James R. Harris, Sr., *Southwestern American Indian Rock Art and the Book of Mormon*

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James R. Harris, Sr., *Southwestern American Indian Rock Art and the Book of Mormon*. Orem, UT: By the author, 1991. x + 180 pp., with subject index. $9.95.

Reviewed by Stephen E. Thompson

In this book, the author attempts to show that in a few Fremont and Anasazi petroglyphs one can discern Egyptian-type signs and even interpret some of the petroglyphs as containing Egyptianizing or "Mormonesque" messages. Since the Book of Mormon states that Egyptian was known and used in one form or another by Lehi and his descendants, the author interprets the supposed presence of these signs as "external evidence" of the historicity of the Book of Mormon.

In his foreword, Harris states his purpose in writing this book:

> It has never been and never will be desirable for Latter-day Saints or investigators of the Restoration to build their faith in the Church upon external evidences. But some investigators and members of the Church become so inhibited by anti-Mormon use of external evidence material that they are unable to give scriptural messages a fair or open-minded consideration. In many instances a little help from a knowledgeable believer could open their minds to the message of the restored scriptures. It is this author's hope that the preceding pages will accomplish just such a realization for some otherwise troubled saint or investigator. (p. viii)

Harris's intentions are noble; however, there are major faults to be found with this work. These can be categorized as technical flaws, errors of fact, and methodological failings. I will deal with each in turn.

**Technical Flaws**

There are numerous technical flaws in this book which lead one to believe that very little care was expended on its production. Perhaps most glaring and irritating (because it would have been so simple to correct) is the fact that pages are frequently left unnumbered. When page numbers are included,
their location varies from the upper right-hand corner on a left page (p. 47) and a right page (p. 48), to centered at the top of the page (p. 52). I can discern no reason for the variation. In addition, beyond page 30, most of the book is paginated with the even numbers on the right side.

In a work such as this, for the reader to be able to follow and appreciate the arguments of the author, one of the most necessary features is the presentation of clear, intelligible photographs and figures of the primary source material. Such are lacking in this work. The reader usually has to content him/herself with the author’s hand copies and trust that his renditions are accurate. Further, when photographs are included, their quality leaves much to be desired. There is also a poor relationship between the text and the figures. There are figures to which the text never refers the reader (maps 1 and 2, figs. 10-14). Since these figures are usually without labels, one is never sure what they are supposed to illustrate. In one instance (p. 11), the reader is referred to a map of western Utah and eastern Nevada on pages 18-19. When one turns to these pages, however, one discovers a map of Utah on page 18, and photographs of photographs of petroglyphs on page 19. Given the visual nature of the subject, the lack of clearly labeled and legible photographs must be considered a major fault in the work.

The author’s style of writing also leaves much to be desired. Harris frequently writes in incomplete sentences. For example, on page 129 we read: “Also as the major source of the sweet fruit that is the ultimate objective of mankind’s sojourn in mortality.” There are also numerous examples of the infelicitous use of English. On page 72 we read: “how like Xerpera the wing [sic] beetle”; and on page 75 we find “all of the above suggesting temple of the sky gods.” The author seems to have a problem with the hyphenate function of his word processor, for there are numerous examples of hyphenated words for which there is no justification; e.g., “hiero-glyphic” (p. 9), “express-ed” (p. 146), and “para-phrase” (p. 158), all occur within a given line on the page. A further glaring technical

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1 A comparison between the photograph and figure on p. 66 (fig. 22) would seem to indicate that Harris’s hand copies are not entirely accurate.

2 See also p. 104, first full paragraph, and p. 148, second paragraph, for further examples of such incomplete sentences.
error is the repetition of almost an entire page (cf. pp. 65 and 72).

The author has developed a quite idiosyncratic and confusing bibliographic style. Occasionally he is not immediately forthcoming with the necessary bibliographic information. On page 1 we are told that “a reviewer for the journal titled American Anthropologist described the subject matter of Grant’s book as ‘complex and immense.’” The reference accompanying this statement refers the reader to page 180 of Grant’s book! Are we to conclude that Grant somehow included a quote from a reviewer within the text of his book?3

On page 13 Harris tells us that “a paper published by the San Diego Museum of Man, no. 14, titled, ‘Ceremonial Fertility Sites in Southern California,’ ably supports the above interpretation.” At this point he fails to provide further information on this paper. It is not until the following page that we learn that the author of this paper is McGowan. We are never told how this paper supports Harris’s interpretations, only that it does.

