

THE BAT CREEK STONE REVISITED:
A REPLY TO MAINFORT AND KWAS IN *AMERICAN ANTIQUITY*

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Feb. 23, 2005

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This paper was submitted to American Antiquity, the journal of the Society for American Archaeology, as a comment on an article there by Robert C. Mainfort, Jr. and Mary L. Kwas, but was summarily rejected as being “far outside the expertise and interests of the readership.” It may be cited with the URL:

<<http://www.econ.ohio-state.edu/jhm/arch/batcrk.html>>

Abstract

The remarkable similarity between the Smithsonian's Bat Creek inscription and a Paleo-Hebrew phrase published in an 1870 Masonic reference work reported in *American Antiquity* by Mainfort and Kwas (2004) demonstrates that its Hebrew affinity should have been apparent even in 1889. However, the two texts are different, even to the number of letters in the two words, and Bat Creek correctly uses the mandatory word divider absent in the Masonic text. The former therefore could not have been copied from the latter, and is not necessarily a fraud, as asserted by Mainfort and Kwas.

The "recent work" illustrating "modern productions" and "never-existing" earthworks denounced in 1898 by Cyrus Thomas was not his own *Mound Explorations* with its Bat Creek inscription, as claimed by Mainfort and Kwas, but rather Peet's 1892 *Mound Builders*, for its illustrations of the controversial Davenport artifacts and East Fork earthworks.

The patina on the inscription demonstrates that it must have been buried for a considerable time before its discovery. The existing radiocarbon date of 32–769 cal A.D. on ear spoils from the same burial could be corroborated or contradicted by a test on a bone awl, or even by a retest on the ear spoils using more material.

Resumén en Español

La similitud notable entre la inscripción del 'Bat Creek' de la Institución Smithsonianiana y la frase paleo hebrea publicada en un trabajo de referencia Masónica en 1870, y reportado aquí por Robert C. Mainfort, Jr. y Mary L. Kwas (2004), demuestra que su afinidad hebrea debió haber sido prontamente aparente aunque no fue descubierta hasta 1889.

Sin embargo, los dos textos son diferentes hasta en el número de letras en las dos palabras, y el 'Bat Creek' correctamente usa una forma de arcano de la división obligatoria de las palabras que está ausente en el texto masónico. El primero de todas maneras, no pudo haber sido copiado del último, y no es necesariamente un fraude perpetrado a la inspección monticular Smithsonianiana, como ha sido propuesto por Mainfort and Kwas.

El "trabajo reciente" ilustrando las "producciones modernas" y "no existentes" de terraplanes denunciadas en 1898 por Cyrus Thomas, no fueron sus propias *Exploraciones Monticulares* con su inscripción del Bat Creek, como ha sido reclamado por Mainfort y Kwas, pero más bien las *Construcciones Monticulares* de 1892 de Stephen Peet, el cual sin vergüenza mostró artefactos controversiales de Davenport, Iowa, así como ciertos trabajos disputados sobre los terraplanes East Fork en el pequeño río de Miami en Ohio.

La patina en la inscripción demuestra que ésta debió haber sido enterrada por un tiempo considerable antes de su descubrimiento. La datación existente de radio carbono de 32–769 cal d.C. sobre los carretes de oreja de un mismo funeral puede ser reforzada o contradicha por la prueba en una lezna de hueso que fue también encontrada, o aún por una corroboración de la prueba sobre los carretes de oreja usando más material.

In a recent report in *American Antiquity*, Robert C. Mainfort, Jr., and Mary L. Kwas (2004) claim to have demonstrated that the Smithsonian Institution's Bat Creek stone (National Museum of Natural History [NMNH] catalog number 134902) is a fraud.

According to the 1894 official report of the Smithsonian Bureau of Ethnology's Mound Survey, as directed by Cyrus Thomas, this small inscribed stone was excavated from an undisturbed burial mound on the lower Little Tennessee River at the mouth of Bat Creek (Thomas 1894:393-394). A meticulous 1890 engraving of the inscription (Thomas 1890:36) has been reproduced by Mainfort and Kwas (2004: 762), and an early photograph of the stone appears on p. 394 of the monumental 1894 report. Thomas himself identified the curious symbols on the stone as being "beyond question letters of the Cherokee alphabet" (1894: 393).

