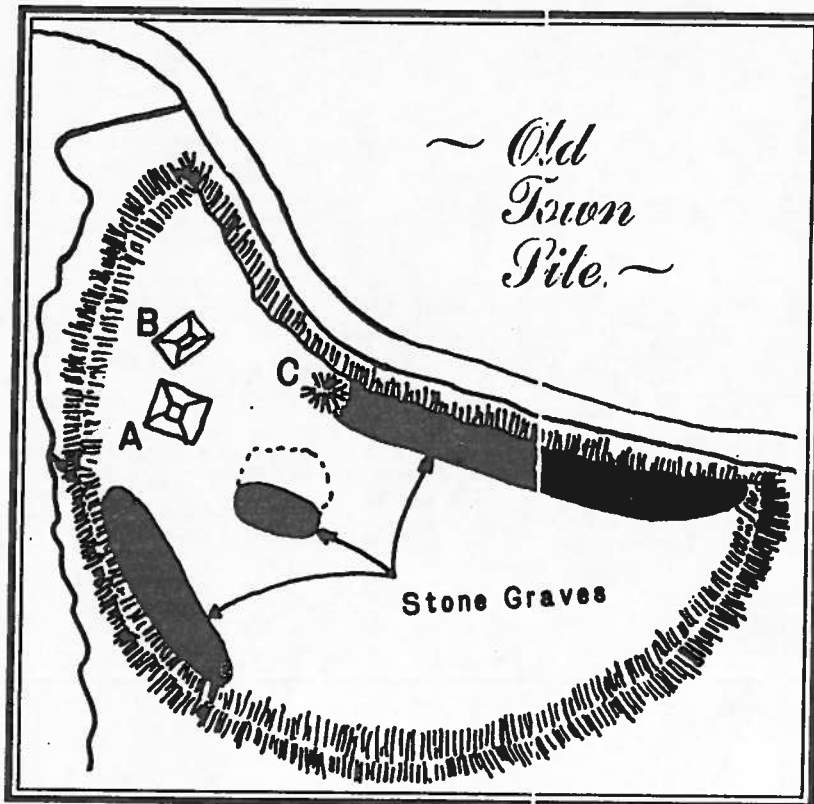


TENNESSEE ANTHROPOLOGIST

JOURNAL OF THE TENNESSEE

ANTHROPOLOGICAL ASSOCIATION



VOLUME XVIII, NUMBER 1
SPRING
1993

THE OHIO STATE UNIVERSITY LIBRARIES
received on: 07-09-93
SER 9M
7873745
C. 1

THE BAT CREEK STONE: A REPLY TO MAINFORT AND KWAS

J. Huston McCulloch

ABSTRACT

Mainfort and Kwas (Tennessee Anthropologist 1991), in a comment on my own 1988 TA article, criticize the Bat Creek stone and Cyrus Gordon's identification of the script on it as 1st or 2nd century A.D. Paleo-Hebrew on several grounds. They argue 1. that the inscription is not Paleo-Hebrew, 2. that the brass bracelets are in all probability modern trade items, 3. that the radiocarbon date I reported is unreliable, 4. that Cyrus Thomas indirectly denounced his own Mound Explorations report as containing fraudulent artifacts, including the Bat Creek stone itself, and 5. that the Smithsonian agent who found the stone was particularly unreliable. The present reply refutes these arguments and adds new information concerning the patina of the letters and the precedents for the word divider.

Introduction

In their recent article, "The Bat Creek Stone: Judeans in Tennessee?" (1991), Robert C. Mainfort, Jr. and Mary L. Kwas comment on my own earlier article (1988) on the Bat Creek stone (National Museum of Natural History 134902). This small inscribed stone was found in 1889 in a burial mound on the lower Little Tennessee River by a technician working for the Smithsonian Bureau of Ethnology under Cyrus Thomas (Thomas 1894: 392-4). Although many professional archaeologists and Semitists have privately expressed doubts about the antiquity and/or authenticity of the stone, or about Cyrus Gordon's (1971, 1972) identification of the exotic characters on it as a 1st or 2nd century A.D. Paleo-Hebrew graffito, they have, at least until Mainfort and Kwas's article appeared, allowed Thomas's account and Gordon's analysis to go virtually unchallenged in print. Whatever case there may be against the claims that have been made concerning this unusual artifact deserves to be presented out in the open, as Mainfort and Kwas have done, rather than behind closed doors. Mainfort and Kwas are particularly to be congratulated for having elicited comments from Frank Moore Cross on the Paleo-Hebrew aspects of the artifact.

The principal arguments presented by Mainfort and Kwas are: 1. The brief inscription on the stone is not Paleo-Hebrew, as identified by Cyrus Gordon. 2. The brass bracelets found with the stone are in all probability modern trade artifacts. 3. The association of the radiocarbon-dated wood fragments with the inscribed stone is tenuous. 4. In 1898 Cyrus Thomas discretely denounced his own 1894 Mound Explorations report as containing imaginary earthworks and fraudulent artifacts, and the Bat Creek stone was one such fraudulent article that Thomas had in mind. 5. The Smithsonian agent who excavated the stone was particularly unreliable.

The present reply to Mainfort and Kwas's comment refutes these arguments one by one, and goes on to add some new materia! concerning the patina on the inscription, and regarding the word divider.

The Inscription

Mainfort and Kwas readily concur with Joseph Mahan (1973) and myself that Cyrus Thomas (1894:393) and Marshall McKusick (1979) were wrong to identify the inscription on the Bat Creek stone as Cherokee. In 1991, McKusick was still alluding to the inscription as "a scrap of 1820s Cherokee syllabary" (1991:156). However, he gave no new information to support this claim, and made no mention of my 1988 article which had extensively and expressly refuted his 1979 position. A consensus may therefore be said to have formed, at least among informed parties, that the inscription is not Cherokee.

If it is not Cherokee, then, what is it? According to Cyrus H. Gordon, recently retired Professor of Hebrew and Other Oriental Languages at New York University, and a leading expert on the pre-Hebrew Semitic inscriptions of Ugarit and Ebla, the inscription is in Paleo-Hebrew script of the 1st or 2nd centuries A.D. Although not entirely clear, it contains the word *lyhwd*, or "for Judea," in Hebrew language.

In their critique of Gordon's interpretation of the inscription as Paleo-Hebrew, Mainfort and Kwas disclaim any knowledge of the Hebrew language or scripts, and instead rely entirely on the views of Frank M. Cross, Hancock Professor of Hebrew and Other Oriental Languages at Harvard University. Cross was a logical person to approach, since he is a leading authority on ancient Hebrew scripts, and since he has crossed pens with Gordon in the past concerning other allegedly pre-Columbian Semitic inscriptions reportedly found in the New World (Cross 1968). Cross gave his considered opinion on the Bat Creek inscription in two related letters, one to Mainfort, (1989a) and one to myself (1989b), with copies of each to both of us. Mainfort and Kwas quote extensively from the letter they received. They report that Cross read and commented on an earlier draft of their paper, so we may assume that his views have been represented accurately, and with his permission.

Unfortunately, Professor Cross makes no less than three elementary and readily documentable errors of Hebrew paleography, both in his letters and as accurately quoted and paraphrased by Mainfort and Kwas.

