] Golan was October 24, 2001, when he called me on the phone and asked if I could decipher for him certain ostraca. On November 1, he brought photos of the ostraca, as well as three ossuaries and a stone in the "Jewish" cursive script. On November 12th, a man who called himself Issac Tzur called to ask if I could examine an inscription in the ancient Hebrew script. On November 18, a messenger arrived with photographs of the so called "Yehoash Inscription". The following day Tzur called and I asked to see the stone itself. On November 20 a messenger brought me the photographs again, but not the stone. On the November 22, Tzur called again and asked me to write a palaeographical report of the inscription. I again asked to see the original. After two more phone calls Tzur, or a person who called himself Tzur, arrived with the stone and with a geological report, on December 2. While he stayed at my home, I examined the stone with the help of a magnifying glass (x10) with a lamp. He did not agree to leave the stone with me. I wrote my report on the basis of my examination of the stone and of the photographs he lent me.

2. Translation of my report, originally written in Hebrew on November 25, 2001:

Since the inscription has not been found in excavations, there is no archaeological evidence for its authenticity. The impression is that the surface of the stone is in a state of excellent preservation, more than expected of a find which is supposed to be 3,000 years old. However, this fact alone cannot refute the antiquity of the inscription.

The orderly diffusion of the letters on the stone may indicate an early planning of the inscription and a drawing of the letters before engraving.

A comparison of the form of the letters with that of a few inscriptions dated to the tenth and ninth centuries B.C.E. (Gezer, Mesha, Tel Dan, Cyprus and Killamua), shows that the script mostly resembles that of the Cyprus and Tel Dan inscriptions, and seems to fit generally into an intermediate phase in the evolution of the script from the Phoenician to the Early Hebrew.

According to the accepted palaeography, Mesha's inscription from the mid-ninth century B.C.E. represents the Hebrew script in its early phase. Most of the letters in this inscription look more developed than those in the so-called "Yehoash Inscription". Therefore, in my opinion, the script of our inscription could characterize a phase of development of the Hebrew script from the Phoenician script in about the ninth century B.C.E.

The inscription from Cyprus was dated to the early ninth century B.C.E. A few letters in the so called "Yehoash Inscription" resemble the letters of this inscription (see the chart below).

These are the main features of these letters: a rounded *Alef*, *Bet* leaning forward, *Waw* with its top turning to the right, the inclination of *Zayin*, the convex curve at the top of *Yod*, *Lamed* with a curved base, the size of *Mem* as well as its inclination, rounded *Ayin*, *Qof* with a round and closed top, *Resh* with a triangular top and a straight down-stroke.

The main features of the letters which do not resemble those of the Cyprus inscription: *Gimel* is more erect, *Dalet* more angular, *He* and *Shin* wider, *Mem* and *Nun* more curved, *Pe* is shorter, both strokes of *Taw* are of equal size.

There is a striking resemblance in general appearance between the so-called “Yehoash Inscription”, written in the Hebrew language, and the Aramaic Tel Dan inscription. They resemble each other in the size of the letters, the spacing of the letters and the method of flat engraving, but in the Tel Dan inscription the letters sharpen toward their bottom whereas in the so-called “Yehoash Inscription” most of the strokes have a flat end. Despite the general affinity, there are several differences between these two inscriptions in the form of the letters (see the chart).

The main features common to letters in the two inscriptions: *Bet* leaning forward, triangular *Dalet* with a short, right down-stroke, the convex curve at the top of *Yod*, the zigzag of *Mem* and of *Nun*, the general structure of *Samekh*, curved *Ayin*, *Resh* with a triangular upper part and a straight down-stroke.

Some of the differences between the two inscriptions may be personal while others may be stylistic, local differences.

The following may be personal differences: In the so-called “Yehoash Inscription” the *Bet* is more angular, the three parallel strokes of *He* are of equal size unlike a certain shortening of the lower strokes in the Tel Dan inscription, the down-strokes of *Kaf*, *Mem* and *Nun*, are somewhat shorter, *Ayin* is smaller, *Pe* is shorter, *Shin* is much broader.

The following may be stylistic, local differences: In Yehoash, the *Alef* is more curved, the *Gimel* has a horizontal “roof” as against the slanting left stroke in Tel Dan, the upper part of the *Waw* opens up to the right whereas in Tel Dan it opens upwards, the parallel strokes of the *Zayin* are connected by a short, middle stroke unlike the slanting stroke creating a zigzag form of the *Zayin* in Tel Dan, the down-strokes of *Het* join the right upper and left lower corners whereas in Tel Dan they extend beyond the meeting points, *Mem* and *Nun* curve down to the left, *Lamed* is more curved, *Sadi* has a very short left down-stroke unlike the longer down-stroke in Tel Dan, the top of *Qof* is a small, closed circle as against the oval, slanting to the left top of the letter in Tel Dan, both strokes of *Taw* are of equal size as against the much longer left stroke of *Taf* in Tel Dan.

The fragments of the Tel Dan Aramaic inscription have been discovered in two excavation seasons conducted by Abraham Biran. It was dated to about the last third of the ninth century B.C.E. In this period, it is still difficult to distinguish between Aramaic script and its ancestor, the Phoenician script. Some of the stylistic, local differences between the two inscriptions could be described as differences between two types of the Phoenician script, sometimes later evolving into the Hebrew and Aramaic scripts.

However, certain features of the script of the so called “Yehoash Inscription” may be problematic from the palaeographical point of view, in comparison with the known epigraphical material.

These are the problematic features: The occasional extreme inclination of *Bet* and the angular join of the strokes at the right lower corner, the horizontal “roof” of *Gimel*, the somewhat wide *He* and the very wide *Shin*, the relative small size of *Pe* and of *Sadi*, the relative large, x-formed *Taw*. However, there is not enough epigraphical material from that period to enable a definite conclusion. One can hardly explain the features mentioned above as idiosyncratic features of the scribe.

A puzzling detail is the down-stroke of *He* in the second line of the inscription, which does not reach the break at the top of the stone and the two-fold carving of the upper stroke of this *He*, the remains of which appear near the break.

There also are a few linguistic questions concerning this inscription which I mentioned in my report but won't deal with them here since they were dealt in detail by other scholars.

In view of the above, I could not confirm with certainty the authenticity of the so-called "Yehoash Inscription."