Almost 80 years later, however, the late Semitic scholar Cyrus H. Gordon, then of Brandeis University and later of New York University, argued that when inverted from Thomas's orientation, the inscription in fact uses the Paleo-Hebrew alphabet of the first or second century A.D., and that the longest word can be read, from right to left as in Hebrew, *LYHWD*, "for Judea," or *LYHWDM*, "for the Judeans," if the broken final letter is reconstructed as *mem* (1971:appendix, 1972). Hebrew scholar and archaeologist Robert R. Stieglitz of Rutgers University concurs with this reading (1976, 1993).

A radiocarbon test reported by the present author (1988: 108) on wooden earspool fragments (NMNH 134899) found with the same skull as the inscribed stone yielded a point estimate of 427 cal A.D., with a 2σ range of 32 cal A.D. – 769 cal A.D. (Beta-24483/ETH-3677). This 95 percent confidence interval is significantly pre-Norse, not to mention pre-Columbian, yet is consistent with Gordon's first or second century A.D. paleographic dating of the text.

The principal arguments Mainfort and Kwas raise against the authenticity of the stone are: 1. The inscription was copied from an 1870 Masonic treatise, and therefore is clearly fraudulent. 2. In 1898, when Cyrus Thomas denied the reliability of an unnamed recently published volume and the authenticity of certain unspecified artifacts published therein, he must have had in mind his own *Mound Explorations*, and the Bat Creek stone in particular. 3. John Emmert, the Bureau field agent who found the stone, was particularly unreliable. 4. Emmert's likely motive for the fraud was to ingratiate himself with Thomas by "finding" an unmistakably Cherokee artifact in a mound. 5. A pair of brass bracelets found with the inscription are not ancient but rather are characteristic of modern trade goods.

In his 1991 *Fantastic Archaeology*, a book approvingly cited by Mainfort and Kwas in their first paragraph, Stephen Williams of Harvard University wrote, "When the Smithsonian published Cyrus Thomas's great report of the Mound Survey in 1894, it for all intents and purposes shut the door from a scientific standpoint on the Moundbuilder question" (1991: 74). Mainfort and Kwas themselves characterize Thomas's *Mound*

Explorations as “one of the landmark volumes in the history of North American archeology” (2004:761). If even one demonstrable forgery found its way into this “landmark volume,” that in itself should be of major concern to the Smithsonian and archaeologists alike. But if it is so riddled with errors that Thomas himself disavowed it just four years after its publication, as Mainfort and Kwas now claim, then all the conclusions that archaeologists have confidently based on it for over 100 years would now have to be carefully reexamined. At the very least, if Emmert in particular was truly unreliable, the many portions of the *Mound Explorations* that are based on his work and the many artifacts that he found should now be clearly identified as such and “recalled to the factory” by the Smithsonian.

The present comment examines and refutes the above-mentioned principal arguments raised by Mainfort and Kwas. It goes on to suggest various physical tests that could be performed on the stone itself and associated artifacts in order to shed further light on this curious artifact.

1. The 1870 Masonic Treatise

Mainfort and Kwas (2004: 765) have found an illustration in Robert Macoy’s 1870 *General History, Cylopedia, and Dictionary of Freemasonry* (1870: 169) that indeed is remarkably similar to the Bat Creek stone’s inscription, provided that Thomas’s illustration and photograph are inverted to Gordon’s “Hebrew” orientation. Read from right-to-left, both inscriptions have a short first word, followed by a longer second word, in their main line. The second word in both inscriptions begins with the four-letter string LYHW-, and the first letters in the short first word both have the same form. Both inscriptions use the coin script of the first Jewish War (66 - 73 A.D.), including even ornamental “pearls” or dots at the ends of certain line segments.