Cross's first clearcut error is his statement that the E-like letter I designated in my article as letter iii, and which Gordon identifies as a Paleo-Hebrew he, "is impossible as Paleo-Hebrew in the period 100 B.C.-A.D. 100, based on shape and stance." In his letter to Mainfort, Cross praises Mark McLean's Harvard doctoral dissertation (1982) as "the best treatment of the Palaeo-Hebrew script" available. Indeed, Cross served on McLean's committee and signed off on the cover page. However, if we open McLean's dissertation to his Plate 13, we find the letter reproduced in Figure 1a, which is essentially identical to the Bat Creek letter iii, identified as being a he from a bulla (seal impression) of a King Jonathan. Furthermore, according to

McLean's source, Avigad (1975), this particular King Jonathan would be Alexander Jannaeus, whose Hebrew name was Jonathan, and who ruled Judea from 103-76 B.C. Bat Creek letter iii, far from being "impossible," as Cross assured Dr. Mainfort, is therefore in fact a perfectly acceptable Paleo-Hebrew letter for the late Second Temple period, including, specifically, the first century B.C. expressly denied by Professor Cross. //

The letter he appears with essentially the same stance as the Bat Creek letter on the coins of the first century A.D. that I cited already in my Table 3 (1988:90), though with a very formal and inessential (as demonstrated by Figure 1a) overhang of the top crossbar, and with purely ornamental dots or "pearls" at the ends of the lines. The Bat Creek letter is rather sloppy, but is just as readily identifiable as he in this orientation as it would be as an English "E" or Cherokee gun when inverted. The reason the Bat Creek letter looks so much like a backwards E is that our E was originally just a backwards Canaanite he. The Bat Creek form would be out of place in a much earlier period, such as the 9th century B.C., but works fine as Paleo-Hebrew circa the 1st century A.D.

Cross's second clearcut error is his assertion that "The broken sign [to the left of the letter I designate as i] cannot be mem in the designated period [which in his letter is the 1st century B.C. - 1st century A.D.]..." (1989a) Now in his other letter, Cross urged me to consult Freedman and Mathews' book (1985) on 11 Q paleo Lev (the paleo-Hebrew Leviticus Dead Sea scroll from Qumran cave 11), which he describes as "the largest script in paleo-Hebrew and the best preserved MS." However, if we turn this volume to its Plate 19, we find the three specimens of mem reproduced in Figure 1b. These are particularly clear examples of the mem used throughout this scroll, as indicated in the volume's summary Table III. A mem of this type would fit the broken Bat Creek letter very well. Furthermore, Richard Hanson dates this particular scroll to approximately 100 B.C. (in Freedman and Mathews 1985, p. 23). The same form of mem also appears on coins of Hyrcanus II, 63-40 B.C. (Birnbau 1971: Chart 51*). The broken letter is therefore a perfectly good mem for the period Professor Cross designates, contrary to his assurances to Dr. Mainfort.

Cross's third clearcut error is his statement that "if we limit the 'deciphered' text to Gordon's lyhwd, ignoring the following broken sign, the reading would be anomalous. In Palaeo-Hebrew, Judah (Judea) is spelled yhw dh, not yhwd. The latter is the Aramaic designation and appears only in Aramaic scripts," (Cross 1989a).

In fact, however, the Paleo-Hebrew script was used to write both Hebrew language and Aramaic language, as evidenced by the "Abba" inscription, a 7-line tomb inscription found carved into the bedrock of Jerusalem (Naveh 1973; 1982: 120-21, plate 15). Despite the fact that the script of the Abba inscription is Paleo-Hebrew, Joseph Naveh identifies its language as Aramaic, not Hebrew. There is therefore no reason why the word yhwd could not appear in Paleo-Hebrew letters on the Bat Creek stone, even if it is a purely Aramaic idiom. // Indeed, the very word yhwd does appear, in Paleo-Hebrew letters, in line 6 of the Abba inscription, as reproduced in Figure 1c. In the Abba inscription, the word yhwd is used as a personal name. However, it a matter of little consequence whether the reference on the Bat



a.



b.



c.

Figure 1.

- a. Paleo-Hebrew he from a bulla of King Jonathan, 103-76 B.C. (McLean 1982: Plate 13)
- b. Specimens of Paleo-Hebrew mem from 11 Q paleo Lev, circa 100 B.C. (Freedman and Mathews 1985: Plate 19)
- c. The Aramaic word yhwd, in Paleo-Hebrew Letters, from the "Abba" inscription. (Naveh 1982: Plate 15)

Creek stone is to a person named Judah, to the land of Judah, or to the people of Judah, or whether its language is Hebrew per se or Aramaic. Any which way, it indicates a Roman-era contact between the Old and New Worlds.

In addition to the above three points, Cross also objects to my use (1988: 93) of a fourth century B.C. text that he himself had documented (1969: Fig. 35) to strengthen Gordon's identification of letter ii as waw. It should be noted that Naveh (1973: 87) uses this same specimen to identify the unique waw of the Abba inscription, shown in Figure 1c as the third letter when read from right to left. This is despite the fact that the fit is much worse than in the Bat Creek case, and even though he has to stretch to 3rd century A.D. and even later Samaritan inscriptions for parallels to some of the other letters. Naveh's best guess for the date of the Abba inscription is the 1st century A.D. or late 1st century B.C.

Cross raises several other, basically valid objections to the letters and orthography of the Bat Creek inscription. However, every one of these problems has already been noted, and in some cases even resolved, by Gordon, in his definitive 1972 treatment. Cross has therefore added nothing to Gordon's interpretation, other than his overall assessment (which admittedly deserves note) that the specific problems Gordon had already identified are too serious for the inscription to be Paleo-Hebrew. /3/

Mainfort and Kwas, following Cross (1989a), object that "Gordon's interpretation of the Bat Creek inscription could justifiably be criticized on the grounds that his zeal to make a case for the radiation of higher culture from a single Near Eastern center caused him to relax the disciplines of historical linguistics, paleography, and historical orthography" (1991: 6). I have demonstrated above that it is Cross, not Gordon, who is shooting from the hip, at least when it comes to the Bat Creek inscription. Gordon's (1972) claims about Bat Creek generally check out, while what Cross says often does not. Gordon did make a few outright errors, that I have already pointed out, such as his reading of the shape of letter ii and his assumption that the two vertical strokes were part of the original inscription, but these errors actually weakened the case for Paleo-Hebrew. Where he disagrees head-on with Cross (as on the above three points), Cross is documentably wrong.

If anyone has permitted himself "to relax the disciplines of historical linguistics, paleography, and historical orthography" in the present instance, it is therefore Cross himself, and not Gordon. Cross's errors do not, of course, reflect directly on Mainfort and Kwas themselves, since they were justifiably relying on what they could reasonably have expected to have been an authoritative outside opinion. Given the numerous and demonstrable elementary errors in Cross's response, however, readers would do well to seek out additional qualified opinions before leaping to any conclusions about this unique artifact.

One additional expert who has looked at the stone is Professor P. Kyle McCarter of the Johns Hopkins University Dept. of Near Eastern Studies. McCarter, an expert on the ancient Hebrew "Copper Scroll" from Qumran, has given me permission to quote him to the effect that while he reserves final judgment on the inscription, he believes that it looks too much like

Paleo-Hebrew to be a mere coincidence. At the same time, he finds that it lacks the sophistication found in most forgeries. (Personal communication.)

In their concluding remarks (p. 14), Mainfort and Kwas add the astonishing statement that their conclusion that "the inscription is not a legitimate Paleo-Hebrew inscription" is "based on assessments by two Near Eastern language specialists," one of whom is Cyrus Gordon. In fact, Gordon is clearly on record (1971, 1972) that despite its admitted problems, he is confident that the Bat Creek inscription is Paleo-Hebrew. This is as much a misrepresentation of Gordon's views as it would be of Cross's to take his admission that letter iv bears a "striking resemblance to Paleo-Hebrew script," and that letter v is "normal," as an assessment on his part that the inscription as a whole is Paleo-Hebrew.

The Brass Bracelets

Mainfort and Kwas (pp. 7-9) also readily concur with me that the composition of the brass bracelets (NMNH 134898) found with the Bat Creek stone is not by itself conclusive as to whether they are ancient or modern. Brass is an artificial alloy of copper with zinc that became common in the Old World after 45 B.C., but which is not believed to have been made in the New World before Columbus. /4/ Even if there had been no inscription found with the Bat Creek burial, the brass bracelets, together with the radiocarbon date discussed in the next section, therefore constitute evidence of either a pre-Norse Old World contact with the New World, or else, equally remarkably, of the independent invention of brass in the New World.

Mainfort and Kwas object, however, that "importantly, no documentation regarding the production and use of comparable artifacts by first or second century A.D. Mediterranean peoples has been presented" by either myself or others, and deduce that "application of Occam's Razor strongly suggests a relatively recent European origin for the bracelets from Bat Creek" (p. 9).