Kilamuun

Tel Dan

Yekesht

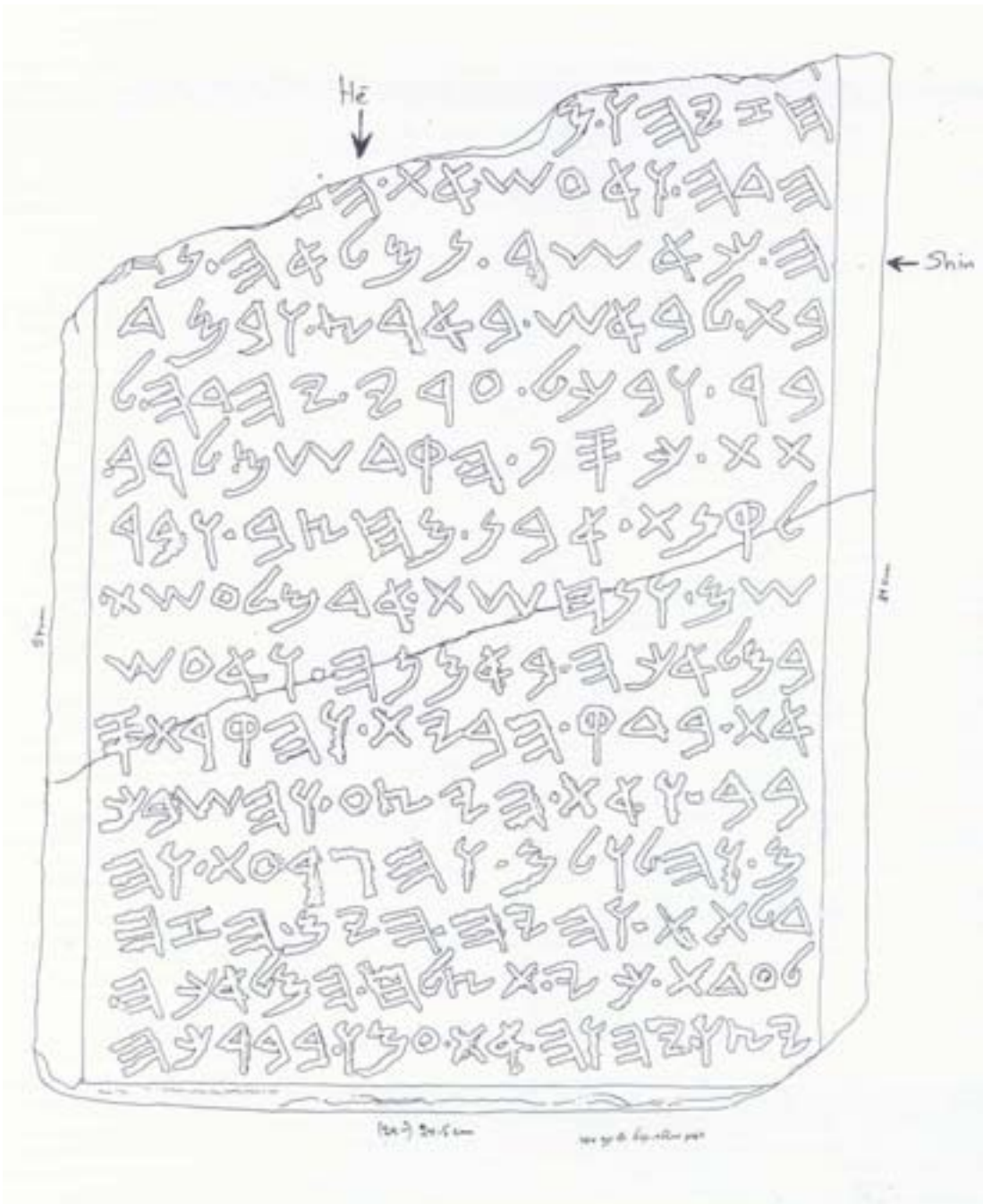
Mesha

Gezer

Cyprus

𐤀	𐤁𐤁	𐤁𐤁 (circled)	𐤁	𐤁	𐤁 (circled)
𐤂	𐤂𐤂	𐤂𐤂 (circled)	𐤂	𐤂	𐤂𐤂
𐤃	𐤃	𐤃 (circled)	𐤃	𐤃	𐤃𐤃
𐤄	𐤄𐤄	𐤄𐤄 (circled)	𐤄	𐤄	𐤄𐤄
𐤅	𐤅𐤅	𐤅𐤅 (circled)	𐤅	𐤅	𐤅𐤅
𐤆	𐤆𐤆	𐤆𐤆 (circled)	𐤆	𐤆	𐤆𐤆
𐤇	𐤇𐤇	𐤇𐤇 (circled)	𐤇	𐤇	𐤇𐤇
𐤈	𐤈𐤈	𐤈𐤈 (circled)	𐤈	𐤈	𐤈𐤈
𐤉	𐤉𐤉	𐤉𐤉 (circled)	𐤉	𐤉	𐤉𐤉
𐤊	𐤊𐤊	𐤊𐤊 (circled)	𐤊	𐤊	𐤊𐤊
𐤋	𐤋𐤋	𐤋𐤋 (circled)	𐤋	𐤋	𐤋𐤋
𐤌	𐤌𐤌	𐤌𐤌 (circled)	𐤌	𐤌	𐤌𐤌
𐤍	𐤍𐤍	𐤍𐤍 (circled)	𐤍	𐤍	𐤍𐤍
𐤎	𐤎𐤎	𐤎𐤎 (circled)	𐤎	𐤎	𐤎𐤎
𐤏	𐤏𐤏	𐤏𐤏 (circled)	𐤏	𐤏	𐤏𐤏
𐤐	𐤐𐤐	𐤐𐤐 (circled)	𐤐	𐤐	𐤐𐤐
𐤑	𐤑𐤑	𐤑𐤑 (circled)	𐤑	𐤑	𐤑𐤑
𐤒	𐤒𐤒	𐤒𐤒 (circled)	𐤒	𐤒	𐤒𐤒
𐤓	𐤓𐤓	𐤓𐤓 (circled)	𐤓	𐤓	𐤓𐤓
𐤔	𐤔𐤔	𐤔𐤔 (circled)	𐤔	𐤔	𐤔𐤔
𐤕	𐤕𐤕	𐤕𐤕 (circled)	𐤕	𐤕	𐤕𐤕
𐤖	𐤖𐤖	𐤖𐤖 (circled)	𐤖	𐤖	𐤖𐤖
𐤗	𐤗𐤗	𐤗𐤗 (circled)	𐤗	𐤗	𐤗𐤗
𐤘	𐤘𐤘	𐤘𐤘 (circled)	𐤘	𐤘	𐤘𐤘

similar forms are encircled



(Yardeni, cont.)

My View Concerning the James Ossuary Inscription

I have already expressed my opinion that the so called “James Ossuary Inscription” is authentic. My view is based on palaeographic as well as circumstantial considerations.

A few years ago, I received from Oded Golan a photograph of the ossuary together with photographs of two other ossuaries as well as of a stone vessel. Three of the four items, including the “James Ossuary” were inscribed in what is known as the “Jewish” script of the Herodian Period. I made drawings of the inscriptions, copying them on transparent film, in order to decipher them. This is my method of deciphering ancient inscriptions because in this way I can trace the letter forms and confirm my readings. I never noticed any palaeographical problem with any of the inscriptions mentioned above.

Oded has not shown any particular interest in my drawings of these inscriptions.

When working on Hebrew ostraca with André Lemaire in Jerusalem later that year I was informed by him that he intends to write an article about the so-called “James Ossuary”. I was surprised, because I thought that since I worked on it I should also publish it. However, it never occurred to me that this inscription referred to the brother of Jesus. The names Ya’aqov, Yosef and Yeshua were very popular in that period and each one of them appears in a number of ossuary inscriptions as well as in documentary texts of the period on other materials. A short time after being informed of the matter, I was asked by Hershel Shanks to make a new drawing of the inscription, this time from the original. I phoned Oded and asked him if I could come to his place and copy the inscription. He answered that I should come the next day, as long as the ossuary is at his home, because he intends to move it to another place. I went there and made copies of the three ossuaries, which he placed in the kitchen under the window in the sun light so I can see the letters very clearly. I did not notice any problem with the letters or any change in their execution.

As for the claim that the two parts of the inscription were made by two different hands, I insist on my opinion that this is not the case. If there are differences between the forms of certain letters, this is a natural phenomenon in handwritten inscriptions, as can be seen in numerous ossuaries (see, e.g., Rahmani nos. 27,61, 70, 200, 246, 256, 573, 702, 730, 893). In many inscriptions and documents the script becomes smaller or more cursive toward its end. This is the result of the natural tendency to increase the pace of writing, mainly with ink on soft material. One may perhaps assume that the engraver copied the inscription from a draft written in ink. The person who wrote the text in the first place may have been trained in cursive writing and therefore some letters, such as *Waw*, *Yod* and the *Alef* of the word *Akhui*, as well as the following *Dalet*, appear in their cursive rather than lapidary forms. But even if not copied from a draft, most of the ossuaries display a quite negligent script, unlike monumental inscriptions, such as the Uzziah epitaph, which display a more elegant and careful script, perhaps influenced by the contemporary Greek inscriptions. The reason is probably that these short inscriptions were not meant to be representative and served mainly for the identification of the deceased. It is also interesting to see the contemporary Nabataean tomb inscriptions, which display a very beautiful scribal practice. Some of them even mention the name and title of the engraver, such as *Aptah passala*--Aptah the engraver--who appears in several inscriptions.

In any case, most of the Hebrew-Aramaic as well as Greek ossuary inscriptions, and even certain tomb inscriptions, such as the engraved Benei Hezir inscription or the

painted Jason inscriptions, do not show a calligraphic concern. The simplicity of the script and its seemingly careless execution is amazing.

I would like to add two more points:

1. Of all the inscriptions accused of being forgeries, the so called "James ossuary" is the only one in a different script, namely, the so called "Jewish" script. For this reason alone it does not belong to the group of inscriptions written in the Ancient Hebrew, or Palaeo-Hebrew script, the authenticity of which was put in question.

2. As can be seen with bare eyes in the drawing of the relatively small inscription, here represented in its original size, all the strokes of the letters consist of a central deep groove with slanting "walls" on both its sides. The approximate width of the strokes seems similar in all the letters. The claim that the depth of the engraving differs from the first to the second half of the inscription seems to me mistaken if not biased. I would like to know exactly how the depth was measured and the exact difference in the depth. In such a small script, I doubt if a significant difference in the depth can be observed.