There, however, the similarity ends. Macoy’s inscription is clearly intended to read *QDSH LYHWH*, or “Holy to-Yahweh [or ‘the Lord’],” a phrase from *Exodus* 39:30, just as he says it should. However, the second word in the Bat Creek inscription, *LYHWD*, or *LYHWDM* if the broken final letter is included, is an entirely different word. In Hebrew, many theophoric personal names are said to be *Yahwist*, in that they contain a fragment *YH-* (Ya-) or *YHW-* (Yahu-) of the name of the Hebrew God Yahweh, or Jehovah as it came to be written in English. The word *YHWD* (the Persian-era version, according to Gordon, of the personal name *YHWDH* or Judah) that appears on both the Bat Creek stone and the “Abba” inscription of 37 B.C. – 70 A.D. from Jerusalem (McCulloch 1993a:53), is in fact one such Yahwist name. There is, however, no trace on the Bat Creek stone of the second *he* that would make the word *YHWH* itself. Finding the common string *YHW-* in an inscription is strongly suggestive that it is Hebrew, but does not in itself show that it was copied from another inscription containing the same string, particularly if the rest of the word is different, even down to the number of additional letters.

In Hebrew, the common prefix *L-* forms the dative, indicating “for” or “to” the word that follows. The fact that both words begin with *L-* is therefore again diagnostic of Hebrew, but is not necessarily indicative that the one inscription is copied from the other.

In both inscriptions, the longer word beginning with the Hebrew-diagnostic string *LYHW-* is preceded with a shorter word. In Macoy’s illustration, this word has three letters, and is clearly intended to read *QDSh*, “holy” or “sacred”. On the Bat Creek stone, the first word has only two letters and there is no trace of the conspicuous W-shaped *shin* that appears in Macoy’s illustration. Whatever the first Bat Creek word is, it is clearly something different.

Macoy in fact makes no claim that his illustration is an ancient Hebrew inscription. Instead, he merely presents it as an illustration of how a biblical expression of particular interest to Masons *would* have appeared in the Old Hebrew characters, as he or a collaborator has reconstructed it from a 19th century letter chart. Although Macoy’s illustration is generally well done, he in fact makes his *Q* wrong. In the Paleo-Hebrew of the Jewish War coins, *qoph* should have a vertical stem with a P-like loop on the right, but it should also have a conspicuous arm to the left with an upward-turning hook at its end (Kadman 1960: 124-32). Perhaps under the influence of standard Square Hebrew, he or his source has omitted the hooked arm. Although the context is compelling that this letter is intended to be a *Q*, Macoy’s version therefore has no standing as to how a Paleo-Hebrew *Q* should look.

On the Bat Creek Stone, the *second* letter of the first word in fact has the required hooked arm on the left, along with a vestigial loop, of sorts, on the right. On the Bat Creek Stone, it is therefore the *second* letter that should be read as *Q*, as originally suggested by Stieglitz (1976). The similarity of form of the first letters on the two inscriptions therefore could be entirely coincidental, and does not necessarily mean that the one is copied from the other.

Given that the second Bat Creek letter is *Q*, the easiest reading for the first letter is an inverted *resh* or *R*, as originally suggested by Henriette Mertz (1964), and detailed by the present author (1988: 88-97). This would make the first Bat Creek word *RQ*, or “Only,” a totally different word than *QDSh*, or “Holy,” as in Macoy’s illustration.

On the Bat Creek stone, there is a ninth letter (counting the broken letter as the eighth) below the main line. It is not clear what was intended by this letter, but it is nowhere to be found in Macoy’s illustration.

Mainfort and Kwas did ask Hebrew expert Frank Moore Cross to examine the Macoy inscription. He confirmed that “it is copied from the coin script of the First Jewish Revolt against Rome and is fairly well done” (2004: 765). However, it is telling that Cross is *not* quoted as believing that the Bat Creek inscription, which both Mainfort and Kwas (1991: 5-7) and the present author had asked him to examine on earlier occasions, is copied from Macoy’s illustration. The inference that Bat Creek could have been

copied from Macoy despite its different text is therefore entirely that of Mainfort and Kwas, and not of Cross.

The most conclusive difference between the Bat Creek inscription and Macoy's is in fact the humble mark that separates the two words on the former. Macoy, who was transcribing standard Square Hebrew into Paleo-Hebrew with the use of a letter chart, naturally assumed that it would be appropriate to separate his two words with a space as in English or Square Hebrew. In Paleo-Hebrew, however, words are required to be separated with a small mark. The fact that the Bat Creek inscription correctly uses such a word divider, while Macoy does not, demonstrates that it could not simply have been a bungled copy of Macoy's illustration.