While it is true that I did not actually cite any evidence for C-shaped bracelets being used in ancient times, they were in fact a popular ornament in the Mediterranean world, from the early Iron Age down to Byzantine and Islamic times; see Richter (1915: 336-7, #1115), Comstock and Vermeule (1971: 188, #222), Davidson (1952: 263, Plate 112, #2133, #2134), Holum et al. (1988: 210, Fig. 151). Richter (1915: 336) notes that simple types such as her #1115 were in use for a long time, and that in the absence of ornamentation they provide no clue as to date. /5/ The ancient specimens I have been able to find to date are all bronze, silver, or gold, though as noted above, brass was also available after 45 B.C.

Mainfort and Kwas concede that most modern brass trade bracelets were cut from mass-produced drawn wire rather than being laboriously hand-wrought as were the Bat Creek bracelets. Indeed, the "Tunica Treasure" of eighteenth century trade goods from Louisiana that they cite even contains rolls of the uncut wire (Brain 1979: 193-4). They conjecture that some of the heavier Tunica bracelets may have been wrought. If this were true, it would by no means prove that the Bat Creek bracelets are modern, but it would be an interesting consideration.

However, they do not cite a single one that is actually known to have been wrought and not drawn or cast.

Mainfort and Kwas make a point of attempting to exonerate Cyrus Thomas for having mistakenly identified the brass bracelets as merely "copper." For some purposes it may make little difference whether an object is brass or pure copper, e.g. the modern kettle lugs they cite from Fort Michilimackinac. A major issue that was to be addressed and resolved by the Smithsonian's Mound Survey, however, was whether or not the builders of the ancient North American mounds had access to advanced metallurgy, including the ability to smelt and alloy copper and iron. Thomas concluded, on the basis of the mass of evidence his project found, that there was no evidence of such advanced metallurgy, and that all copper and iron artifacts in the mounds could be explained away as native copper, meteoric iron, or modern intrusions. The Bat Creek bracelets prove, if nothing else, that in at least one instance Thomas misidentified brass as unalloyed copper. This legitimately casts doubt on his identification of the many other "copper" artifacts his study found. Note that had it not been for Gordon's identification of the script on the stone as Paleo-Hebrew, the Bat Creek bracelets would never have been analyzed, and would have remained "copper" to this day.

Robert N. Anderson of the San Jose State University Materials Engineering Department has further developed the copper alloy age-dating methods I mentioned in my original article (p. 107), and now reports success using merely the electrical conductivity of the article, in place of PIXE spectrometry (personnal communication). His current method is completely non-destructive, except for two pin-pricks made by the electrodes through the patina. If Mainfort and Kwas wish to demonstrate that the bracelets are modern, despite their mound context and the radiocarbon date, they could evaluate the reliability of Anderson's method, and then have it applied to the Bat Creek bracelets. Since I have obtained the radiocarbon date, however, the burden of proof is now with them.

The Radiocarbon Date

In my article (pp. 107-110), I reported an AMS radiocarbon date (Beta-24483 / ETH-3677) on wooden disk fragments (NMNH 134899), shown in Figure 2, that were found with the inscribed stone and brass bracelets. The dendrocalibrated date was A.D. 427, with a 2s band (corresponding to a 95% confidence interval) of A.D. 32 - A.D. 769. These disks are apparently portions of earspools, similar in style to those worn by the Etowah figures (Thomas 1894: 304-307).

As Mainfort and Kwas would have it, this date "was obtained on fragments of preserved wood that were recovered during the removal of the burial with which the inscribed stone was allegedly associated (McCulloch 1988) [emphasis added]." Note that my own paper is the only source they give for this "alleged" association. They continue that, "while it is possible that the recent AMS determination accurately dates the burial, McCulloch's claim that the date 'rules out the possibility of a modern origin for either the inscription or the bracelets' (1988: 116) is not only erroneous, but also represents a characteristic, non-skeptical, cult archaeology assertion



Figure 2. The Bat Creek wood fragments (NMNH 134399). Courtesy of the Smithsonian Institution, Smithsonian photograph 89-13231.

about a topic in which he has no expertise." They cite an authority of whom I was admittedly unaware, namely Wendy Hanford Arundale (1981), who "has offered a number of precautions relative to the interpretation of radiocarbon dates. Many of these are pertinent to the Bat Creek stone, but of particular importance is the degree of association between the dated material (in this case, the 'polished wood' fragments) and the cultural event to be dated (in this case, the burial of an individual with which the inscribed stone was purportedly associated) [emphasis added], as well as the age association between the dated material and the associated remains. In the case of the former, the primitive excavation and recording techniques employed render the certainty of association between the wood fragments, the inscribed stone, and the skeletal remains indeterminant (or at best very tenuous)." (1988: 9)

Mainfort and Kwas thus make it appear that although the wood fragments were "recovered during the removal of the burial," there is an "indeterminant (or at best very tenuous)" association between them and the burial itself, and that it is only I who "allege," and "purport," as they put it, that the inscribed stone and/or bracelets were in any way associated with the wood fragments and/or burial.

In fact, Cyrus Thomas's official *Report on the Mound Explorations of the Bureau of Ethnology*, which constitutes the bulk of the *Twelfth Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution, 1890-'91*, is quite clear as to the degree of association between the dated material, the burial, the bracelets, and the inscribed stone:

[Bat Creek] Mound 3 was of small size, measuring but 28 feet in diameter and 5 feet in height. Some large sassafras trees were standing on it, and the owner, Mr. Tipton, stated that he had cut trees from it forty years ago, and that it had been covered by a cluster of trees and grapevines as long ago as the oldest settler in the locality could recollect. At the time the excavation was made there was an old rotten stump yet on the top, the roots of which ran down to the skeletons. It was composed throughout, except about the skeletons at the bottom, of hard red clay, without any indications of stratification. Nothing of interest was discovered until the bottom, where nine skeletons were found lying on the original surface of the ground, surrounded by dark colored earth. These were disposed as shown in Fig. 272. No. 1 lying at full length with the head south, and close by, parallel with it, but with the head north, was No. 2. On the same level were seven others, all lying close side by side, with heads north and in a line. All were badly decayed. No relics were found with any but No. 1, immediately under the skull and jaw bones of which were two copper bracelets, an engraved stone, a small drilled fossil, a copper bead, a bone implement, and some small pieces of polished wood. The earth about the skeletons was wet and the pieces of wood soft and colored green by contact with the copper bracelets. The bracelets had been rolled in something, probably bark, which crumbled away when they were taken out. The engraved stone lay partially under the back part of the skull and was struck by the steel prod used in probing. This stone is shown in Fig. 273 [on Thomas's p. 394]. [Thomas 1894: 392-4]

Thomas did not just make up this description of the excavation five years after the event. Rather, he lifted it, almost word for word (without attribution, as was his custom), from the first-hand account of John W. Emmert, the Bureau agent who actually excavated the stone. /6/ This account (Emmert 1889c) was written the month after the discovery. Thomas deleted a few sentences from Emmert's report, and I have already quoted (1988: 104, 108) a few of these. For the sake of completeness, the following should be added: Emmert, in his then extensive experience excavating mounds, found it "a little Singular [sic] that all [the skeletons] were lying with heads north but no. 1 whose head was to the South." Furthermore, although he admitted that "I punched it [the stone] on the rough side with my steel rod in probing before I came to the skeletons," he added that "the other side of the of the stone is exactly as it was taken from the skeleton at the bottom of the mound about five feet deep." There is indeed a still fresh-looking gash on the back side of the stone. Emmert included a sketch of the burial, shown in Figure 3, which was the basis for Thomas's Fig. 272.

