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Vive Zapato! Hurray for the Shoe!

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Reviewed by John L. Sorenson

Since I first began publishing on Book of Mormon topics, I have urged colleagues to criticize and thus to improve my work. However, the reviews of An Ancient American Setting for the Book of Mormon published heretofore have contributed little that I could use to correct or clarify the book. Matheny’s article is the first review to treat a wide range of the book’s subject matter at a professional level. I find her piece weak in scholarship and faulty in logic. Yet her challenge has had a beneficial effect—it has sent me back for a full look at my own work. I come away confirmed in my view that it is sound, although in sixty pages Matheny was apparently unable to discover its strengths.

Her critique is phrased in a manner that will be welcomed by those who do not take the Book of Mormon seriously or wish it would go away. Some of them are already busy citing her piece as demonstrating that the Book of Mormon and my book fail the test when confronted by her “critical methodology.” Dr. Matheny has since assured me that she did not intend such a condemnation of the scripture, and I am glad to know that. However, no hint of a positive evaluation of the Nephite record is apparent in her critique. I have found by experience that scholars cannot be too careful in phrasing their results relating to a sensitive issue like “archaeology and the Book of Mormon” to avoid twisted attributions. Neither can we be too careful of the publishing company we keep.2


2 Despite care I have taken to make explicit my belief in the Nephite scripture, I was recently described by activist disbelievers as “an honest Mormon doubter” and misquoted to support the imputation that I do not really believe the Book of Mormon is ancient (in the August 1993
Matheny leaves me with a dilemma. Rather than take time and energy to prepare a response to her, I would much prefer simply to move ahead with my projects. But by doing so I would permit the naysayers to claim that I cannot respond, that I have been intimidated by the power of Matheny’s “new approach,” that my book has been disemboweled by her scholarly sword. They could paint me as brooding, defeated, in my “tent.” So, reluctantly, I am obliged to correct the record.

There are five general problems with this critique:

1. Matheny’s stance is often ethnocentric, visibly failing to appreciate how Nephite culture differed from that of the modern Western world. This results in projecting back upon the text her own unjustified notions.

2. She neglects to pay careful attention to the data and arguments in my writings.

3. Her statements are often inadequately documented or explained; that is, she appears to be ignorant of, or at least to omit, crucial current data, as well as to ignore optimal logic and scholarly methods.

4. She readily accepts, or actually prefers, “authoritative” assertions instead of discerning and correcting errors of fact, interpretation, and theory independently—the hallmark of a genuine scholar.

5. There are gaps in her grasp of the epistemology and history of our shared field, Mesoamerican archaeology/anthropology; consequently, she appears naive about its status and limitations.

Summary of Specific “Problems” Raised

Matheny begins by lumping my model of “Book of Mormon geography” with the writings of Hauck, Allen, and unmentioned others in a category she labels the “Limited Tehuantepec Geography.” (I have never used such a category because I consider the differences among the models of the writers mentioned too great for them to be combined usefully.) But then most of the rest of her critique deals with “cultural problems” which she says must be dealt with before any correlation of the Book of

Newsletter of “Concerned Christians and Former Mormons,” Whittier, California.)
MORMON with "the Limited Tehuantepec models" can be considered credible. The following are the major "problem" areas noted:

1. The set of directions I employ in my model does not fit what Matheny considers a "standard system of cardinal directions" which she supposes was used in Bible lands, by Book of Mormon peoples, in Mesoamerica, and in the modern world.

2. Jaredite and Nephite metalworking referred to in the Book of Mormon fails to match the picture she has in mind of Mesoamerican metallurgy.

3. She believes certain weapons, especially swords, mentioned in the Book of Mormon were of metal, but she is not aware of Mesoamerican parallels for weapons she thinks are indicated by certain words in the scripture.

4. Tents are often mentioned in the Book of Mormon. No evidence for a tradition of tent use exists for Mesoamerica, she says.

5. She reads the Book of Mormon as indicating that "economically important plants" were brought from the Old World, but these have not been found in Mesoamerica, she asserts.

6. Animals which she believes are referred to in the Book of Mormon are not familiar to her from Mesoamerica.

7. She says that the site of Santa Rosa, Chiapas, proposed by me as a plausible candidate for the city of Zarahemla, has already been excavated and what was learned does not agree with how she thinks the scripture characterizes Zarahemla.

8. She does not believe that Mesoamerican archaeology provides evidence for cultures comparable to what the scripture describes for the Jaredites of the third millennium B.C.

Summary of My Response

I disagree with the phrasing, discussion, and documentation in her discussion of all these "problems." None of the major issues she raises constitutes a useful or lasting challenge to my work or to the Book of Mormon, nor does her work constitute a contribution to knowledge. A few minor points provide supplementary data or ideas that are of modest interest.