I wonder if anybody investigating the case of the allegedly forged inscriptions saw the tools used for the engraving of the different inscriptions. I would assume that for each inscription a different implement was used. This evidence seems to me essential in such accusations. What do we know about the technique of the engraving? I would like to learn about the form of the tip of the tool in each case. Were all the engraved inscriptions made with the same tool? Were more than one inscription made with the same tool? Have such tools been found in excavations? Have such tools been found with the alleged forger?

It is quite clear that the scribe of the so called "James ossuary" has not copied the letters from any other ossuary inscription. The claim that he was inspired by the one mentioning the word *Akhui* (Rahmani 570) is not convincing. The fact that this form of the pronominal suffix, which is very well attested in documents from a later period, already appears in an ossuary inscription of the first century C.E., indicates that it was already in use in this period and therefore likely to appear in another inscription.

Handwritten text in a stylized script, possibly representing a name or a title.



James Ossuary Photo Report

RICHARDS FORENSIC SERVICES
EXAMINER OF QUESTIONED DOCUMENTS AND PHOTOGRAPHS
15307 Alan Drive, Laurel, Maryland 20707
301-725-3778; Fax 301-725-1714

June 8, 2005

Mr. Lior Bringer, Advocate
39 Shaul Hamelech Blvd.
Hadar Dafna Building
3rd. Floor
Tel Aviv 64928
Israel

REFERENCE: Your letter dated May 19, 2005

LABORATORY FILE NUMBER: A05021P

ITEMS RECEIVED: Via Federal Express, May 25, 2005

LABORATORY FILE NUMBER: A05021P

ITEMS RECEIVED: Via Federal Express, May 25, 2005.

Q1 One black and white photographic print approximately 110mm X 148 mm containing an image of a book shelf, a number of pieces of pottery and that appears to be two stone boxes.

Q2 One black and white photographic print approximately 127mm x 168mm containing an image of just the two stone boxes shown in Q1.

ALSO SUBMITTED ITEMS: An enlarged work copy of both Q1 and Q2.

REQUEST: Determine if the Q1 and Q2 prints were or were not produced approximately 30 years ago, in the 1970s.

RESULTS OF EXAMINATION: Only non-destructive examinations were conducted on Q1 and Q2. This examination is based on a **detailed individual examination** of the questioned images. The examination is normally conducted visually using various degrees of magnification and different light sources. The prints are further examined using short and long wave ultraviolet luminescence and using infrared reflectance and luminescence techniques. In addition, the photographic prints are scanned and analyzed using a variety of imaging tools to provide relevant information regarding the specific questions being asked. Based on all of the aforementioned information, a determination is made as to whether a reasonable conclusion can be drawn.

Q1 and Q2 are both black and white photographic prints produced on resin coated (RC) papers.

Q1 is approximately 110mm x 148mm by .236mm thick. Q2 is approximately 127mm x 168mm

by .236 thick.

Both Q1 and Q2 have a black inked stamp on their reverse side which reads “Kodak Polycontrast rapid RC paper” with the next line showing the notation “exp 3/76 F|mw”. Kodak Polycontrast Rapid RC Paper was introduced in the early 1970s and was eventually replaced in the late 1970s/early 1980s by Kodak Polycontrast Rapid II RC Paper. Today’s current replacement to it is Kodak Polycontrast Rapid IV RC Paper. The notation “exp 3/76 F|mw” indicates an exposure date of March 1976 and a “F” surfaced paper (smooth glossy) with a “mw” or medium weight thickness. The reverse side of both Q1 and Q2 also contain a manufacturer’s repetitive “watermark” which reads “THIS PAPER MANUFACTURED BY KODAK”. (See Exhibits A & B).

The reverse side of both Q1 and Q2 also contained numerous chemical stains both random and in the shape of a rectangle. The rectangular stains indicate another piece of paper or other object was in contact with the reverse side of these prints. (See Exhibits A & B). These stains appear as light soiling, but have been color and contrast enhanced in the exhibits for better visualizing. In addition, a second rectangular shape was observed on the reverse of Q1 & Q2 when viewed with infrared luminescence (See Exhibits C&D). These shapes are not visible to the human eye.

Examination of the image on Q1 and Q2 indicated a naturally lit, reasonably sharp photograph having good dynamic range. It was noted that the borders of the prints appeared to be gray, indicating some “fogging” due to either light or chemicals. Further examination of the books in Q1 revealed that the far right book has a date of “1974” printed on its spine. (See Exhibit E).

Although it could not be definitely determined if Q1 and Q2 were produced in the mid 1970s, nothing was noted that would indicate or suggest that they were not produced March 1976 as indicated on the stamp appearing on the reverse side of each print. In addition, Q1 and Q2 appear to contain normal wear and tear due to age and handling. This is exemplified by the random stains which are not fully noticeable in the normal visible portion of the spectrum, and the rectangular shapes from contact with other objects. All of the characteristics noted, suggest or indicate that Q1 and Q2 were not produced recently, but were prepared in the mid to late 1970s.

For reference purposes, exhibits are attached showing Q1 and Q2 as they looked when received, Q1 front is Exhibit F; Q1 reverse is Exhibit G; Q2 front is Exhibit H; and Q2 reverse is Exhibit I.

Q1, Q2 and the Also Submitted Items are being returned herewith. Copies of Q1 and Q2 are being retained.

[signed]

Gerald B. Richards



(Low Res. – 1:1) Q-1



Q-2 (Low Resolution, 1:1)