Emmert added at the end of his report, "I have packed the Specimens carefully and put the engraved stone in a box Separate as you directed and have made two Catalogues one to you and one to Maj. Powell." Immediately upon receipt of the artifacts, Emmert's catalog numbers, which clearly identified which artifacts in the shipment were the ones described as having come from Bat Creek mound no. 3, were assigned NMNH catalog numbers. The number 134899 assigned to the wood fragments (Emmert's #7) is clearly legible, eleven times over, in Figure 2. It was from one of these pieces that Carolyn Rose extracted a sample to send to Beta Analytic, Inc. for testing. In the color slide of 134899 that is available from Smithsonian Photo Services, the green copper salts from the bracelets, as noted by Emmert and that preserved the wood over the centuries, are clearly visible. /7/

In letters dated 2/15/1889 and 2/25/1889, Emmert had already announced the find to Thomas, and indicated that it had aroused considerable excitement locally. There is therefore no possibility that Emmert inattentively mixed up the artifacts found with the stone with those from any other burials.

Unfortunately, the National Anthropological Archives did not save the actual field notes of Emmert or of any of the other Mound Survey agents in its MS2400, so we have no record of how accurate or primitive they may have been. However, Emmert wrote Thomas (1889b) that "I am taking full notes as I go along," so we at least know that they at one time existed, and that he did not simply write his report from memory a month after the excavation. Given Thomas's interest in, and puzzlement over, the stone (1890: 35-37, 1894: 714), we would expect that he examined these notes most carefully for any discrepancies. As I have already noted (1988: 113), Thomas actually took the unusual precaution of sending another agent, James Middleton, to the field after the discovery to check the details of Emmert's report. His investigation "confirmed the statement by Mr. Emmert in every particular."

In a complex earthwork such as the Citico mound, which Emmert excavated in January through March of 1885, reference to accurate and detailed field notes would be essential in order to be confident of the relationships among the 91 interjected burials and all of the associated

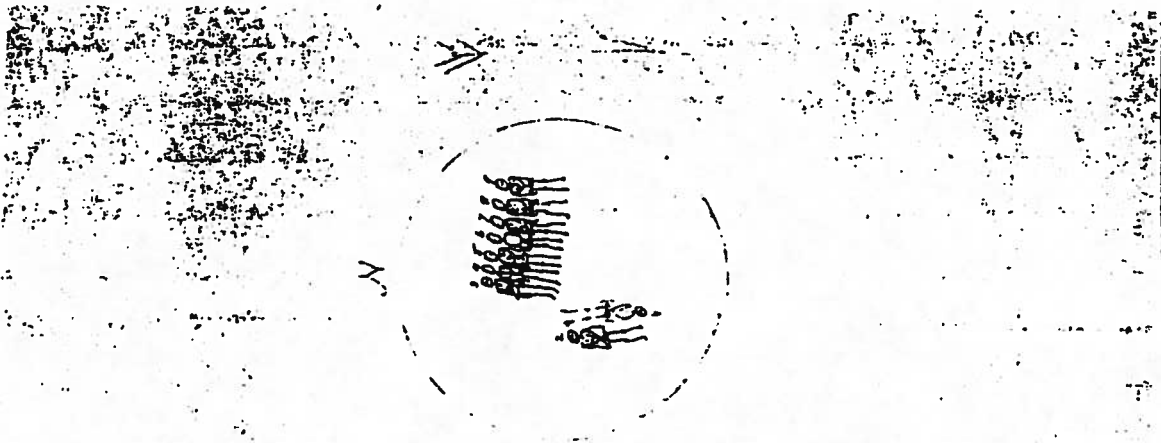


Diagram Mound no 3. Tipton Group showing the position of skeletons.

Figure 3. Emmert's sketch of the Bat Creek burial (1889c). National Anthropological Archives, MS 2400.

artifacts (Thomas 1894: 373-77). However, Bat Creek mound no. 3 had none of this complexity. It would of course be ideal if we could see Emmert's actual field notes, but even without them, the association of the artifacts of interest is adequately clear from his report. There is no reasonable possibility that the wood fragments radiocarbon dated in 1988 were not the same ones reported to have been found 99 years earlier, under the same skull as the inscribed stone and bracelets, and preserved by corrosion that appeared to have come from the bracelets. /8/

As for the age association between the dated material and the associated remains, which Mainfort and Kwas would have me neglecting, I in fact clearly stated (1988: 109) that "the carbon date refers, of course, to the tree growth represented by the wood fragments, and not to the inscription or the contact itself." The tree growth could well have been several decades, or conceivably even a century or two old, if the wood was taken from the heart of a very old tree, at the time of the burial. But even if we add 200 years to the upper end of the 2σ band, we are still left with a pre-Norse, not to mention pre-Columbian, date for the burial. (Vikings began raiding England in the 790s, but Leif Erickson is not said to have reached "Vinland" until approximately A.D. 1000.)

R.A. Taylor (1987: 108-15) classifies radiocarbon dates into three major confidence categories, according to the reliability of the association of sample material with the archaeological feature of interest. In our case, we are interested in the date of burial of the inscription and brass bracelets, as an upper bound to the date at which knowledge of the script and the bracelets themselves were presumably brought over from the Old World. His highest category, #1, has two sub-categories, which he designates as "Essential certainty," and "High probability." We do not have his "Essential certainty" for the inscription or bracelets, since this would require a direct date on the inscription or bracelets themselves. We do, however, have his "High probability" for the burial, which he defines as " ^{14}C analysis on organics in direct functional relationship with [the] object/event for which temporal placement [is] sought." As an example, he gives " ^{14}C analysis of textile used to wrap [a] burial to obtain [an] age estimate on [the] burial." Grave goods, such as earspools placed with a skeleton in the Bat Creek case, have a comparable degree of association with the burial. We may therefore say that according to Taylor's classification, the Bat Creek radiocarbon date has a "High probability" of providing an upper bound on the date of the contact. This degree of association is far superior to his category #2, "Reasonable possibility," an example of which would be a date on charcoal in sediments adjacent to the burial to obtain a date on the burial itself.

The article by Arundale [1981], which Mainfort and Kwas found to contain "a number of precautions relative to the interpretation of radiocarbon dates," "many of [which] are pertinent to the Bat Creek Stone," is in fact concerned with the special problems archaeologists confront in dating Arctic sites, "where the skin, bone, fat, ivory, or baleen from a sea mammal may be the only available organic substance." As she points out in her introduction, "Arctic researchers face difficulties with radiocarbon dating not shared by Temperate zone colleagues." (Arundale 1981: 224, emphasis added.) These special problems are irrelevant to the Bat Creek burial.

Mainfort and Kwas do make the valid observation that the "dark soil" at the base of the mound may have been an occupation midden or old humus zone, and add the interesting suggestion that the wood fragments may have derived from this midden or humus rather than being grave goods actually placed with the burial (pp. 5, 9). However, in a midden or humus zone in the Temperate climate of Tennessee, small pieces of wood lying on or near the surface would not last for more than a few years without some artificial means of preservation. It is only the fact that the wood fragments were in direct contact with the cupreous bracelets and ear spoons such as those shown in Figure 2 are much more likely to be intentional grave goods, particularly when found under a skull with other fancy items, than village refuse.

Mainfort and Kwas correctly note that the soil at the base of the mound was wet, and suggest that this "raises the possibility of contamination from groundwater." (p. 9) However, the cover letter on the report from Beta Analytic Inc., signed by Dr. Murry Tamers and dated May 2, 1988, certifies that:

Your wood was pretreated by first examining for rootlets. The sample was then given a hot acid wash to eliminate carbonates. It was repeatedly rinsed to neutrality and subsequently given a hot alkali soaking to take out humic acids. After rinsing to neutrality, another acid wash followed and another rinsing to neutrality.

Contamination by either calcium carbonate or humic acid from groundwater has therefore already been eliminated as a possibility, to the best of the laboratory's ability. The real problem with the radiocarbon date, one which Mainfort and Kwas did not mention, but which I have already raised on p. 108 of my 1988 article, is its unusually large standard error, namely 170 years on the uncalibrated date, whereas 100 years or less would be more usual. Using the Gaussian probability distribution, a 95% confidence interval for the true date extends 1.96 standard errors on either side of the point estimate.