The style the author uses in citing bibliographic entries, and in the entries themselves, is inconsistent and confusing. The author has the habit of leaving off the “19” prefix when referring to a work by using the author’s last name and the year of publication (i.e., on page 1 we find “Grant, 67:180” for “Grant, 1967:180”). Frequently the journal or magazine in which an article appears is given in lieu of the author’s name, as on page 21, where “Era 1927: 1088-1089” serves as a bibliographic reference. The entries in the bibliography itself are frequently incomplete and in error. Articles are cited in journals without accompanying page numbers,4 titles of articles are also omitted,5 and the information provided is at times incorrect.6

Pyramid Texts Spell 1677 receives a separate entry in the

3 The review is by F. D. McCarthy, and appeared in American Anthropologist 71 (1969): 969-70. Also note that the year of publication of Campbell Grant, Rock Art of the American Indian (New York: Crowell) is variously given as 1976 (p. 1) and 1967 (p. 166). The correct date is 1967.
4 See the entries under Beckwith (p. 164); Howard (p. 167); Warner (p. 171).
5 See the entry under Pfeifer (p. 169).
6 The author of Major Trends in Jewish Mysticism is Gershom Scholem, not Gershona Scholem; see p. 170.
7 Harris probably means Pyramid Text Spell 267, which is referred to on p. 29.
bibliography even though it is not a separately published work. Harris fails to tell the reader whether his reference is to the publication of the hieroglyphic text of the *Pyramid Texts* by Sethe,⁸ or whether he is referring to Faulkner’s English translation.⁹ Neither item is in the bibliography, leading one to wonder what Harris used as a source for this spell. The author also fails to supply the reader with a list of abbreviations, making it impossible to determine the source for some of the items in the bibliography.¹⁰

One further comment concerning the bibliography is in order here. Harris has the habit of citing Egyptological works which are several generations out of date. He quotes translations from Adolf Erman, *Life in Ancient Egypt*, which first appeared in German in 1885 and was translated into English in 1894. There are much more recent translations available of the texts quoted by Harris.¹¹ An even greater problem arises from Harris’s frequent use of works by E. A. W. Budge as authoritative.¹² These works are considerably out of date,¹³ and they should not be relied on today. The only reason they should be cited at all is if one wishes to review the history of scholarship on a particular topic. Concerning these works, Leonard Lesko has written:

The numerous works by E. A. Wallis Budge dealing with many aspects of Egyptian religion cover so much material and have so much intuitive

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¹⁰ I was unable to determine what the abbreviation E.S.O.P. stands for. It appears twice in the bibliography, under Morehouse (p. 168), and under Underwood (p. 170).
¹¹ For the hymn to Seti II (Papyrus Anasasi 4, 5, 6-12), quoted by Harris on p. 50, see Ricardo Caminos, *Late Egyptian Miscellanies* (London: Oxford University Press, 1954), 153. For the myth of Isis and Re, quoted by Harris on p. 51, see Joris Borghouts, *Ancient Egyptian Magical Texts* (Leiden: Brill, 1978), 51-55.
¹² He quotes Budge’s sign list found in his *Hieroglyphic Dictionary* on p. 68, and from Budge’s two-volume work on Osiris, *Osiris and the Egyptian Resurrection*, on pp. 64, 72, 73.
¹³ Contrary to Harris’s bibliography, these works did not appear in 1973. The *Hieroglyphic Dictionary* appeared in 1920, and the works on Osiris in 1911.
speculation that some of what he presented is surely correct, but his work must be dealt with by specialists with the same critical scrutiny that would be given to classical sources.\textsuperscript{14}

I would go one step further. Anything on which Budge happened to be correct has probably been restated and improved on in more recent scholarship. Anything which he wrote that has not been reiterated in more recent scholarship is probably wrong and should not be relied upon. Budge’s dictionary has certainly been superseded by such works in English as Faulkner’s \textit{Concise Dictionary of Middle Egyptian}\textsuperscript{15} and Lesko’s \textit{A Dictionary of Late Egyptian}.\textsuperscript{16}