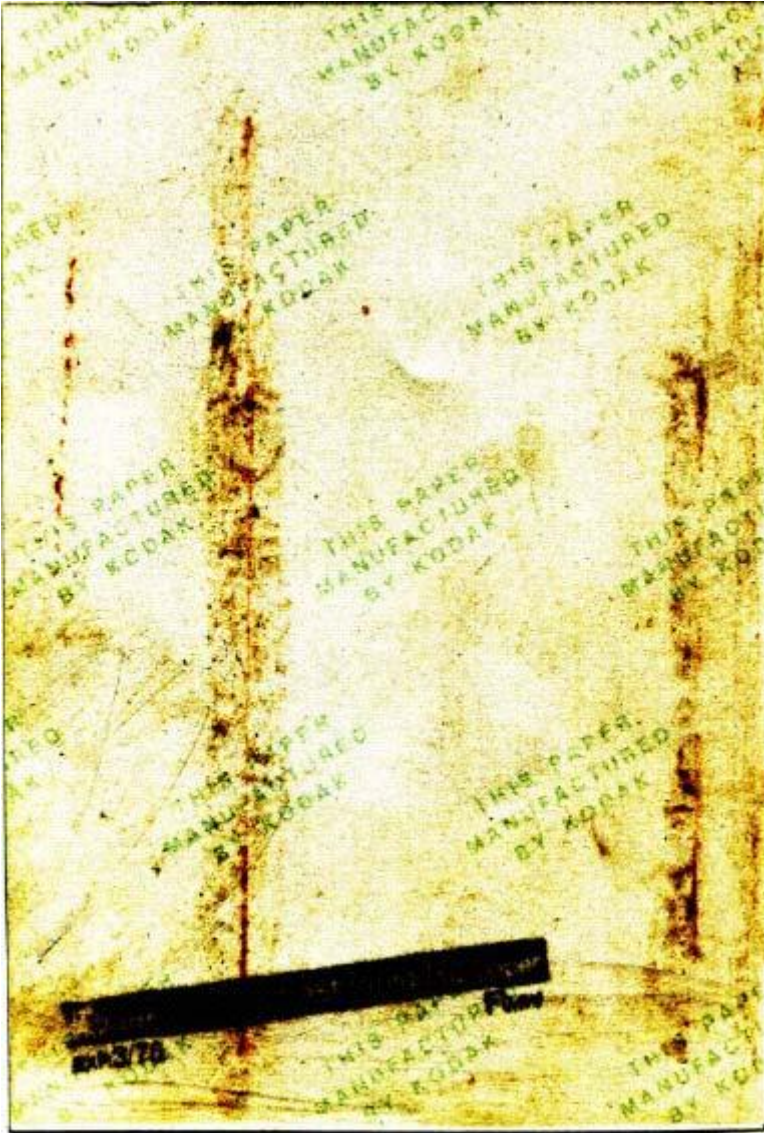


Exhibit A

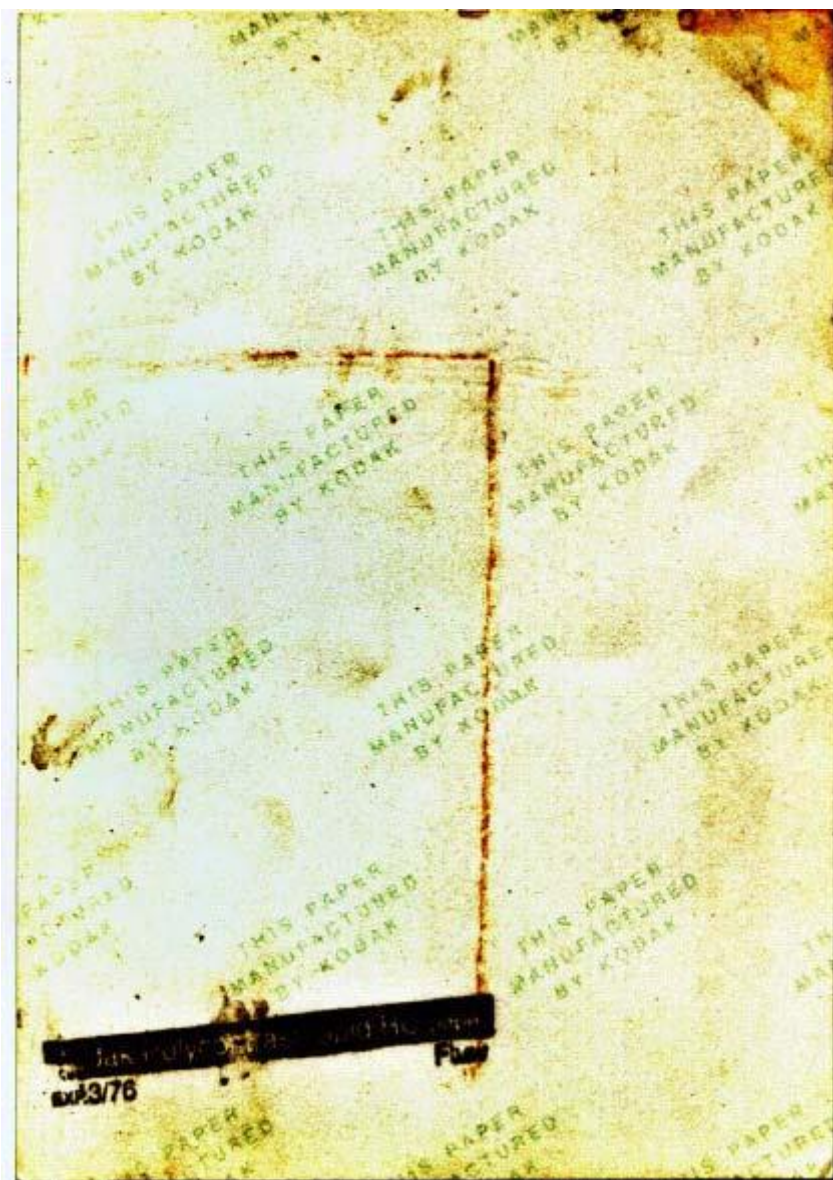


Exhibit B



Exhibit C

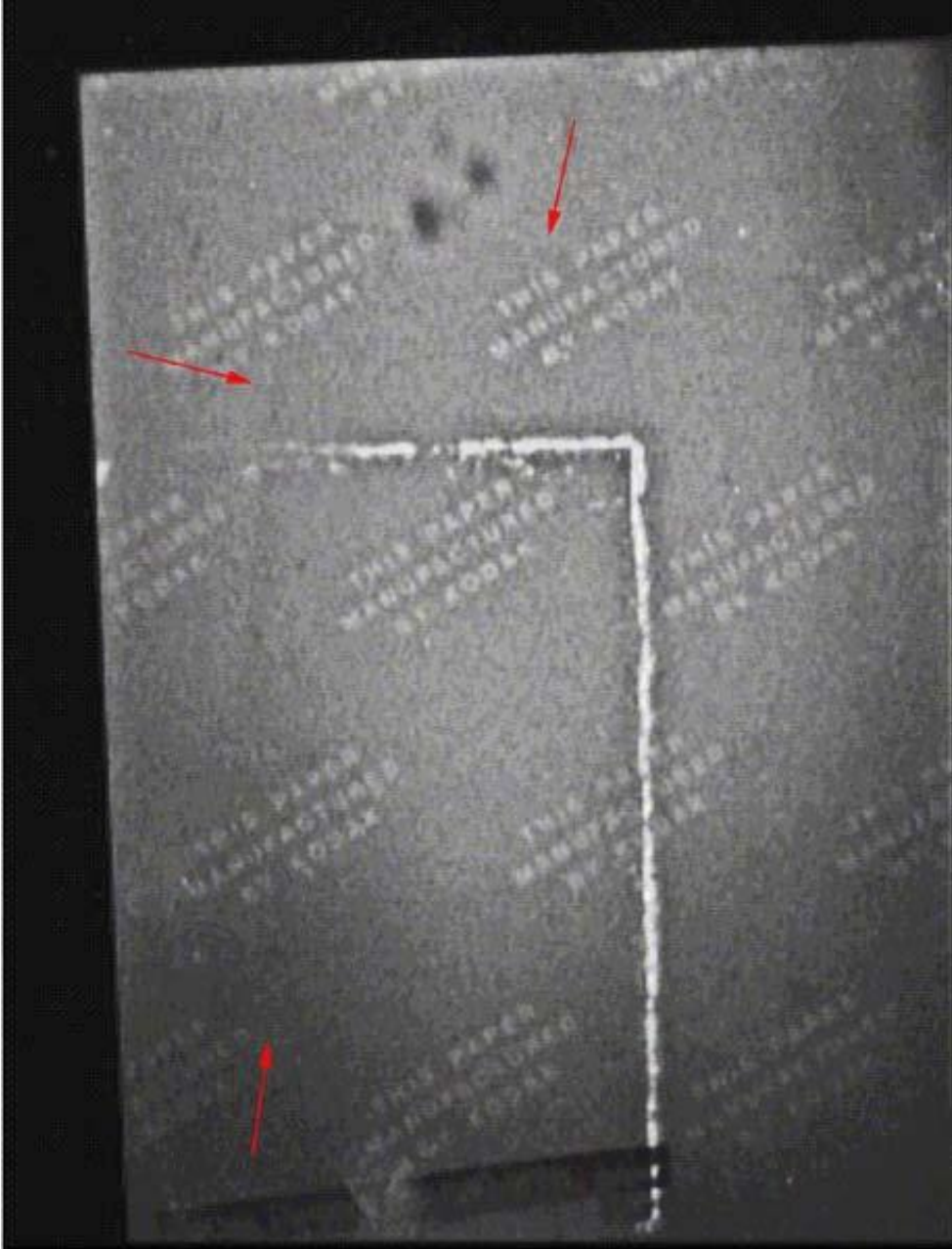


Exhibit D



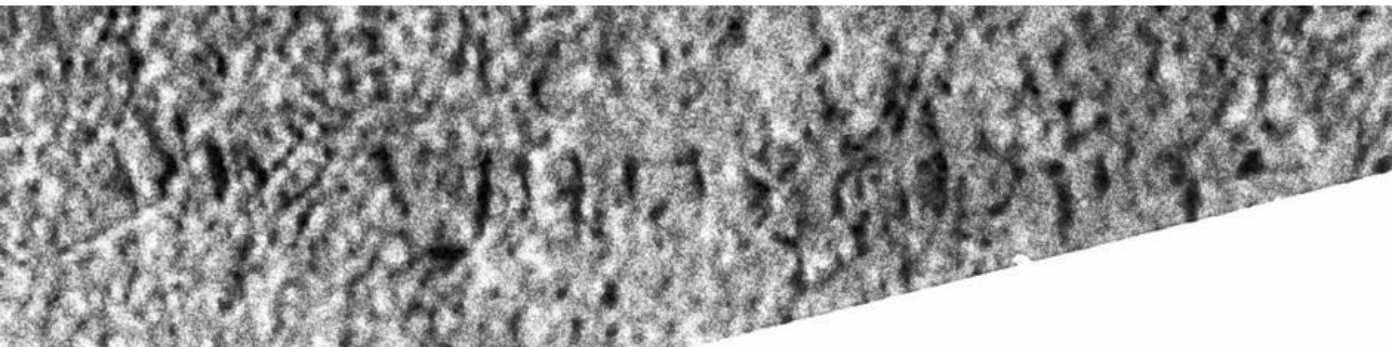