In fact, the Bat Creek word divider is relatively uncommon in that it takes the form of a short diagonal line rather than a simple dot. A nineteenth century expert on Old Hebrew inscriptions who was not, like Macoy, simply working from a letter chart, would have known about the famous Mesha Stele, published in 1869 and now in the Louvre Museum in Paris. Although this inscription is Moabite of the 10th century B.C., it uses a Canaanite alphabet very similar to Old Hebrew, complete with word dividers. On the Mesha stele, however, the word dividers are simple dots.

The unusual Bat Creek word divider does appear in the Siloam inscription, which was discovered in 1880 in Hezekiah's tunnel in Jerusalem and dates historically to 701 B.C. Although Guthe's transcription (1882) of the inscription into Square Hebrew letters uses bold dots to represent the word dividers, a close examination of his accompanying photograph of an impression of the inscription reveals that they are in fact short diagonal lines set at approximately the same angle as is the Bat Creek word divider.

Although the Siloam inscription dates from First Temple times, long before the apparent date of the Bat Creek inscription, the diagonal word divider did survive well into the Second Temple period, as evidenced in the Qumran *Leviticus* Fragments (Birnbaum 1954:plates 28-30, Naveh 1982:plate 14c). Hansen (1964:41) dates these fragments to circa 125-175 B.C., and Cross (1961:189, note 4) concurs that they are of much later date than Birnbaum would allow. There is of course no possibility that a Bat Creek forger could have known about this manuscript, which was discovered only shortly after World War II.

Although the Bat Creek inscription could not have been copied from Macoy's illustration, the Macoy figure does show that Bat Creek has unmistakable similarities to Paleo-Hebrew, and that this similarity should have been readily apparent to anyone with even a passing interest in ancient scripts, even in 1889. The fact that Cyrus Thomas, the Smithsonian's chief debunker of allegedly Old World inscriptions, did not see this glaring similarity, demonstrates, if nothing else, that he was incompetent for this task.

2. A Landmark Bulldozed

According to Mainfort and Kwas, “Thomas provides the most conclusive, albeit indirect, indictment of the Bat Creek stone’s authenticity” on pp. 24-25 of his 1898 *Introduction to the Study of North American Archaeology*, where he states,

It is unfortunate that many of the important articles found in the best museums of our country are without a history that will justify their acceptance, without doubt, as genuine antiquities. 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*].

From this passage, Mainfort and Kwas conclude, “We believe that the ‘best recent work’ that Thomas alludes to is *his own* final report on the Smithsonian mound explorations (1894), and that the ‘articles whose history . . . is fully known’ is a veiled reference to the alleged discovery of the Bat Creek stone. . . . Thus, there is strong, albeit circumstantial, evidence that the Bat Creek stone was recognized as fraudulent by 1898” (2004: 263; their emphasis).

Note that Thomas did not precisely allude to “the ‘best recent work,’” as quoted by Mainfort and Kwas, but rather 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” would at least potentially embrace the efforts of other authors. In fact, the whole point of the Mound Survey was to produce a large body of evidence, “whose history, as regards the finding, is fully known,” as Thomas put it in 1898. Thus, his reference to “one of the best recent works on ancient America” that “is flawed to some extent by want of this precaution,” in that “Mounds and ancient works are described and figured which do not and never did exist,” and in which “articles are represented which are modern productions,” would necessarily have been to some *other* recent work, and *not* to his own, as claimed by Mainfort and Kwas.

In fact, however, as I have already conclusively demonstrated (McCulloch 1993c:14-16), the “recent work” that Thomas had in mind must have been the first (1892) edition of Stephen D. Peet’s *The Mound Builders*. On pp. 13 and 41 of Peet’s book, we find illustrations of the hotly disputed Davenport tablets and elephant pipes. As Mainfort and Kwas themselves point out, “It was Thomas (1885, 1886a, 1886b, 1894:633-643) who authored several sharp criticisms of the fraudulent inscribed tablets and elephant pipes from Davenport, Iowa” (2004: 763). Peet not only failed to deny the authenticity of these artifacts, but actually had the audacity to cite new evidence in favor of the pipes.

And as for the “Mounds and ancient works [that] are described and figured which do not and never did exist,” Thomas clearly had in mind the reproduction on p. 78 of Peet’s book of Figure 34, No. 2B from Squier and Davis’s 1848 *Ancient Monuments*, illustrating intricate earthworks on or near the East Fork of the Little Miami River in southwestern Ohio. Just four years earlier, Thomas had declared, using very similar words, “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” (1894: 566).