The *reason* for the large standard error was that although 30 mg. of material was reportedly submitted for testing, an unusually small amount of pure carbon remained after the above-described pretreatments. The reasons for this are unclear, but may include: 1. Much of the mass of material submitted may have been the very copper salts from the bracelets that preserved the wood. 2. The wood may have been partially fossilized, further reducing its carbon content. 3. The laboratory admittedly "hit the sample pretty hard" with pretreatments, knowing its potentially controversial nature.

The existing test used up less than 1% of the total 5.5 gm. of material that was available. I personally would favor AMS retesting with as much as 10% of the available material, e.g. one of the larger disk segments shown in Figure 2. This should be more than adequate to obtain a date in which we can have more confidence, while at the same time leaving enough behind for further study and/or display. Again, however, the burden of proof is on Mainfort and Kwas or others to have this done if they wish to check the accuracy of the existing date.

One pertinent recommendation that Arundale (1981) did make (and which Mainfort and Kwas did not mention) is that carbon-dated wood should routinely be identified as to species if at all possible. In the Arctic, for example, willow almost surely rules out driftwood, which can be quite old when used. This should be done (by someone other than myself) with the Bat Creek wood fragments. This may not tell us anything, but if by chance they turned out to be the heartwood of a burr oak, some adjustment for this could be appropriate.

Fantastic Archaeology

Mainfort and Kwas's least tenable argument is that in 1898, Cyrus Thomas personally denounced his own Mound Explorations report as having presented certain fictitious earthworks and recently fabricated artifacts as if they were authentic and ancient, and that the Bat Creek stone in particular was one of the artifacts he thus repudiated. They base this claim on the following passage from Thomas's Introduction to the Study of North American Archaeology:

It is safe therefore to base important conclusions only on monuments in reference to which there is no doubt, and on articles whose history, as regards the finding, is fully known, except where the type is well established from genuine antiquities. One of the best recent works on ancient America is flawed to some extent by want of this precaution. Mounds and ancient works are described and figured which do not and never did exist; and articles are represented which are modern productions [sic]. (Thomas 1898: 24-25.)

From this they deduce:

We believe that the "best recent work" alluded to by Thomas is his own final report on mound explorations (1894), and that the "articles whose history... is fully known" is a reference to the alleged discovery of the Bat Creek stone. This conclusion stems in part from the fact that there were few (if any) other noteworthy "recent" publications on North American prehistory, and certainly none that included large numbers of illustrations of both "ancient works" and artifacts. Moreover, Cyrus Thomas was never shy about naming names, whether by way of praise or criticism. Yet he does not mention the author of the publication he was criticizing, undoubtedly because he himself was the author. This of course begs the question of why Thomas did not admit to the failings of his magnum opus in a more direct manner. With respect to the Bat Creek stone, which we have now demonstrated beyond a reasonable doubt was one of the "modern reproductions" [sic] alluded to by Thomas, we believe that the answer is quite straightforward--Thomas had placed himself in a position such that he could not really afford to pronounce the Bat Creek stone a forgery.

Note that Thomas did not really allude to "the 'best recent work'", as misquoted by Mainfort and Kwas, but rather merely to "one of the best recent works." In Thomas's view, "the

best recent work" could of course only be his own 1894 Mound Explorations, whereas "one of the best recent works" at least potentially embraces the efforts of other authors.

Furthermore, Thomas's words. "mounds and ancient works are described and figured which do not and never did exist," echo his own earlier attack on certain illustrations in Squier and Davis (1848): "Some of the singular works described and figured in *Ancient Monuments* and elsewhere are to a large extent imaginary. Of these we may name Nos. 1 and 2, Pl. XXXIV of that work. The wing to No. 1 is not only imaginary, but, according to the Bureau assistant who visited the locality, was made impossible by the topography" (1894: 566).

Now in 1898, Squier and Davis's 1848 volume, although extensive and still authoritative, could not have been considered "recent." However, had Mainfort and Kwas done their homework, they would have checked Silverberg's extensive bibliography (1968), and found that in 1898 there was indeed another recent publication on North American prehistory besides Thomas's own work, containing many illustrations of both "ancient works" and artifacts, namely the first (1892) edition of Rev. Stephen D. Peet's *The Mound Builders*. And indeed, if we turn to p. 78 of Peet's book, we find an illustration of the very Squier and Davis earthworks that Thomas had earlier denounced as "imaginary." /9/ Furthermore, on pp. 13 and 41 of the same volume, we find illustrations of the famous Davenport, Iowa tablets and elephant pipes, which Thomas was well on record (1894: 632-643) as believing were fakes. Peet not only failed to deny the authenticity of the tablets and elephant pipes, but actually had the temerity to cite new evidence in favor of the pipes.

It was Peet's book, *and not his own*, that Cyrus Thomas was denouncing in 1898. The Mound Survey was precisely intended to have the very virtues he cites as having been to some extent absent in the unnamed book to which he so delicately alluded. The "modern productions" (misquoted by Mainfort and Kwas as "reproductions") alluded to by Thomas were clearly the Davenport artifacts, and not his own Bat Creek stone, however enigmatical he may have found the latter. If he did not call Reverend Peet a fool by name, it was perhaps merely out of deference to the clergy./10/

In an article approvingly cited by Mainfort and Kwas, Stephen Williams (1988: 17) defines "Fantastic Archaeology" as, "when you try to track down the actual sources for the story, it dissolves into nonsense." Mainfort and Kwas are thus indulging in some "Fantastic Archaeology" of their own when they interpret Cyrus Thomas's veiled attack on Peet's book as pertaining to the Bat Creek stone. It is most unfortunate that Mainfort and Kwas's absurd claim was too recent to have been included in Professor Williams' new book by the same name. Williams himself praises Thomas's 1894 volume, Bat Creek stone and all, or so it would appear, as having "for all intents and purposes shut the door, from a scientific standpoint on the Moundbuilder question" (1991: 74).

Nor are Mainfort and Kwas on much stronger ground when they claim (p. 10) that "an extensive review of roughly contemporary and later professional literature contradicts [my own] assertion" that "authoritative contemporaries, who knew the circumstances better than anyone

today, accepted the tablet as genuine" (p. 113). I had cited Thomas and Fowke, who clearly regarded it as a genuine, though Cherokee, artifact. To "contradict" me, as they put it, they cite a string of archaeologists and Cherokee ethnologists who make no mention of it at all, one way or the other! Silence is hardly the equivalent of denunciation.

Mainfort and Kwas do deserve credit for having unearthed a reference to the Bat Creek stone of which I was unaware, by the same Rev. Peet whose book they so carelessly overlooked:

In another mound on the Little Tennessee, two miles from Morgantown, were found nine skeletons, and with one were two copper bracelets, copper beads, a small drilled stone, an engraved stone which had some of the characters of the Cherokee alphabet on it. The argument which Dr. Thomas makes in connection with these finds [in Thomas 1890] is that the mound-builders were Indians, and the particular tribe who built these mounds were Cherokees. The argument is, however, misleading. It may be forcible as proving the migration and the modern character of the Cherokees, but it begs the question as to the other tribes of mound-builders.... The value of the finds consists in the fact that the record of the Cherokees is carried back into prehistoric times and the record of mound-building brought up to modern times; but to make the Cherokees the moundbuilders of the Mississippi Valley is absurd. (1891: 146-147)

Like Fowke in 1902, however, Peet was simply taking Thomas's word for it that the Bat Creek inscription is genuine and Cherokee. If Fowke in 1907 denied the existence of any "ancient article," as quoted by Mainfort and Kwas, north of Mexico with other than pictographic writing, it was simply because he continued to accept Thomas's misguided verdict that the inscription was Cherokee and therefore modern.

John Emmert

Mainfort and Kwas go on to accuse Emmert himself of having forged, or at least planted, the Bat Creek inscription (pp. 12-13). This is implausible on several grounds that I have already enumerated (1988: 112-5), but its major problem is that there is absolutely no indication that the inscription is a forgery in the first place, other than the circular, and therefore unscientific, argument that being Hebrew, it must surely be fake./11/

They argue that Emmert's motive was "that he felt the best way to insure permanent employment with the Mound Survey was to find an outstanding artifact, and how better to impress Cyrus Thomas than to 'find' an object that would prove Thomas' hypothesis that the Cherokee built most of the mounds in eastern Tennessee? As to the specific signs on the Bat Creek stone, several are passable Cherokee, and the inspiration for the remainder could have been any number of published sources, including illustrations of the Grave Creek stone and the Davenport tablets."