Exhibit E



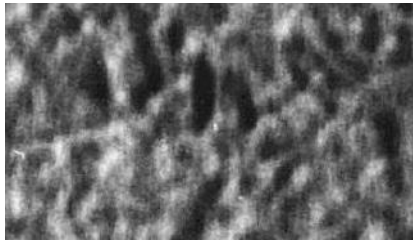
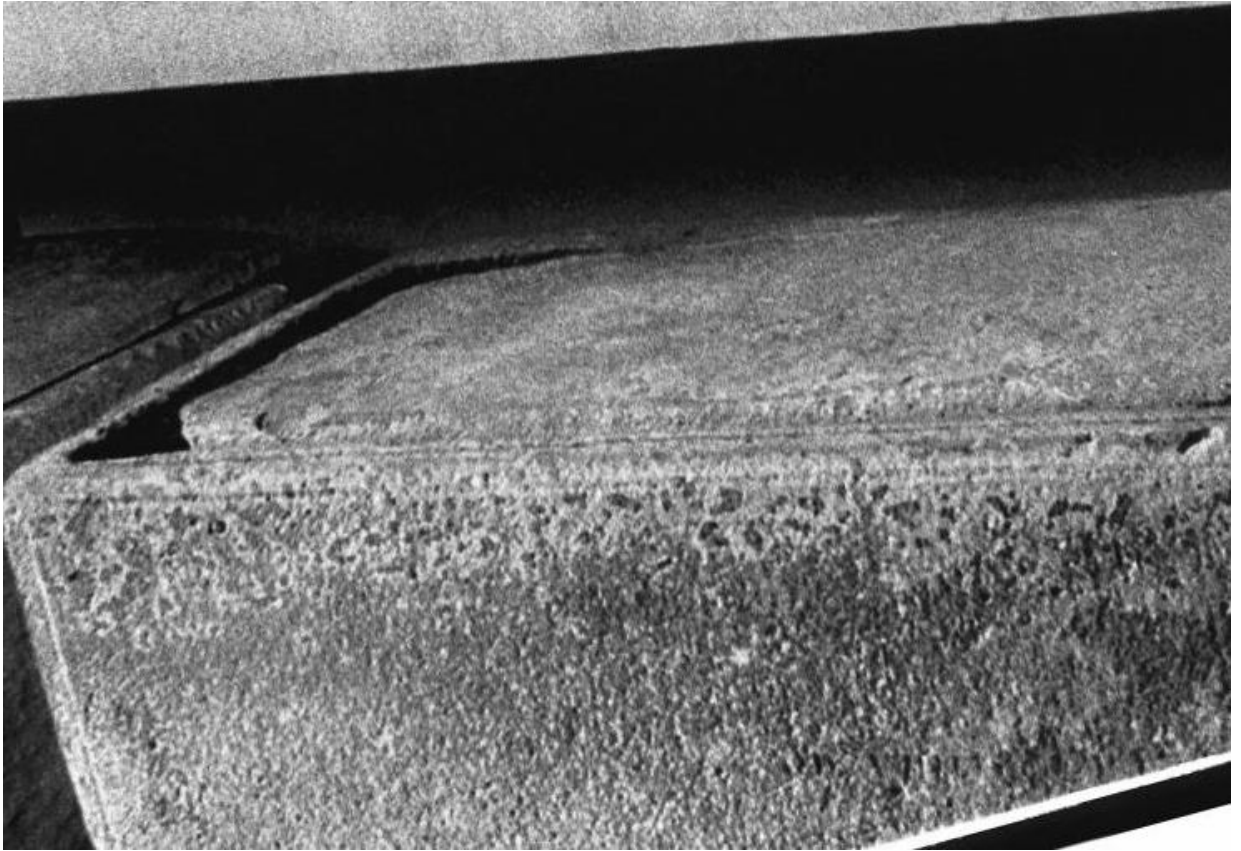
Exhibit F - Detail 2



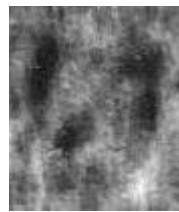
Exhibit G (Photo Q1 – Back)