Thus it was clearly Peet’s book, *and not his own*, that Cyrus Thomas was denouncing in 1898. The “modern productions” (once again misquoted by Mainfort and Kwas as “reproductions”) alluded to by Thomas were clearly the Davenport artifacts, and not his own project’s Bat Creek stone, however enigmatical he may have found the latter (Thomas 1894:714). If he did not mention Peet by name, it was perhaps because he did not wish to dignify Peet, who had only recently demolished Thomas’s own Cherokee theory of the Middle Woodland mounds, with actual identification by name (Thomas 1890, Peet 1891).

As it happens, the present author has discovered the ultimate source of Squier and Davis’s Figure 34, Nos. 1 and 2, in an 1823 U.S. Army Corps of Engineers drawing that is preserved in Record Group 77, Drawer 144, Sheet 20 of the U.S. National Archives in Alexandria, Va. See McCulloch (2001) for online color photographs, Wallace (1999:140-141) concerning Thomas Jefferson’s interest in these works, and McCulloch (1996) for further details. Although Squier and Davis had only third-hand information about these earthworks, by way of Warden (1834), their depiction of the East Fork earthworks themselves, if not the terrain surrounding them, and therefore Peet’s reproduction of this figure, is in fact remarkably faithful to the 1823 original. Thomas was therefore not justified in claiming that the East Fork earthworks as depicted by Peet never existed.

Mainfort and Kwas go on to argue that Thomas deliberately did not explicitly identify the Bat Creek stone as a forgery, in order to protect his own reputation as a debunker of the Davenport artifacts:

This inference [that Thomas in 1898 was denouncing his own *Mound Explorations*] begs the question of why Thomas did not admit to the failings of his magnum opus in a more direct manner. We believe that he answer is straightforward. Thomas, and indeed the Smithsonian Institution itself, had placed themselves in a position such that they really could not afford to pronounce the Bat Creek stone a forgery after publishing it. It was Thomas . . . who authored several sharp criticisms of the fraudulent inscribed tablets and elephant pipes from Davenport, Iowa. [Mainfort and Kwas 2004: 763].

This accusation, by Mainfort and Kwas, of a knowing cover-up of forgery on the part of Thomas would be sufficient to destroy the integrity of the remaining portion of the *Mound Survey* that was *not* based on discoveries by Emmert. However, their argument simply makes no sense: If Thomas really thought he had been tricked into publishing an “undoubtedly” fraudulent Hebrew inscription, and wanted to cover this up, why would he then have gone out of his way in 1898 to raise vague questions about the reliability of his own study?

3. Emmert’s Credibility

Having thus “demonstrated” that the Bat Creek inscription is a forgery, Mainfort and Kwas go on to identify John Emmert, the Smithsonian field assistant who discovered the stone, as the most likely suspect for having forged it, or at least for fraudulently having planted it.

Among the “evidence” of Emmert’s unreliability that they cite is, “an extraordinary suite of artifacts spanning the entire known cultural sequence from Paleoindian times (circa 10,000 B.C.) to the historic period,” that Emmert found at a cave site in Sullivan County, Tenn. in 1883 when he was working for the Harvard Peabody Museum.

Yet according to their source for this find, curator Stephen Williams of the same museum, the reported context of this anachronistic group of artifacts was that “an historic Indian pot circa 1870-90 was found *filled* with artifacts: sheet mica, potsherds and the tip of a greenstone celt. The vessel had grooved stone axes ‘packed around’ it (Peabody Museum catalogue notes)” (1993:16; Williams’ emphasis).

Now it would not have been Emmert’s responsibility as a mere field agent to date and evaluate artifacts or their contexts, but merely to discover them and to report as accurately as possible the context in which they were found. From the eclectic nature of this find, and in particular from the nineteenth century pot containing it, it would appear that this was a nineteenth century artifact hunter’s cache of finds that had been stored in the cave while their finder went in search of other items. If from Emmert’s accurate description of how these artifacts were found, Williams is now able to date their deposition, if not the artifacts themselves, to circa 1870 or later, and thereby to dismiss the archaeological significance of the find, this should not cast doubt in any way on Emmert’s integrity or accuracy as an objective reporter.