In 1889, however, there was no prospect of "permanent employment with the Mound Survey," since the project was almost completed except for a little mop-up work (Smith 1985:

18-19). Furthermore, as Bruce Smith has pointed out, Cyrus Thomas "was not an easy person to work for" (1985: 12). He was tight with expenses, and in one letter to J.W. Powell actually prided himself on driving his staff, including specifically Emmert, "day and night" (1887). Emmert's first assignment as a regular assistant, in January through March of 1885, was to excavate the Citico mound in snow and freezing rain, a task that left him gravely ill with ague, a malaria-like fever. If he had just wanted employment, he could easily have found a less demanding job.

As for ingratiating himself with Thomas by producing a Cherokee mound inscription, I have already demonstrated (1988: 113) that in February of 1889 Emmert could easily have done a fine job of Cherokee if that was what he intended. Yet Mainfort and Kwas themselves readily admit (p. 7) that the Bat Creek inscription is not intelligible as Cherokee. Grasping at straws, they claim that the Bat Creek letters that do not work as Cherokee may have come from the Grave Creek stone and the Davenport tablets, but do not specify which Bat Creek letters come from which Grave Creek and/or Davenport letters. In fact, of the letters that do not work even fairly well as Cherokee, specifically a, c, d, e, and h in my Table 1 and iv, vi, and viii in my Table 2 (1988), only one would rate even a "fair" on my scale when compared to either the Grave Creek stone or the principal Davenport tablet. (This is the very simple letter d, which has a fair match on the upper panel of the Davenport tablet). And even if these letters could be matched, how could Emmert have pleased Thomas by finding a tablet with letters drawn from artifacts which Thomas openly regarded as frauds? Once again, when we try to check their story out, it "dissolves into nonsense."

Mainfort and Kwas do point out (pp. 10, 13) that Whiteford (1952) expressed some doubts as to the accuracy of Emmert's work in Tennessee for the Mound Survey. Whiteford, as quoted by Mainfort and Kwas, specifically questioned certain "non-typical" artifacts, in particular the Bat Creek Stone itself, and four burial types that were either "unique," or never "duplicated" by any "recent investigation."

Now in a discussion of the Bat Creek stone, it is circular to use the Bat Creek stone itself as evidence against Emmert's, and therefore the stone's, credibility. And as for the burial types, it should be noted that the "stone domed vaults or 'stone hives'" from Sullivan Co., Tennessee (Thomas 1894: 352-353), that were excavated by Emmert and to which Whiteford objected, far from being unique, are in fact very similar to those found by Emmert's colleague John Rogan, in Caldwell Co., North Carolina (Thomas 1894: 334). Furthermore, Peet (1903: 223) illustrates yet another such vaulted burial in a mound on the Iowa River.¹² If Emmert's "stone hives" have never been duplicated (or quadruplicated, to be precise) by more recent investigations, does this mean that Rogan's famous (and unique) finds at the Etowah group (Thomas 1894: 292-311) should not be taken seriously either?

Or does it just mean that the Mound Survey excavated and reported on more mounds than any study before or since (over 2000, according to Thomas 1894:23), and therefore had a higher probability of finding unique artifacts and structures? Mainfort and Kwas themselves admit (p. 12) that Emmert alone "personally directed a truly amazing number of excavations at sites in

eastern Tennessee and adjacent areas." Yet the rapidity with which these mounds and earthworks were disappearing forever was noted already in 1884 by F.W. Putnam:

The opportunities offered by these excursions for comparing the present condition of these wonderful monuments of antiquity with their condition when first described have shown how rapidly they are becoming obliterated. A generation of men has not yet passed away since most of these earthworks were in a good state of preservation; our children's children will look for them in vain unless something is done at once to preserve them.... Every year that passes without action is one more year allowed for ploughing over and destroying these wonderful works (1884: 350).

If Whiteford and his contemporaries did not observe anything like some of the less common structures and artifacts reported by the Mound Survey, it is most likely because there were simply none left.

The Patina

An important part of the case against a forgery by Emmert or any one else, however, comes from a careful examination of the patina of the letters, in conjunction with the unusual, comma-shaped word divider that appears between letters v and vi.

I have already demonstrated (1988:96) that two vertical strokes now clearly present on the stone were not on it when it was found. These strokes were added by an unknown party while the stone was in the National Museum of Natural History, at some time between 1894 and 1970. They are clearly visible in the modern photograph reproduced by Mainfort and Kwas (p. 4), but are conspicuously absent in Emmert's 1889 sketch (1889b), in Thomas's 1890 drawing (reproduced in McCulloch 1988: 84), and in Thomas's 1894 photograph (1894: 394).

Gus Van Beek, Curator of Old World Archaeology at the Smithsonian Institution, was aware that these two strokes were modern, and was quoted in *Science Digest* (Ford 1972) to the effect that they appeared to him to have the same patina as the rest of the inscription. He concluded that the original inscription must therefore also be modern, and could not date to the 1st or 2nd century A.D., as claimed by Cyrus Gordon. When I wrote my 1988 article, I had not yet had an opportunity to check this out, but on the basis of the new radiocarbon date concluded (p. 108) that Van Beek must somehow have been mistaken. I have since had a chance to actually examine the stone with Van Beek's considerations in mind.

On examination with the naked eye and with a 2X magnifying glass, there are indeed no obvious differences between the vertical strokes and the brighter portions of the original inscription. In particular, they are of roughly the same brightness in contrast with the dark surface of the stone, and therefore seemed to be about as fresh. (This is apparent in the Smithsonian photograph reproduced by Mainfort and Kwas in their Figure 1.)

Under a low-power (20X) microscope, however, the two groups of marks look very different. To understand these differences, it should be recalled that the stone itself is of a light-tan color, but that it is coated on the inscribed face and edges with a thin, dark brown crust. According to a tag from the USNM Dept. of Paleontology that accompanies the stone, the stone is an iron-rich siltstone, and the crust is an iron oxide. The tag does not indicate which oxide of iron this is, but it is presumably limonite, goethite or hematite, chemically similar minerals composed of ferric oxide with varying degrees of hydration, with yellow to red streaks.

Ferric oxide is semisoluble, so that in a wet environment like that of the Bat Creek burial mound, molecules of it will slowly dissolve into the water. After the solution becomes saturated, the ferric oxide will continue to dissolve, but an equal amount of material from the solution will redeposit on the solid mass. In this manner, any sharp corners will slowly become rounded over time, and separate but adjacent pieces may become fused together.

The original letters are, for the most part, cut entirely through the iron oxide crust into the light siltstone beneath, making the letters stand out very brightly against the dark face of the stone, even without special lighting. In a few places (notably the heads of the letters I identify in my article as ii and vi), the crust was either thicker or the scribe lost patience, and the letters do not penetrate the crust. In these places the original letters are very hard to read or photograph without oblique lighting, and indeed Gordon (1971, 1972) actually misread the shape of letter ii as a result.

Under the microscope, it becomes clear that the two vertical strokes do not penetrate the dark crust, and are bright only because they are the red-orange color of freshly pulverized iron oxide. Furthermore, they have sharp, fresh edges. In the original characters, on the other hand, the red-orange coloration that must originally have been present along the sides of the grooves and in the heads of letters ii and vi has entirely reconsolidated into the same dark brown finish as the rest of the encrusted face. At the same time, their edges are rounded. If the new strokes had patinated under the same wet conditions as the stone, and for as many centuries, they would be as difficult to see without oblique lighting as are the non-penetrating heads of letters ii and vi. The relevant comparison for brightness is therefore between the new strokes and the heads of ii and vi, not between the new strokes and the rest of the letters.