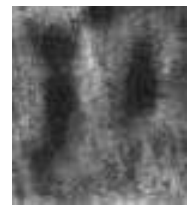
ב ר י ו ס א ח ו י ד י ש ו ע



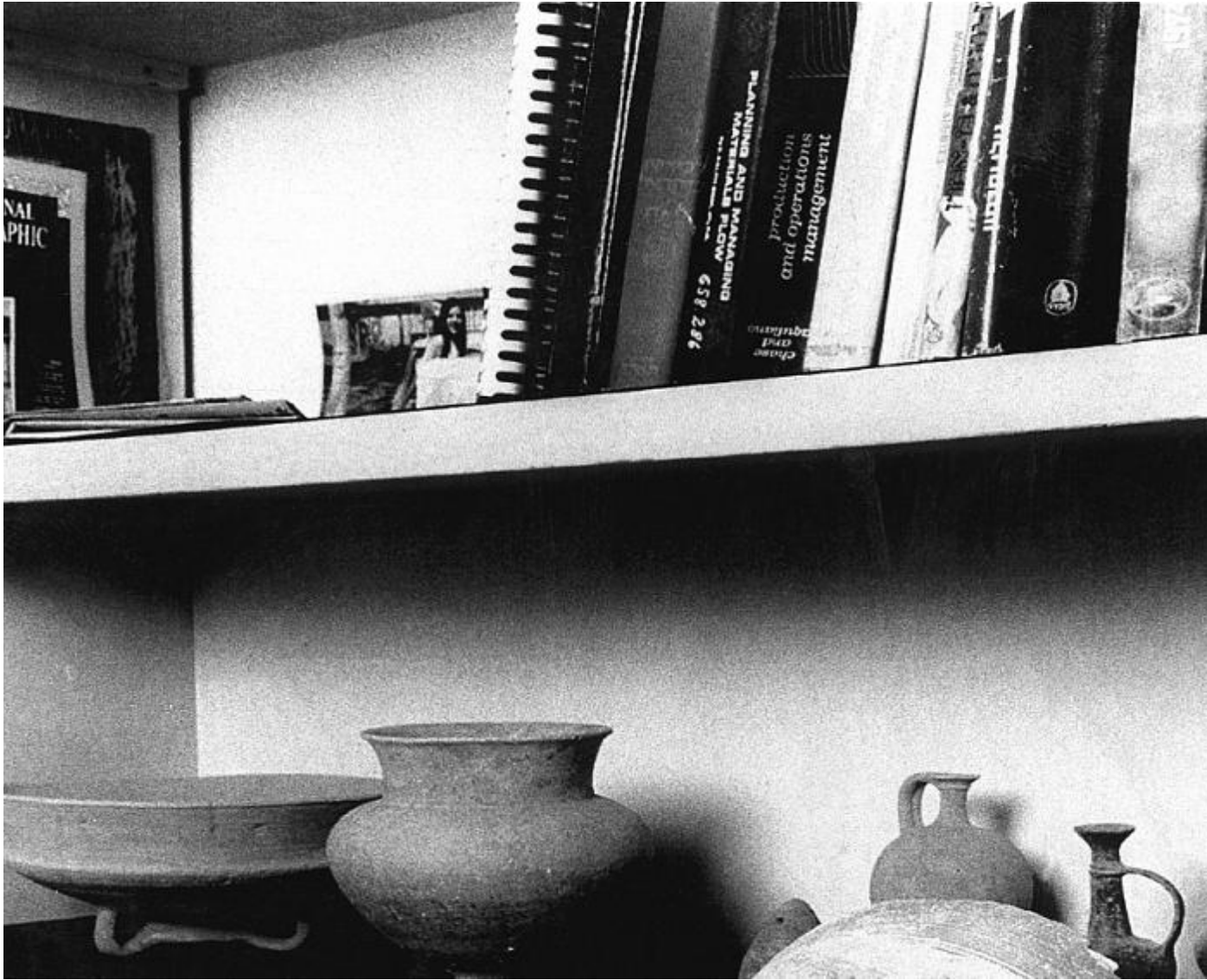
ע ו ש י



ח



יו

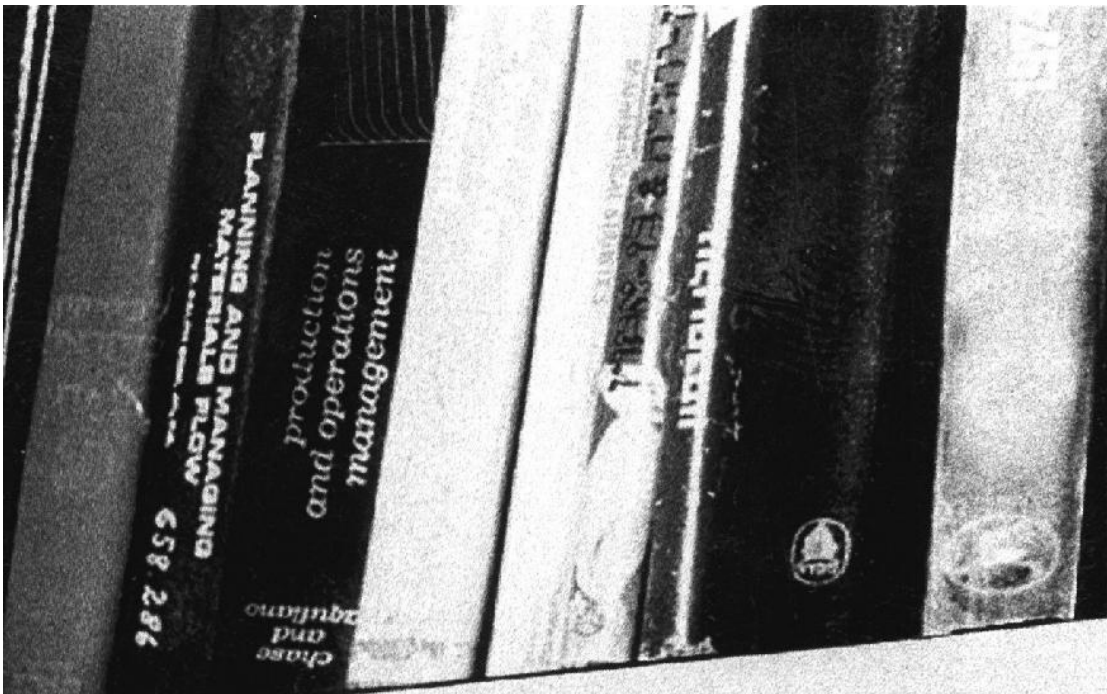


Q-1 Upper Part (High Res. by Total-Shapira)



Q-1 Lower Part (High Res. By Total-Shapira)

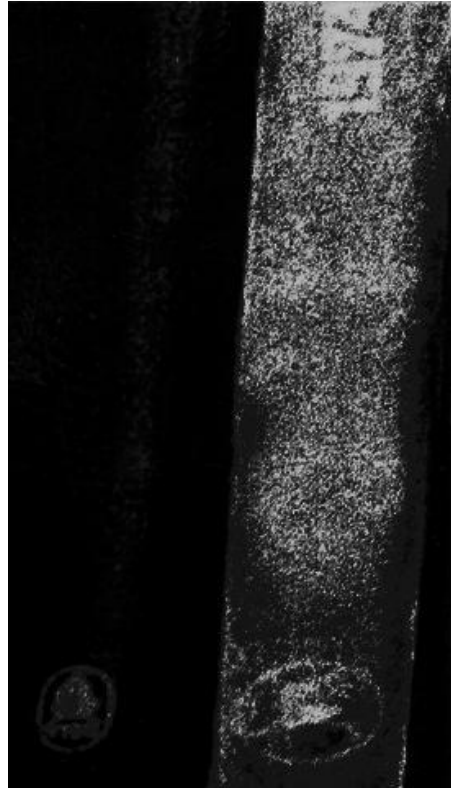
Photo Q-1 Details



negative photo:



positive:



JOHN Record

[Elton John "CARIBOU" 1974]



Source for Elton John's record: The Internet

RICHARDS FORENSIC SERVICES

EXAMINER OF QUESTIONED DOCUMENTS AND PHOTOGRAPHS

15307 Alan Drive

Laurel, Maryland 20707

Voice 301-725-3778

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e-mail: gerald.richards@verizon.net

CURRICULUM VITAE

GERALD B. "JERRY" RICHARDS

DATE AND PLACE OF BIRTH

MARCH 25, 1943 AT CHICAGO, ILLINOIS

PRESENT POSITIONS

PRIVATE CONSULTANT - LECTURER & EXAMINER OF QUESTIONED DOCUMENTS AND PHOTOGRAPHS:

RICHARDS FORENSIC SERVICES: EXAMINER OF QUESTIONED DOCUMENTS AND PHOTOGRAPHS: 1/94-PRESENT

GEORGE WASHINGTON UNIVERSITY, FORENSIC SCIENCE DEPARTMENT, ASSOCIATE PROFESSORIAL LECTURER: 1/94-PRESENT

EDUCATION

SOUTHERN ILLINOIS UNIVERSITY, CARBONDALE, ILLINOIS:

BACHELOR OF SCIENCE DEGREE-1966: MAJOR-PHOTOGRAPHY

MASTER OF SCIENCE DEGREE IN EDUCATION-1967: SECONDARY EDUCATION

GEORGE WASHINGTON UNIVERSITY, WASHINGTON D.C. COURSES: 1973

FORENSIC SCIENCE IN LAW ENFORCEMENT III

EXAMINATION OF QUESTIONED DOCUMENTS

THE CRIME LAB - THE FORENSIC SCIENTIST AND THE CRIMINAL LAWYER

UNIVERSITY OF VIRGINIA, AT THE FBI ACADEMY, QUANTICO, VA: 1974

CRIME LAB PHOTOGRAPHY

EMPLOYMENT

COUNTRY COMPANIES INSURANCE COMPANY, BLOOMINGTON, ILLINOIS -

PUBLIC RELATIONS REPRESENTATIVE: 6/67-10/70

FEDERAL BUREAU OF INVESTIGATION (FBI)

ASSIGNMENTS:

SPECIAL AGENT TRAINING: QUANTICO, VA. 11/70

ATLANTA DIVISION: SPECIAL AGENT INVESTIGATOR

BALTIMORE DIVISION: SPECIAL AGENT INVESTIGATOR

FBI HEADQUARTERS - SUPERVISOR, LABORATORY DIVISION

DOCUMENT SECTION UNITS - DOCUMENT/PHOTO EXAMINER

11/72-6/80

SPECIAL PHOTOGRAPHIC UNIT DOCUMENT/PHOTO EXAMINER

6/80-4/86

DOCUMENT OPERATIONS AND RESEARCH UNIT - CHIEF: 4/86-8/87

SPECIAL PHOTOGRAPHIC UNIT - CHIEF: 8/87-12/93

RETIRED FBI 12/93

CONSULTANT - INTERNATIONAL CRIMINAL INVESTIGATIVE TRAINING ASSISTANCE PROGRAM (ICITAP) 7/95-7/96

CONSULTANT - **FBI LABORATORY, DOCUMENT/PHOTOGRAPHIC TRAINING & EXAMINATION** MATTERS: 6/94-9/99

AREAS OF EXPERTISE

EXAMINATION OF QUESTIONED DOCUMENTS

CERTIFIED:

DIRECTOR, FEDERAL BUREAU OF INVESTIGATION

AMERICAN BOARD OF FORENSIC DOCUMENT EXAMINERS, INC.

FORENSIC PHOTOGRAPHIC EXAMINATIONS

CERTIFIED:

DIRECTOR, FEDERAL BUREAU OF INVESTIGATION

AMERICAN SOCIETY OF PHOTOGRAMMETRY & REMOTE SENSING (INACTIVE)

FOREIGN ESPIONAGE TRADECRAFT

COURT TESTIMONY

QUALIFIED ON NUMEROUS OCCASIONS AS AN EXPERT IN ONE OR MORE AREAS OF EXPERTISE:

FEDERAL COURTS, STATE COURTS, LOCAL COURTS

FOREIGN COURTS

COURT MARTIAL PROCEEDINGS

ORGANIZATIONAL AFFILIATIONS

AMERICAN ACADEMY OF FORENSIC SCIENCES (AAFS)

FELLOW

QUESTIONED DOCUMENT SECTION SECRETARY, 1998

BOARD OF DIRECTORS, 2000-2003

AMERICAN BOARD OF FORENSIC DOCUMENT EXAMINERS, INC. (ABFDE)

DIPLOMATE

BOARD OF DIRECTORS, 2000-2003

MID-ATLANTIC ASSOCIATION OF FORENSIC SCIENTISTS (MAAFS)

PRESIDENT, 1980-1981

MEMBER AT LARGE, 1989

PRESIDENT, 1991-1992

INTERNATIONAL ASSOCIATION FOR IDENTIFICATION (IAI)