Mainfort and Kwas (2004:764) question the integrity of Emmert’s discovery of the Bat Creek stone itself on the grounds that “there are no field photographs or detailed field records” of the find. In fact, as I have already pointed out (1993a:10), the Smithsonian discarded *all* the field notes of the Mound Survey, probably in a circa 1920 house cleaning, perhaps in a belief that the detailed published account made them redundant. All that survives in MS 2400 of the National Anthropological Archives are letters that were sent through the mail. Emmert did write Thomas on 2/25/1889, within the same

month as the discovery, “I am taking full notes as I go along” (Emmert 1889a), so we at least know that his field records at one time existed, and that he did not simply write his report (1889b), which was incorporated almost verbatim into the *Mound Explorations* (Thomas 1894:393-394), from memory a month after the excavation. Given Thomas’s interest in, and puzzlement over, the stone (1890:35-37; 1894:714; and below), we would expect that he examined these notes most carefully for any discrepancies. It is most unfortunate that the original field notebooks were discarded, but this is a problem that afflicts the entire Mound Survey, and one that should place no special onus on either the Bat Creek stone or John Emmert.

Mainfort and Kwas (2003: 764) go on to point out that in 1890, Thomas reported that in light of the unusual nature of the inscription (which might have been regarded as *too* neatly confirming his Cherokee theory of the Midwestern mounds), he actually took the precaution of sending another agent “to the field where Mr. Emmert was at work, to learn the whole history of the find” (1890: 36). This quote would lead the reader to believe that Thomas must have been left with lingering doubts about the authenticity of the inscription. However, what Mainfort and Kwas failed to tell the readers of *American Antiquity* is that Thomas continued, “**The examination by the person sent** [who turns out to have been James Middleton] **confirmed the statement by Mr. Emmert in every particular**” (emphasis added).

Thomas expressed full confidence in Emmert’s “good and faithful work” in an 1892 letter of reference for him, and personally certified the authenticity of the inscription by including it in his 1894 report. Furthermore, James Fowke, himself a Bureau insider who would have been privy to any suspicions of forgery, found no reason to question its authenticity in his 1902 book, despite the fact that it appeared to conflict his own view that the mounds were much older than Thomas would allow (Fowke 1902:458), and despite the fact that Fowke devoted several other pages of the same book to various inscriptions that he regarded as fraudulent.

Mainfort and Kwas cite nine works on Cherokee archaeology and ethnology that make no reference to the stone at all, as purported evidence that the Bat Creek stone was not regarded as authentic by contemporary scholars. However, even though the stone admittedly contains a few letters that look like Cherokee, it also contains a few letters that could pass for English. In fact, it works no better as Cherokee than as English, and no one has ever proposed a Cherokee translation (McCulloch 1988:83-88). The fact that it is not Cherokee, despite Thomas’s unfortunate claim, would fully account for the subsequent lack of interest in it by Cherokee ethnologists. But not being Cherokee is hardly equivalent to not being authentic.

It should be noted that archaeologist Marshall McKusick of the University of Iowa (1994) does maintain that the Bat Creek inscription, when held in Thomas’s orientation, actually represents an early 1820s form of Cherokee writing. This is diametrically

contrary to the new claim by Mainfort and Kwas (2004) that it was copied from Macoy's illustration of a Hebrew phrase. See, however, my reply (1994) to McKusick.

4. Emmert's Purported Motive

According to Mainfort and Kwas, "The limited available evidence suggests that ... John Emmert was at least a partner in the fraud, even if he did not inscribe the characters on the Bat Creek stone himself. We believe that Emmert's motive for producing (or causing to have made) the Bat Creek inscription 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 his employer than to 'find' an object that would prove Thomas's hypothesis that the Cherokee built most of the mounds in eastern Tennessee?" (2004: 766).

The first obvious problem with this theory is that the inscription is not intelligible as Cherokee. Emmert could easily have done a much better job of Cherokee, since in a letter to Thomas dated Dec. 19, 1888, he indicated that he had just spent the summer with the Cherokee in North Carolina. Even if he did not himself understand or read Cherokee, he could easily have asked someone to write out a plausible epitaph or personal name in Cherokee script and simply copied it onto the stone.