\textbf{Errors of Fact}

Perhaps more damaging to the author’s arguments are the numerous errors of fact found in the text. From his treatment of Egyptian hieroglyphs, it is apparent that Harris does not read the languages of ancient Egypt written in this script. On page 29 he tells us that \textsuperscript{17} means “honors or blessings,” and cites Gardiner’s sign list as his authority. There is nothing in Gardiner’s discussion of this sign which supports Harris’s interpretation. Evidence is also lacking for his statement that this sign is the “ideographic sign for protection or to encircle or surround” (p. 42). Harris has confused \textsuperscript{17} with \textsuperscript{17}, the sign, and with \textsuperscript{17}, the sign. On page 30 Harris tells us that \textsuperscript{17} can be a “negative participle” meaning “not.” Harris has misunderstood the abbreviation on page 52 of Faulkner’s \textit{Concise Dictionary of Middle Egyptian}, where we find \textsuperscript{17} described as “encl. neg. part.;” that is, “enclitic negative particle.” On pp. 84 and 91 Harris tells us that the negative arms sign (\textsuperscript{17}, D-35) “means nti = not or without.” Actually, \textsuperscript{17}

\begin{itemize}
  \item \textsuperscript{15} Raymond O. Faulkner, \textit{Concise Dictionary of Middle Egyptian} (Oxford: The Griffith Institute, 1962), with several reprints.
  \item \textsuperscript{16} Leonard Lesko, \textit{A Dictionary of Late Egyptian}, 5 vols. (Providence, RI: Scribe, 1982-90).
  \item \textsuperscript{17} The letter and number references which accompany the hieroglyphic signs are given to allow the reader to locate the sign in the sign list found in A. H. Gardiner, \textit{Egyptian Grammar}, 3d ed. (Oxford: Griffith Institute, 1957), 438-548.
\end{itemize}
can represent both phonetic $n$ and the negation $n$, but not $nt$. Harris is confused by the negative masculine relative adjective, which although written $\text{nt}$ is read as $\text{nty}$, meaning “who or which is not.” $\text{nty}$, which is what I presume Harris means by $\text{nt}$, is the positive masculine singular relative adjective meaning “who” or “which.”

A further example of Harris’s failure to understand the hieroglyphic writing system is found on page 75, where he describes an inscription found on Deseret Mountain. According to Harris, there are three signs in this inscription which he interprets as representing the Egyptian signs $\text{nt}$.

Speaking of this inscription, Harris states that “the determinative preceded the phonetic signs and supplies the ‘k’ in the word $\text{hyst}$, meaning ‘foreign land’” (p. 75). There are several things wrong with this statement. Firstly, determinatives never occur at the beginning of an Egyptian word. By definition, determinatives occur at the end of words “to assist in establishing their meaning.” Further, the foreign land sign ($\text{nt}$) does not appear to have functioned as a phonogram, i.e., as a sign which represented a consonant or consonants, before the Ptolemaic and Roman periods (which began in 332 B.C.). Prior to that time, it could function as an ideogram, in which case it represented the word $\text{hyst}$, or as a determinative to numerous words, but never as a sign simply representing the letter $\text{nt}$.

As the observant reader will have noticed, the correct transliteration of the word under discussion is $\text{hyst}$, not $\text{hyst}$, as given by Harris. This points up another flaw in Harris’s treatment of Egyptian, his system of transliteration. Here we are told that the foreign land sign supplies the “k” in

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18 We have to take the author’s word on the existence of this inscription and its appearance, since neither hand copy nor photograph is provided.

19 The arrangement of the signs is mine, following common Egyptian practice. I have no idea what the arrangement of the signs in the original might be.


21 During this period, when the principles of hieroglyphic writing had changed, and the number of signs used increased dramatically, the foreign land sign could represent the letters $\text{h, t, d}$, the biliteral $\text{mn}$, and the word $\text{hyst}$. 
“h’st.” Apparently Harris considers \( h, h, \) and \( k \) to be interchangeable letters. They are not. This problem is also evident on page 29, where Harris transliterates the Egyptian word for life, ‘\( nh \), as ‘\( nk \). Note also that Harris is using the same symbol (‘) to represent two Egyptian letters, aleph \( \aleph \) and ayin \( \aleph \) .

A final example of the author’s failure to understand the hieroglyphic writing system is found on page 53, where we are told that the meaning of the harpoon sign ( \( \checkmark \), T-19) is “‘to be irksome’ and is also associated with burial.” It is not correct to speak of a hieroglyphic sign as possessing “meaning.” The closest hieroglyphs ever come to having a “meaning” is when they are used as logograms, i.e., the sign represents a word by actually depicting the object that it denotes. Even in this case, however, it is the word that has meaning, not the sign. The harpoon sign is a biliteral, and stands for the two letters \( qs \). Because of this, it is used in writing the Egyptian words for “to be irksome” (\( qsn \)) and “to bury” (\( qrs \)). The sign by itself has no association with these words.