AMERICAN SOCIETY FOR TESTING AND MATERIALS (ASTM)

EVIDENCE PHOTOGRAPHERS INTERNATIONAL COUNCIL (EPIC)

THE PHOTOGRAPHIC HISTORICAL SOCIETY OF NEW ENGLAND, INC. (PHSNE)

SOCIETY OF FORMER SPECIAL AGENTS OF THE FEDERAL BUREAU OF INVESTIGATION

ASSOCIATION OF FORMER INTELLIGENCE OFFICERS (AFIO)

TECHNICAL WORKING GROUP FOR FORENSIC DOCUMENT EXAMINATION (TWGDOC)

SUBCOMMITTEE FOR STANDARDIZATION OF OPERATING PROCEDURES AND
TERMINOLOGY

TECHNICAL WORKING GROUP FOR EDUCATION AND TRAINING IN FORENSIC SCIENCE
(TWGED)

TRAINING (PARTIAL LIST)

QUESTIONED DOCUMENT AND FORENSIC PHOTOGRAPHIC EXAMINATIONS - FBI
LABORATORY: 11/72-8/75

SEMINAR ON SINGLE PHOTO PERSPECTIVE, WASHINGTON D.C.: 9/18-22/78

ADVANCED SEMINAR SINGLE PHOTO PERSPECTIVE, WASHINGTON D.C.: 10/22-26/79

BASIC PHOTOGRAMMETRY, QUANTICO, VIRGINIA: 3/24-28/80

LABORATORY MANAGEMENT SEMINAR, QUANTICO, VIRGINIA: 5/27-30/80

DIGITAL IMAGE PROCESSING - INTEGRATED COMPUTER SYSTEMS, WASH. D.C.: 9/81

NATIONAL SENIOR CRYPTOLOGIC COURSE (CY-600), NATIONAL SECURITY AGENCY,
(NSA) LAUREL, MD, 4/1/85-5/17/85

CANON PHOTOCOPIER & FACSIMILE TRAINING WORKSHOP, NORCROSS, GA., 10/4-6/99

CHEMISTRY OF COLOR WORKSHOP, MAAFS, WILLIAMSBURG, VA., 4/01

PAPER MANUFACTURING WORKSHOP, MAAFS, WILLIAMSBURG, VA., 4/01

AWARDS RECEIVED

OUTSTANDING AUDIOVISUAL ACHIEVEMENT AWARD

ASSOCIATION OF FEDERAL PHOTOGRAPHERS, 11/29/84

NATIONAL INTELLIGENCE MEDAL OF ACHIEVEMENT

NATIONAL FOREIGN INTELLIGENCE COMMUNITY, 7/19/91

THE NATIONAL INTELLIGENCE MEDAL OF ACHIEVEMENT IS AWARDED FOR ESPECIALLY MERITORIOUS CONDUCT IN THE PERFORMANCE OF OUTSTANDING SERVICE BY A MEMBER OF THE INTELLIGENCE COMMUNITY. THE SERVICE MUST RELATE DIRECTLY TO THE INTELLIGENCE COMMUNITY'S MISSION TO PROVIDE THE INTELLIGENCE REQUIRED FOR NATIONAL SECURITY POLICY DETERMINATION.

ORDWAY HILTON AWARD

AMERICAN ACADEMY OF FORENSIC SCIENCES - QUESTIONED DOCUMENT SECTION, 2/2001.

THE ORDWAY HILTON AWARD IS PRESENTED IN RECOGNITION OF OUTSTANDING CONTRIBUTIONS TO FORENSIC DOCUMENT EXAMINATION.

ALUMNI ACHIEVEMENT AWARD, 2005

SOUTHERN ILLINOIS UNIVERSITY, CARBONDALE, (SIUC) COLLEGE OF EDUCATION AND HUMAN RESOURCES, COMMENCEMENT, 5/14/05.

THE ALUMNI ACHIEVEMENT AWARD IS THE HIGHEST HONOR CONFERRED BY THE SIUC ALUMNI ASSOCIATION.

Comments by Gabriel Barkay

Full Version

Gabriel Barkay

I would like to bring up ten points that I think people should support. These ten points are of general attitude toward inscriptions, whether authentic or whether forged.

The **first point** is the issue of expertise and importance of knowledge, expertise and experience in the subject. It is impossible that the petty employee of the Antiquities Authority that doesn't know a thing will have his view expressed on methods of inscriptions, without being an expert. It is impossible for people who never saw an ossuary to express their view about an ossuary inscription without knowing anything about any other ossuary inscriptions. Just dismissing the view of an expert like André Lemaire who is, to my humble view, one of the most experienced and one of the most sharp-eyed epigraphers in the world, this is something which ought not to be done. The importance therefore of experience and expertise is the first point.

Second point: The primary responsibility in establishing the authenticity of an inscription is by people from the humanities: epigraphers, paleographers, linguists, historians and archaeologists. In this place, I represent the archaeologists, who are able to see the complete picture. They are usually the first to see an inscription if it comes out of the dirt. They can see the stratigraphic connections. The dating of objects is usually done by them. It is not a matter for the geologists, the geomorphologists, the chemists or the physicists. Their opinion is secondary to the opinion of people coming from the humanities.

I had here a discussion with Prof. Porten, who said that if one has an earache, he goes to a specialist. I say no, he should go first to a GP, to a general practitioner, and then he may find out maybe it is caused by the nerves, maybe it is psychosomatic and he should see a psychiatrist. In any case, I think that the GP in this case is the archaeologist. The contribution of the material sciences is crucially important, but secondary. Paleography, linguists' ideas, historians ideas, the analysis of epigraphers is the most important.

That is also true concerning the two geologists, Amnon Rosenfeld and Shimon Ilani, who published the Yehoash inscription in a geological journal. Again, this is something that should not be done. The expertise and the experts in inscriptions are those who should publish inscriptions and not geologists--with all due honor to geology and geography. I myself studied geology. I honor those people, but inscriptions are a cultural product and it should be dealt with by people who deal with ancient civilizations and ancient culture.

Point number three is that the fixing of authenticity of objects and especially of inscribed objects is also a matter of time, not only of expertise. Sometimes there are conflicting views of different scholars and one should give it time. Sometimes time cures all kinds of diseases. We should know that the Mesha Stone, the Moabite Stone, which was discovered in 1868 by F.A. Klein in Dibon in Transjordan was regarded by some as a forgery for ten years after the discovery. There are later examples, but these are exceptions. The Dead Sea Scrolls were regarded either as a modern forgery or medieval forgery for quite a long time. They were regarded as medieval manuscripts or modern forgeries by some for a period for more than ten years after their discovery and some held to these views even later. So time cures and one has to give time to certain things.

The usual procedure in establishing authenticity is via articles, pro and con--not by any other means. Finally, those that convince others or those that are more convincing, they are going to win. They should publish articles, pro and con and not any other way.

The next point is that no committee and no court can establish the authenticity of an inscription. If a committee made up of the most important experts in the world decides that the earth is flat, it is not going to make the earth flat. So the decision of committees is neither originating in the Israeli law of antiquities, nor is it a procedure which is common in scholarly methods. Committees I think are good for communist regimes. They establish a view that has to be accepted by all, but they are not for scientific methods.

So, too, is the court. Only in the case where there is somebody who admits that he forged an inscription or somebody testifies that he saw somebody, he saw with his own eyes somebody forging an inscription, then we can accept the testimony. But the existence of a forged object in a collection doesn't say that all of the collection in the hands of that suspected person, all the objects in the collection of that person who has also forgeries, are forged. That is jumping to an incorrect conclusion.

I think that the court will not be able to establish the authenticity of inscriptions. That is not the job of a court and even if the court is influenced by one expert more than the others and has a tendency to believe one of the experts more than the other experts, it does not give credibility to that view. I think that it is not a matter of neither a committee nor the courts. Just imagine that every scholarly disagreement will be decided by committees. If the dating of Lachish III or low chronology of Finkelstein would be decided by committees, then I don't know what would be the end. This is not the way to deal with scholarly matters.

Number five: The TGTBT [too good to be true] principle, I think, is not acceptable. I'll give you an example. Let us say a new Dead Sea Scroll surfaces. It is a new text. It is, let us say, something that is better than the Temple Scroll. It is too good, very good. Would you use this principle of too good to be true? You are not going to use it.

Lots of goodies are discovered in archaeologist excavations. About a five-minute walk from where we are sitting right now, I had an excavation in which we discovered the two tiny silver scrolls which include a version of the Priestly Benediction. The discovery for sure is too good to be true.