The second obvious problem with this theory is that the inscription *is* intelligible (with admitted difficulties) as Paleo-Hebrew. Mainfort and Kwas (2004) themselves actually maintain that it was copied directly from Macoy's illustration of a Hebrew phrase. Emmert could hardly have "ingratiated" himself with Thomas, whose thinly disguised mission was to debunk the once-popular hypothesis that the Moundbuilders were descended from Old World peoples, and in particular from ancient Israelites, by turning in an apparently Hebrew inscription from an Indian mound. If Thomas had been competent to spot the clear and diagnostically Hebrew string *LYHW*- on the stone, it is no exaggeration to say that Emmert would have been out the door in an instant, with the stone flying through the air right behind him. It is only thanks to the fact that Thomas, an entomologist by training, equally incompetently misidentified it as Cherokee, that it was published at all.

5. The Brass Bracelets

A pair of heavily leaded yellow brass bracelets (NMNH 134898) were also found with the same skull as the inscribed stone and radiocarbon-dated earspools. A color photo of these appears in McCulloch (1993a: 51). As detailed in McCulloch (1988: 104-7), their composition could either be ancient or modern.

The bracelets are, however, somewhat unusual in that they were wrought, i.e. hammered into shape. In McCulloch (1993a: 51), I had stated, "The way in which the

bracelets were crafted favors an ancient origin because they do not resemble most modern trade goods, which were usually drawn or cast.”

Mainfort and Kwas, remarking on my admitted lack of “experience in contact period archaeology,” quote this statement and then challenge it, as follows:

He [McCulloch] offers no support for this statement.

By ‘modern trade goods,’ McCulloch presumably refers to our (Mainfort and Kwas 1991:7) observation that brass wire bracelets are fairly common at eighteenth-century Euroamerican and Native American sites in eastern North America. Most reported specimens were not ‘drawn or cast,’ but rather cut from lengths of brass wire of varying thickness (Brain 1979:193) [Mainfort and Kwas 204:767].

In fact it is Mainfort and Kwas who are in error here, on the facts of elementary metallurgy. Ever since the Middle Ages, wire has ordinarily been manufactured by being pulled, or *drawn*, through tapered holes drilled into dies (Wire 2004). To state that an eighteenth century bracelet has been *cut from wire* therefore implies that it has been *drawn*, and not wrought or cast (poured into a mold). A bracelet’s being drawn and being cut from wire are therefore not mutually exclusive alternatives, as Mainfort and Kwas evidently believe, but rather the same thing. Since Mainfort and Kwas themselves assert that “most reported specimens” of modern trade bracelets were “cut from lengths of brass wire of varying thickness,” I therefore in fact have it on *their* authority that the differently-made Bat Creek bracelets are indeed atypical of modern trade goods.

In 1993, I had challenged Mainfort and Kwas to come up with even one example of a modern trade bracelet that was in fact wrought rather than inexpensively cut from wire or cast (1993c:6-7). They have risen to this challenge by providing not just one but several such examples (1993:89-90, 2004:797). The one depicted by Birk and Johnson (1992:222), for example, is in fact very similar in construction to the Bat Creek bracelets, in that it was made by hammering a narrow strip of copper or brass into a rounded profile. However, while these examples demonstrate that such bracelets could be modern, they in no way rule out the possibility of an ancient origin for the Bat Creek bracelets.

In the same article I had stated, “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 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” (McCulloch 1993c:21). Mainfort and Kwas (2004:766-767) argue that my reasoning here is “flawed” because, in their view, “the inscription clearly was forged and the Bat Creek stone planted,” so that “there is ample reason to be suspicious of all artifacts allegedly found in Bat Creek Mound 3.” In fact, it is *their* reasoning that is flawed here, since if, as I had hypothesized, the inscribed

stone had never existed, its alleged inauthenticity would have provided no grounds for questioning the well-documented association of the bracelets with the wood.

Manillas, the heavy copper, brass or bronze bracelets made for the eighteenth century West African slave trade, were typically cast (Semans 2004). It therefore would not be surprising to find cast trade bracelets in North America as well.