Other errors of fact worthy of note include the statement on page 135 that Alan Gardiner at one time served as “curator of the Egyptian collection of the British Museum.” He did not.\(^{22}\) On page 85 we are told that \( mhnty \) was the name of the ferryman of the solar bark, in which the sun traveled across the sky (Harris renders it Mahanti). Actually, \( mhnty \) is the generic term for ferryman and not a proper noun at all.\(^{23}\) On page 15 (and p. 63) we are told that signs which Harris interprets as “sky-poles” represent Shu, the Egyptian god who personified sunlight and air. Although Shu is shown numerous times separating Nut, the goddess personifying the sky, from Geb, the personification of the Earth, to my knowledge he is never represented in Egyptian iconography as simply a sky-pole \( \checkmark \) (O-30, \( s\text{hnt} \)).\(^{24}\) Further, speaking of the same figure, Harris interprets a group of signs as “Shu emerging from his egg.” I can find no indication that, in Egyptian mythology, Shu was ever said to come from an egg, but there is an explicit statement to the contrary. In \textit{Coffin Text} 

\(^{22}\) See the necrology by Raymond O. Faulkner in \textit{JEA} 50 (1964): 170-72.


\(^{24}\) During the Egyptian Late Period these sky-poles were personified and given names; but none was named Shu. See Dieter Kurth, \textit{Den Himmel Stützen} (Bruxelles: Fondation égyptologique Reine élisabeth, 1975), 91.
Spell 76 we read "I am Shu. . . . I was not fashioned in the womb; I was not knitted in the egg." Harris’s understanding of the Egyptian use of composite hieroglyphs is in error. The Egyptians could not use a composite hieroglyph, as identified by Harris, to write "beautiful moon" ($\text{i} \text{h nfr}$). In order to explain the petroglyph as an Egyptian sign, Harris has had to greatly distort the Egyptian hieroglyph (),$ as Harris renders as $\text{g}$; pp. 10, 58). Harris fails to realize that the apparent circle at the bottom of the $nfr$ sign is not vacant, but contained striations which indicated that it represented a heart. There is no empty space in which to insert a sign for the moon (p. 58). The above list of errors should not be taken as exhaustive, but merely as an illustrative sampling of the numerous errors of fact that are to be found in this work.

**Methodological Failings**

In addition to the technical flaws and errors of fact pointed out above, this work suffers from major methodological shortcomings that render its argument invalid. Firstly, Harris fails to enter into any sort of meaningful discourse with the mainstream scholarship on Indian petroglyphs. We are given no indication as to whether the figures interpreted by Harris have been the subject of alternative interpretations by other scholars. Harris has by no means made a systematic study of Fremont and Anasazi rock drawings in an attempt to determine whether any Egyptian influence is consistently visible. Rather, he has chosen a few pieces which he feels lend themselves to his "Egyptianizing" interpretations. The reader is given no indication whether or not the individual signs discussed occur in other

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petroglyphs that do not lend themselves to this manner of explanation. Further, Harris presents no objective means of evaluating his interpretations. On page 13, Harris identifies a club-shaped figure with what are apparently arms and legs as "the personification of the male sex organ" (p. 13). Harris cites no parallel instances of such a personification occurring in other petroglyphs, nor any illustrations of the male sex organ with which the figure in question can be compared. It is identified as such because, to Harris, it looks like it. On page 52 Harris identifies figure 17 as "the figure of a ram." He fails, however, to state on what basis he makes this identification, or why the figure must be a ram rather than some other horned animal (goat?). He also fails to explain why he associates this particular animal with the Egyptian god Amun. There were three species of ram which played a role in Egyptian religious iconography; two could be identified with Amun, and one with the god Khnum.27 One suspects that Harris associates the figure with Amun in order to allow him to interpret the scene as a depiction of an event from the mythical stories dealing with the sun-god Re. It should be noted that in these myths, the syncretistic god Amun- Re does not appear, but only Re. There is no precedent in Egyptian mythology for Harris's interpretation of this petroglyphic composition.

Harris makes no attempt to establish that groups of signs he interprets as constituting a text were actually carved contemporaneously. It was not uncommon for petroglyphs to accumulate on a particular stone over a considerable period of time. Although the dating of petroglyphs is notoriously difficult, some attempt should have been made to conclusively link figures which Harris assumes form a text. In discussing his figure 8, Harris interprets a crack in the rock between two figures as representing the horizon, and later associates it with the Egyptian sign for land (ti) or island (iw) (p. 143).28 Is there any way to determine the relationship between the crack and the signs Harris associates with it? Is it possible that the signs were drawn first, and that at some subsequent period the rock cracked? Is there any way to exclude this possibility?