I think that one should not dismiss an inscription just on the basis of too-good-to-be-true. Again, Hanan [Eshel] is correct that one should consider every case for itself, that one should consider all the aspects of an inscription. But the principle of too-good-to-be-true I think is one that should not be a major one in the considerations when one tries to establish the authenticity of an inscription.

Point number six. There is immense importance to context and to a controlled excavation. But also in controlled excavations there are surprises and there are objects which do not fit the stratigraphy. I can site you hundreds of cases. I mentioned one of them yesterday, here, an Assyrian inscription of Sargon II which was found in an excavation. Everything is said to be found in excavations. You cannot be personally in all excavations. I checked the literature even before DeVaux. I understand you [André Lemaire] checked a 1936 article in JPOS. I checked earlier articles. I checked also the reports and the manuscript of a doctoral thesis on the subject under the guidance of Pere Louis Vincent. I checked all the details of that inscription. I tried to find out whether there is anything authentic. I even carried out a detective investigation of who could have planted that inscription there, and I found the name. I know the man who planted it. I think I know. In any case, it was planted into an excavation carried out by the sisters there. Allegedly.

I can give you yet another example. In 1974 or 1975, you [André Lemaire] participated with me in the excavations of Area S in Lachish. We went to breakfast. After breakfast we came back on the floor of level III of Lachish there was lying a coin of the Late Roman Period. I didn't know what to do with it. It was lying within the ashes of the destruction of Sennacherib, a late Roman Period coin. Only later, I discovered that it fell from the upper part of the section and it landed on the destruction level while we were at breakfast. Anomalies exist also in excavations. Even excavated objects coming from a well-controlled excavation can have problematic circumstances and can create problems. So *a priori* we should not dismiss an inscription just because it doesn't originate from a controlled excavation.

I have heard some very extreme views in Tel Aviv University that anything coming from the market or from collections is to be suspected as a forgery. That is also the attitude of some of my American colleagues who refrain from publishing anything that is unprovenanced. I think this is not our goal, not our aim, not our purpose. Scholars are there in order to enhance knowledge about past civilizations and whatever can help the knowledge of past civilizations should be published, whether it comes from here or whether it comes from there. The obligation of scholars is to publish and in this matter there is a big difference between somebody who doesn't sit here [in the excavation], Yosef Naveh sits at home and waits for inscriptions to land from heaven upon his desk or land from colleagues upon his desk. André Lemaire chases the inscriptions and I think he does correctly. I think he does what everyone should do to look for as many hints as possible that may enhance our knowledge about the past.

The Gezer calendar was discovered in 1907 by Macalister in his own dump. Also the Shishak Inscription from Megiddo was found in the dump. The Gilgamesh Fragment from Megiddo was found by a shepherd from Kibbutz Megiddo in the dump. So some of the most important inscribed pieces come from dumps and I can list many other precedents, which are known to me. In the excavations themselves, there are problems. On the well-controlled and even exactly executed excavations, there may still be problems. Also objects which stem from non-controlled sources can have an immense contribution, an important contribution, to make. Unfortunately, they were not found in controlled excavations.

Point number seven. The existence of linguistic and paleographic anomalies is not a reason to dismiss inscriptions and to say that they are fakes or forgeries. The archaism of the Tell Fakhariyeh inscription is a good case. Paleographically it does not fit in the time in which it belongs. I think that if an inscription would be found in Oded Golan's collection in the 1870s and not in the 1880s and including the phrase "*zedah*" [in the Siloam Inscription], that there was a fissure in the rock or something like that, the linguists would say, 'Well, there is no word like "*zedah*.'" "It was made up by a forger. There is no such animal because I didn't see it before in the zoo." This attitude is, I think, incorrect. There are anomalies in provenanced inscriptions and every ancient inscription actually has some peculiar characteristics of its own, some of which do not fit the rules and laws of either linguistics or paleography. Every inscription is a human-hands product, a product of the human mind, and as such it has its own peculiarities, as we all have our own characteristics.

Point number eight. The *a priori* assumption should be that all scholars dealing with inscriptions and publishing inscriptions have personal integrity. I cannot say that something is said to originate in an excavation when the excavator states that it was found in this and that level in this and that locus. I have to accept his word for that. The personal integrity should be accepted *a priori*. Until it is not proven the opposite--that

somebody is a crook and a liar and is dishonest--I think we should accept all scholars around this table and those who are not around this table as honest people.

I should say that spreading rumors about some people who published ancient inscriptions are, how shall I say, very cruel and totally unjustified. There are some cases in which people spread rumors about money that is earned by Andre Lemaire. I can tell you that there are rumors. You know about it that there are rumors that money is involved in this business. Again, I'm most serious about it. I'm very serious about it and I'm very cross about it. We all should be. I think *a priori* we should accept the word of a scholar and the personal integrity should be an *a priori* assumption for all of us; and spreading of such rumors is I think, something that ought not be done.

Point number nine. I think the scholarly community should agree that everyone of the members of the scholarly community is an honest person unless otherwise proven. Unfortunately I know my friend Hanan [Eshel] was a victim of such rumors and such accusations, not concerning forgeries, but concerning something else. We should all fight these methods.

The work of forging an inscription, counterfeiting of inscriptions is something which is a multi-disciplinary work. It is involved with the technical know-how of engraving of inscriptions. It is involved with choosing the proper raw material, proper type of stone or other raw material for the inscription, ivory or such. It has to be involved with deep knowledge in history, in paleography, in epigraphy, in linguistics, in Biblical studies, in chemistry and in geology. I don't see such a person, neither around this table who could be a forger having all of these capacities and I don't see anybody existing in the world who would have all that knowledge, both the technical knowledge of executing the carving of the inscription, as well as the [scholarly knowledge].

I don't think a person could master all those fields together. In antiquity that was obvious for somebody to inscribe an inscription in his own language, own script, own cultural milieu. To penetrate a cultural milieu as an outsider from our times, this requires much knowledge. It requires depth in scholarship. The assumption is that today it could be done only by a team. It has to be teamwork. The assumption should be that if it is a teamwork, there has to be some leak. It is very difficult to organize a group of people, one an expert on Biblical text or Biblical history or Biblical language, the other one an expert on paleography, and the third one as expert on geology and engraving in stone. The community of scholars and the community of people able to do such things is very limited. Actually, I made myself a list of all the possible candidates that could have forged an inscription. I made myself a list and I ruled out the names after I found out that that person is not capable of doing one of the aspects. I was left with no one on the list. I don't think that there is such a person who could forge such an inscription like the [James] ossuary inscription. If the same person is responsible for the forgery of the Yehoash inscription and the ossuary inscription, he has to be a superman, because he is an expert on the cursive script of the Jewish alphabet of the Second Temple Period, he is an expert on ossuaries, and ossuary inscriptions, he is an expert in biblical studies, he is an expert in almost everything else.

In order to forge an ossuary inscription, you have to have a profound knowledge in the cursive writing of Jewish script of the Second Temple period. You cannot do the last words without having an expertise in that. Second, you have to be fluent and knowledgeable in Aramaic to write *Achui d'Yeshua* and not *achu de Yeshua* or something like that. You have to have some knowledge. You have to have the expertise. More than that you have to have [ability to engrave]. In [L.Y.] Rachmani's book, there is

only one case of *achui*. It is an exception. I assume other forgers will go after something that is well known, that is not exceptional. Just to choose one exception (which is problematic, by the way) to follow that, I don't know.

Any forgery, any forger, usually has to base his product on something. If it is too original, then I have a problem with it.

I'll come back in **the tenth point** to one of the points that I mentioned earlier: the time factor. I think that in archaeology the time factor is important. I'll give you an example, again, from yet another field in archaeology: The burial caves in the backyard of the St. Etienne Monastery of the Dominican fathers. The caves were excavated in the 1880s. They were regarded to be of the Roman Period. I think only after [Nahman] Avigad found the Broad wall in the Jewish Quarter, time became ripe looking for Iron Age remains further north in Jerusalem. There is a certain maturity that scholarship has to arrive at and then draw conclusions. The time, to my humble view, has not yet arrived to cast very clear judgment about the inscriptions which were discussed here. It is true that historical and significant inscriptions are very rare. I think that with further discussion, with further studies, with further analyses of the material sciences and with further contributions of the team around this table and maybe further discussions such as this symposium, here, with time, we are going to be able to cast a better judgment.

Thank you very much.