Further Research and Concluding Remarks

Although the Hebrew inscription published by Macoy (1870) shares a diagnostically Hebrew four-letter string with the Bat Creek inscription, the word this string begins is different in the two inscriptions. There are several other differences between the two inscriptions, including, most tellingly, an arcane but valid form of word divider on the Bat Creek inscription. Although the admitted similarity between the two inscriptions demonstrates that the potentially Hebrew nature of the Bat Creek inscription should have been recognized immediately after it was found, it is insufficient to demonstrate that it was copied from Macoy's illustration, as claimed by Mainfort and Kwas.

Several physical tests remain to be performed that could perhaps shed new light on this unusual artifact.

First, the patina on the original letters should be examined by a mineralogist to determine how long, if at all, the inscription rested in the reportedly damp environment of the mound before it was excavated. The inscription is scratched into a thin dark brown iron oxide crust that had formed naturally on one side of a light tan siltstone. Two vertical lines are present in modern photographs of the stone (see the 1970 Smithsonian photograph reproduced in Mainfort and Kwas 1991: 4 and the 1989 photo by Warren Dexter at McCulloch 2001), yet are completely missing in nineteenth century drawings and photographs of the inscription (cp. the 1890 etching reproduced by Mainfort and Kwas [2004:762] and the photograph in Thomas [1894:394]). These lines were clearly added sometime between 1894 and 1970, while the stone was in dry storage in the NMNH.

On examination with the naked eye and with a 2X magnifying glass, there are no obvious differences between the two new vertical strokes and the original inscription. Under a low-power (20X) microscope, however, the two groups of marks look very different: The two vertical strokes have sharp edges and are bright only because they are the red-orange color of the streak of freshly pulverized iron oxide. The edges of the original characters, on the other hand, are rounded, and the red-orange coloration that must originally have been present in the grooves has entirely reconsolidated into the same dark brown finish as the rest of the encrusted face. The original letters are bright only to the extent that they penetrate through the crust to the light siltstone beneath. The siltstone bottoms of the grooves that do 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 the second letter from the right) is on top of the strokes that made the letter (McCulloch 1993c:18-20).

Emmert (1889b) 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 on the back. This gash looks much fresher under the microscope than do either the siltstone back of the stone or the bottoms of the penetrating grooves.

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. A qualified mineralogist might be able to give an estimate of its age.

Secondly, a bone awl (NMNH 134904) was found with the inscribed stone, earspool fragments, and brass bracelets. With modern methods, a radiocarbon date could probably be obtained on its collagen for comparison to the age of the earspools and the Paleo-Hebrew script on the stone. Mainfort and Kwas themselves declare that no professional archaeologist should be satisfied with a single radiocarbon determination in a potentially controversial situation (1993a:90). As an amateur, I gladly defer to such professionals to obtain a corroboration of the existing date.

And thirdly, the earspool fragments themselves should be redated. The 1988 radiocarbon date had a standard error of 170 years, which is much larger than the 60-70 years that is ordinarily obtained. This was apparently due to the fact that only a relatively small sample (reportedly 30 mg. of copper-laden pithy wood) was submitted for testing, and then that very little of its carbon remained after intensive pre-treatments. Because of the small amount of test material, no correction for ^{13}C could be made. Retesting with a larger sample and even newer methods would probably yield a much smaller standard error. The earspool fragments together weigh 5.5 gm. A color photograph of one of the reassembled spools appears in McCulloch (1993a:52).

The mandible of the human skull associated with the artifacts was collected and catalogued (NMNH 134903), but unfortunately has been lost. Perhaps it was lent to another institution for routine study and never returned. If it turns up in some collection, it should be returned to the NMNH for dental, ^{14}C , and DNA analysis. It would be clearly labeled *134903*, and perhaps also *Emmert*, and/or *Loudon Co. Tenn.*

The reader is referred especially to Gordon (1971, 1972), Stieglitz (1976, 1993), McCulloch (1988, 1993a, 1993b, 1993c, 1994), Mainfort and Kwas (1991, 1993a, 1993b), McCarter (1993a, 1993b), McKusick (1994), and to numerous other letters in the

Nov/Dec. 1993 and Jan./Feb. 1994 issues of *Biblical Archaeology Review* for extensive further details and lively discussion.

Since 2002, the Bat Creek stone has been on indefinite loan from the Smithsonian to the McClung Museum of the University of Tennessee, Knoxville, where it is on public display, some 50 km northeast of the findspot.

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