28 Harris would interpret these Egyptian signs as referring to the horizon. They did not. A separate sign was used to depict the horizon, ☼ (N-27).
A further flaw in Harris’s work is his tendency to engage in argument by assertion. Examples of this practice are numerous. On page 73, where we are told that “in America the butterfly could ‘say’ all that the beetle said in Egypt and say it more effectively.” We are never told how the butterfly accomplished this feat. There is no discussion of the symbolic function of this insect among Native American tribes. On page 96 we are told that “the possible relationship of this Book of Mormon text [Alma 46:12-13] to the central Figure on the Dry Fork Panel is sufficiently obvious not to require argument.” In other words, “doesn’t the figure look like General Moroni?” The author gives no argument in favor of his interpretation because there is none. It is a purely subjective interpretation which the author states must be taken seriously “if one takes the Book of Mormon seriously.”

Perhaps the most significant error in Harris’s work is his assumption that when we read in 1 Nephi 1:2 that Nephi makes a record “which consists of the learning of the Jews and the language of the Egyptians,” the Egyptian language spoken of here is written with hieroglyphs. Harris justifies this assumption with the statement that “Egyptian hieroglyphic writing was regarded by Egyptians as the most appropriate means of expressing sacred information” (pp. 9, 137-38), and he appears to conclude from this that the Nephites would have followed a similar convention and chosen the same writing system to record their “sacred information.” For Harris, this use of Egyptian hieroglyphs would have served as the ultimate source for the signs which he finds in the Native American petroglyphs which form the basis of his study.

The Egyptians, however, did not exclusively employ hieroglyphs for the recording of sacred information. The type of script used depended in large measure on the medium in which the scribe (or more correctly, draftsman) was working, rather than the nature of the text. For the recording of texts on monuments of wood or stone, hieroglyphs were usually used, while records on papyrus or ostraca would have been written in hieratic, a cursive form of hieroglyphs. The same religious texts can be found in both hieroglyphic and hieratic script. For example, from about 1085 B.C. copies of the Book of the Dead
were written in hieratic as well as the semi-cursive hieroglyphs known as linear hieroglyphs. 29

A point that Harris fails to consider is the stage of the Egyptian language with which Lehi would have been familiar. There are five stages of the Egyptian language: Old Egyptian (2650-2135 B.C.), Middle Egyptian (2135-1785 B.C.), Late Egyptian (1550-700 B.C.), Demotic (700 B.C.–5th cent. A.D.), and Coptic (from the third century A.D. onwards). 30 The first three stages of the language were written using the hieroglyphic (and hieratic) script. Demotic, however, only employed an extremely cursive script which was an "abbreviated development of hieratic." 31

Although Middle Egyptian was replaced as the vernacular of ancient Egypt, it continued to be "regarded as the 'classical' stage of the language, used in literary, religious, and monumental inscriptions through to the Graeco-Roman Period." 32 By Lehi's day, knowledge of Middle Egyptian (and Egyptian hieroglyphs) would have been necessary only for priests and scribes who were involved in the preservation and creation of the texts used on monuments and in the funerary literature. Nibley has long ago pointed out that the stage of the Egyptian language with which Lehi would have been familiar was Demotic, 33 and the reason given for his familiarity was that he was a merchant engaged in trade with Egypt. 34 If this was the case, then it is extremely unlikely that Lehi would also have received the specialized training necessary to master the Middle Egyptian stage of the language, as well as the hieroglyphic writing system. Such a knowledge was unnecessary to conduct business in the ancient Egypt of Lehi's day.

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30 Davies, Egyptian Hieroglyphs, 9.
32 Davies, Egyptian Hieroglyphs, 9.
If it is granted that Lehi and his family did not have a reading knowledge of hieroglyphs, then it follows that the brass plates, as well as the record kept by Nephi, must have been in the Egyptian language and script which they could read, which was Demotic. There is, then, no reason to attribute to Egyptian hieroglyphs a sacredness derived from their association with scripture, as Harris does (p. 9). Harris's mechanism for the introduction of Egyptian hieroglyphic symbols into Native American cultures then vanishes, and with it the justification for his attempt at interpreting Anasazi and Fremont petroglyphs as Egyptian signs.

As has been seen, this work suffers from technical flaws and numerous errors of fact. The methodological shortcomings render the author's argument unsubstantiated; all the reader is left with are Harris's fanciful interpretations of a few Native American petroglyphs. I can sympathize with Harris when he states that there were times when he was "sorry [he] ever wrote about" this topic. So am I. Such "spiritual scholarship"35 unfounded in fact serves no useful purpose.

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35 Adapted from Harris, 144.