The siltstone bottoms of the grooves in the letters that penetrate the crust have beads of dark brown oxide on them, which must have reconsolidated since the letters were made. One letter actually has a tiny concretion in it, evidently a speck of silica or other foreign matter that became fused into the groove by the iron oxide dust as it reconsolidated. This concretion (near the base of letter vi) is on top of the strokes that made the letter.

Emmert, as quotes above, reported that he struck the stone, but only on its back side, with a steel probe before he actually dug down to it, and indeed there is a small gash in the siltstone on the back. This gash looks much fresher under the microscope than do either the back of the stone or the bottoms of the penetrating grooves.

As Gordon originally noted (1972), three of the Bat Creek letters have small dots or "pearls" at the end of the line segments, an inessential detail which is common in Greek and Paleo-Hebrew coin inscriptions. Under the microscope, it became apparent that these were not drilled in, as Gordon assumed, but rather were nicked in with small knife strokes. Thus the top of letter v looks like a mace head under magnification. Interestingly, the microscope revealed a heretofore unsuspected pearl shallowly cut onto the end of the left leg of the yod (letter iv). This would originally have been as visible as the new strokes, but now has patinated to where it is quite unapparent to the naked eye.

I would not venture to say just how old the patination on the original characters is, but it certainly gives the impression of great age in comparison with the two modern strokes. This patination is not a mere stain, as might be obtained by soaking the stone for a few months in iron-rich water, but is actually a partial healing of the disturbance to the surface. Van Beek was therefore wrong to have claimed that the patina itself indicates the Bat Creek inscription is modern.

The Word Divider

In Paleo-Hebrew, words are ordinarily separated by small marks rather than by spaces as in English, Cherokee, or standard Square Hebrew. In the Bat Creek inscription, there is a small mark between the letters I designate as v and vi, which Cyrus Gordon has identified as a Paleo-Hebrew word divider. The Bat Creek form is rather distinctive, however, being comma-shaped, instead of a simple dot.

In my earlier article, I erroneously indicated that the distinctive Bat Creek word divider was completely unknown in Paleo-Hebrew before the 20th century discovery of the Qumran manuscripts (p. 115). However, the recent correspondence with Frank Cross prompted me to take a closer look at the Siloam Tunnel inscription, which was found in Jerusalem in 1880, and subsequently removed to Istanbul. In Lidzbarski's 1902 drawing, which I had consulted before writing my article, the word divider in this inscription is shown as a simple dot. Under magnification, however, Birnbaum's photograph (1954: Plate 14) shows that the Siloam inscription in fact consistently uses a comma-like mark that is set at about 60° from the vertical, just as is the Bat Creek word divider. I was therefore wrong to have stated that this was entirely unknown in 1889. Nevertheless, it would still appear that no source for it was known prior to 1880. The famous Mesha Stele, found in 1868, for example, uses the simple dot form to separate words, along with a tall vertical stroke to separate clauses or sentences.

The Bat Creek word divider therefore does still rule out the possibility that an 18th century or early 19th century prankster such as James Adair or John Haywood placed the stone in the mound at any time prior to 1880. Furthermore, the state of vegetation on the mound when Emmert excavated it in 1889 rules out the possibility that anyone who had been to Jerusalem or Istanbul to scrutinize the Siloam inscription placed the stone in the mound between 1880 and 1889. Therefore the stone, if a forgery, must have been introduced by Emmert as he dug, and so was never in the mound for the letters to patinate at all. The fact that they are, in fact, well patinated, therefore provides independent evidence that the stone is not a forgery.

Conclusion

Although Mainfort and Kwas have raised some interesting issues concerning the Bat Creek stone, their three principal conclusions concerning this important artifact are seriously defective.

First, their conclusion that the inscription on the stone is not Paleo-Hebrew as identified by Cyrus Gordon is based entirely on correspondence with Frank M. Cross, who makes no less than three elementary and documentable errors. The jury is therefore still out (if, indeed, it has ever been seriously empaneled) as to whether the inscription is really Paleo-Hebrew. No one has ever made a better or even equally good suggestion as to what it might be instead.

Second, their conclusion that the brass bracelets found with the stone "are in all probability relatively modern European trade items" is contradicted by the radiocarbon date on the wooden disk fragments found, according to the official record, in intimate association with the bracelets and inscribed stone. Indeed, even if the inscribed stone had never existed, the bracelets, together with the radiocarbon date, in themselves provide solid evidence of some kind of a pre-Norse contact between the Old and New Worlds, unless we are to believe (equally remarkably) that the cementation process for making brass was independently discovered in the New World.

And third, when we investigate their claim that in 1898 Cyrus Thomas backhandedly repudiated his own 1894 Mound Explorations, including in particular the Bat Creek stone, it turns out that what he really was attacking was an 1892 book by his arch-rival, Stephen D. Peet, and Peet's inclusion of the Davenport tablets and elephant pipes as potentially authentic mound artifacts.

Many real questions concerning the Bat Creek stone do remain unanswered: If the inscription is indeed Paleo-Hebrew, as identified by Cyrus Gordon, what are the unidentified letters and what do they say? Or if it is not Paleo-Hebrew, as maintained by Frank Cross, then what is it instead? Philology should be able to tell us what it meant, and to whom.

Retesting of the wood fragments with a larger sample of material would in all likelihood pin down the radiocarbon date to a narrower interval. Furthermore, a new technique for age-dating copper alloys may soon make it feasible to date the bracelets directly. However, the burden of proof now rests on Mainfort and Kwas or others to have these tests performed if they wish to discredit the existing radiocarbon date.

Acknowledgments

The author is grateful to Curtis Hoffman and Timothy Gregory for invaluable suggestions.

Notes:

1. It is unclear why Cross, in his letters, shifted the time frame under discussion from the first or second century A.D., which Gordon and I (without intending to be rigid) had identified

as the best fit for the letters, to the period 100 B.C. - 100 A.D. Nevertheless, Cross's claim is false as stated.

2. The finer issues of whether *yhw*d eventually became the Hebrew word for Judea and whether it is the same word as *yhd* that frequently appears on Paleo-Hebrew coins are beyond the scope of the present paper.

3. In his letter, Cross did call attention to a minor error in the references to my Table 3 (p.90). In the table, I incorrectly gave Kadman, pp. 124-32 as a reference for the first form of *goph* I show for letter vi. A correct reference for this form is Birnbaum (1954: Plate 55), which still shows a shekel of the Jewish War (1st. century A.D.). It should also be noted that in my discussion of letter viii on p. 95, the reference to Birnbaum (1971: 42) should instead be to Birnbaum (1954: Chart 42).

4. The pre-Incan Sican people of Peru smelted copper, and also alloyed it with arsenic and tin to make bronze, but not with zinc to make brass (Shimada and Merkel 1991). The latter process is much more difficult, since at atmospheric pressure zinc boils below the melting point of copper.

5. C-shaped bracelets were also popular across Asia, as evidenced by the 1st century A.D. specimens from the Silk Road center of Bactria (Sariandi, 1985: 231, #2.4, Ills. 105, 107; 238, #3.17, #4.21; 252, #5.4; 256, #6.16).

6. The sentence in the Report that follows the passage quoted above, "The engraved characters on it are beyond question letters of the Cherokee alphabet said to have been invented by George Guess (or Sequoyah), a half-breed Cherokee, about 1821," was entirely added by Thomas. Emmert himself professed never to have seen letters like these before, and in his letter of 2/25/89 provided an upside-down sketch of the inscription.

7. Norman Totten has noted that the wooded disks may originally have been covered with a thin sheet of copper, now disintegrated, to make up an earspool, and that the copper salts visible on the wood fragments and that Emmert noted in the water saturating them may have come from this sheath rather than from the bracelets themselves. The plausibility of this position is strengthened by the fact that the zinc in the bracelets would have served as a deoxidant, and that the bracelets are not badly corroded. Nevertheless, the intimate juxtaposition of the bracelets and the wood fragments in the burial is confirmed by Emmert's belief that the salts in the water saturating the wood did come from the bracelets.

8. Regrettably, the mandible of this skull, although shipped and cataloged (Emmert #10, NMNH 134903), did not turn up in a recent thorough search of the Smithsonian's holdings by Felicia Pickering, and therefore has almost surely been lost.

9. The ultimate source of panels 1 and 2 of Squier and Davis's Plate 34, incidentally, is a U.S. Army Corps of Engineers map prepared by Major. Isaac Roberdeau in 1823, which is still on file in the National Archives. See McCulloch (1991).

10. Or perhaps he did not wish to dignify Peet, who had just demolished his own Cherokee theory of the Middle Woodland mounds, with actual identification by name (Thomas 1890, Peet 1891). Earlier, Thomas did not hesitate to attack Rev. Jacob Gass by name, though only as "Mr. Gass" (1894: 632-43)
11. The same may be said for the contention of Lowell Kirk (as quoted by Lee 1991) that it was no Emmert, but a local resident named Luther Blackman who forged the stone.
12. Unfortunately, Peet provides no details on this vaulted burial in his book.

References Cited

Arundale, Wendy Hanford

- 1981 Radiocarbon Dating in Eastern Arctic Archaeology: A Flexible Approach. *American Antiquity* 46: 244-271.

Avigad, Nachman

- 1975 A Bulla of King Jonathan. *Israel Exploration Journal* 25, pp. 245-6 and Plate 26 B.C. Reprinted in *IEJ Reader*, vol. II, pp. 1204-6.

Birbaum, Solomon A.

- 1954 *The Hebrew Scripts, Part Two: Plates*. E.J. Brill, Leiden.
 1971 *The Hebrew Scripts, Part One: Text*. Paleographia, London.

Brain, Jeffrey P.

- 1979 *Tunica Treasure*. Peabody Museum, Cambridge, Mass.

Comstock, Mary and Cornelius Vermeule

- 1971 *Greek, Etruscan and Roman Bronzes in the Museum of Fine Arts, Boston*. New York Geographic Society, Greenwich, Connecticut.

Cross, Frank Moore

- 1968 The Phoenician Inscription from Brazil: A Nineteenth Century Forgery, *Orientalia* 37: 437-60.
 1969 Papyri of the Fourth Century B.C. from D^hliyah. In *New Directions in Biblical Archaeology*, edited by D.N. Freedman and J.C. Greenfield, pp. 41-62. Doubleday & Co., Garden City, N.Y.
 1989a Letter to Robert C. Mainfort, Jr., dated 6/19/1989, cc: author.
 1989b Letter to author, dated 6/21/1989, cc: Robert C. Mainfort, Jr.

Davidson, Gladys R.

- 1952 *Corinth: Results of Excavations, Volume XII: The Minor Objects*. The American School of Classical Studies at Athens, Princeton, New Jersey.

Emmert, John W.

1889a Letter to Cyrus Thomas dated 2/15/1889. Smithsonian National Anthropological Archives, MS 2400, Tennessee.

1889b Letter to Cyrus Thomas dated 2/25/1889. Loc. cit.

1889c Letter to Cyrus Thomas dated 3/7/1889. Loc. cit.

Ford, Barbara

1972 Semites in America. *Science Digest* 71 (January): 43-53.

Fowke, Gerard

1902 Archaeological History of Ohio. Ohio Historical Society, Columbus, Ohio.

Freedman, D.N. and K.A. Mathews

1985 The Paleo-Hebrew Leviticus Scroll (11Q paleoLev). American Schools of Oriental Research, Winona Lake, Indiana.

Gordon, Cyrus H.

1971 *Before Columbus: Links Between the Old World and Ancient America*. Crown Publishers, Inc., New York.

1972 The Bat Creek Inscription. In *The Book of the Descendants of Doctor Benjamin Lee and Dorothy Gordon*, edited by C.H. Gordon, pp. 5-18. Ventnor Publishers, Ventnor, New Jersey.

Holum, Kenneth G. et al.

1988 *King Herod's Dream: Caesarea on the Sea*. Norton, New York.

Lee, Larry

1991 Mystery of the Bat Creek Stone. *Knoxville News-Sentinel*, February 18, p. B1.

McCulloch, J. Huston

1988 The Bat Creek Inscription: Cherokee or Hebrew? *Tennessee Anthropologist* 13 (2): 79-123.

1991 Isaac Roberdeau on the East Fork, Milford, and West Milford Works. Unpublished MS, Ohio State University.

McKusick, Marshall

1979 Canaanites in America: A New Scripture in Stone? *Biblical Archaeologist* 42:137-40.

1991 *The Davenport Conspiracy Revisited*. Iowa State University Press, Ames, Iowa.

McLean, Mark

- 1982 The Use and Development of Palaeo-Hebrew in the Hellenistic and Roman Periods. Unpublished Harvard University dissertation.

Mahan, Joseph B., Jr.

- 1971 The Bat Creek Stone. *Tennessee Archaeologist* 27(2):38-44.

Mainfort, Robert C., Jr., and Mary L. Kwas

- 1991 The Bat Creek Stone: Judeans in Tennessee? *Tennessee Anthropologist* 16(1):1-19.

Naveh, Joseph

- 1973a An Aramaic Tomb Inscription Written in Paleo-Hebrew Script. *Israel Exploration Journal* 23:82-91.

- 1982 *Early History of the Alphabet: An Introduction to West Semitic Epigraphy and Paleography*. Magnes Press, Jerusalem.

Peet, Rev. Stephen D.

- 1891 The Migrations of the Mound-Builders. *The American Antiquarian and Oriental Journal* 13: 131-150.

- 1892 *The Mound Builders: Their Works and Relics*, Vol. 1, 1st. Ed. Office of the American Antiquarian, Chicago. Photocopy reprint edition available from Ancient Earthworks Society, 104 Lathrope St., Madison, Wis. 53705.

- 1903 *The Mound Builders: Their Works and Relics*, 2nd. Ed. Office of the American Antiquarian, Chicago.

Putnam, Frederick W.

- 1884 *17th Annual Report*. Peabody Museum, Cambridge, Massachusetts.

Richter, Gisela M.A.

- 1915 *Greek, Etruscan and Roman Bronzes*. Metropolitan Museum of Art, New York.

Sariandi, Victor

- 1985 *The Golden Hoard of Bacteria*. Aurora Art Publishers, Leningrad.

Shimada, Izumi, and John F. Merkel.

- 1991 Copper-Alloy Metallurgy in Ancient Peru. *Scientific American* 265 (July): 80-86.

Smith, Bruce D.

- 1985 Introduction to reprint edition of Thomas (1894).

Squier, Ephraim G., and Edwin H. Davis

- 1848 *Ancient Monuments of the Mississippi Valley: Comprising the Results of Extensive Original surveys and Explorations.* Smithsonian Contributions to Knowledge, Vol. 1. Smithsonian Institution, Washington.

Stieglitz, Robert R.

- 1976 An Ancient Judean Inscription from Tennessee. *The Epigraphic Society Occasional Publications* 3(2,#65): 1-5.

Thomas, Cyrus

- 1887 Letter to J.W. Powell, dated 1/20/1887. Smithsonian National Anthropological Archives, BAE Letters Received, Thomas, Cyrus.
- 1890 *The Cherokees in Pre-Columbian Times.* N.D.C. Hodges, New York.
- 1894 *Report on the Mound Explorations of the Bureau of Ethnology.* In J.W. Powell, *Twelfth Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution, 1890-'91.* Government Printing Office, Washington, D.C. (Reprinted in facsimile, Smithsonian Institution Press, Washington, D.C., 1985.)
- 1898 *Introduction to the Study of North American Archaeology.* The Robert Clarke Company, Cincinnati.

Whiteford, Andrew H.

- 1952 A Frame of Reference for the Archaeology of Eastern Tennessee. In *Archaeology of Eastern United States*, edited by James B. Griffin, pp. 207-225. University of Chicago Press, Chicago.

Williams, Stephen

- 1988 Fantastic Archaeology: Fakes and Rogue Professors. *Symbols* (December): 17-23. Peabody Museum and Dept. of Anthropology, Harvard University.
- 1991 *Fantastic Archaeology: The Wild Side of North American Prehistory.* University of Pennsylvania Press, Philadelphia.