On the first point, the directions, her criticisms are based on the assumption that the Book of Mormon text can be read as "plain" English. She has failed to grasp the significance of my
extensive data showing that Mesoamerican and all other ancient direction systems were constructed on different cultural principles than ours or that Nephite direction usage can reasonably be interpreted in light of what we know from antiquity. I find no merit in her critique on this point. I had long ago considered all of the alternatives the author suggests, only to reject them as simple-minded or contrary to the text.

The discussion on metallurgy ignores or rejects important information I have presented about occurrences and use of actual metal specimens in Book of Mormon times as well as of linguistic evidence for very ancient knowledge of metals in Mesoamerica. Again, none of what Matheny says is new, and none of her arguments persuades me that I should revise my position regarding earlier-than-usually-thought metallurgy.

Point 3: Most of the weapons mentioned in the Book of Mormon can be accounted for by reference to types already known from Mesoamerica. In addition, one must recognize the problems induced by translation to English of the military nomenclature in the text, as well as the incompleteness of the archaeological record due to neglect, until very recently, of serious research on the history of Mesoamerican warfare. Taking those into consideration, I am encouraged, not discouraged like Matheny, about the prospects for resolving remaining obscurities about relationships between Book of Mormon and Mesoamerican weaponry.

Tents, the fourth "problem," are in fact extensively documented in Mesoamerican ethnohistory. Direct archaeological demonstration of their earlier presence is probably impossible because of the ephemeral nature of the structures. The criticism of my position and of the Book of Mormon on this point is seriously flawed because of gaps in the writer's data and logic.

Much relevant literature relating to "problem" areas five and six—plants and animals mentioned in the Book of Mormon—eludes Matheny. She has misconceived crucial related issues by following established, dogmatic opinion which refuses to pay attention to evidence for the trans-oceanic transfer of various types of plants. Moreover, she fails to consider my published clues—especially having to do with translation of the terminology for plants and animals—toward resolving what she thinks are problems with the scripture's statements about flora and fauna. Her arguments I either anticipated in my book or find to lack value.
Supposed difficulties between the Book of Mormon text and what is known about Santa Rosa, Chiapas, result from the limited nature of the archaeological work done there or else from Matheny’s failure to assess critically the reports published on the work at the site. She compounds the problems with the technical information by misinterpreting the Book of Mormon text.

Finally, Matheny betrays defective understanding of current methods and results of dating early Mesoamerican cultures. Her strictures against my dates for the Jaredites are poorly informed and behind the times.

In short, I find the discussion of the eight major “problems” to lack substance and accuracy. Consequently I am unable to accept or significantly benefit from any of the criticisms.

Overall, Matheny has not given me cogent reasons to modify the positions on Nephite geography or culture in relation to Mesoamerica that I took in An Ancient American Setting for the Book of Mormon. Still, the challenge has produced incidental benefits for me. I have been forced to reexamine what I said before, with the result that I am more confident than ever that my 1985 book is soundly based. My model of a considerable degree of geographical and cultural fit between the picture of the Nephites presented in the Book of Mormon and scholarly information on Mesoamerica remains plausible. Yes, the shoe fits—a little stiffly but about as well as most new shoes that need getting used to.

To this point I have given a summary response to the critique. Some readers will stop at this point—bored, disappointed, satisfied, or delighted. Fine. Others will want fuller details. I welcome the chance to provide those, including new information I have not previously published.

From this point on, my detailed comments will connect to the critique by its page numbers. Concerning minor points for which I provide no comment, my silence does not necessarily mean I agree with Matheny’s point; more likely it means fatigue from the length of this response.

Method in Anthropology

I am accused of dealing, in An Ancient American Setting, in “unrelated bits and pieces of information” (p. 269). I plead semiguilty, as would any anthropologist who collects as complete a range of data as possible on the people/culture he studies.
Those not acquainted with how the discipline of anthropology works may consider certain facts “unrelated,” but insiders like Dr. Matheny ought not to do so. The holistic principle, which she must have been taught in her graduate study at the University of Utah if not at Brigham Young University, leads investigators in our discipline to suppose that all “bits and pieces” of information about a culture we study are potentially relatable to themes that emerge only as analysis proceeds. This is as true of the study of “Nephite culture” as of any other. “Bits and pieces” merely means the file is open.

If anything is anathema to good anthropology, it is substituting statements by “authorities” for actual observations. In our field we are specifically, heavily empirical. We accept what informants tell us about a culture only as grist for our analysis/synthesis mill. Neither do we trust what other scholars think who have worked on the same people, unless they document their statements (“in spades,” if they are archaeologists). Many archaeologists have not internalized this need for comprehensive inquiry, probably because in their specialized training and experience as a particular kind of anthropologist, they have not had personally to undergo that bracing experience that social or cultural anthropologists must when they try to make sense out of a strange people’s pattern for living based on fragments of observed behavior. Even the archaeological branch of anthropology, however, depends upon “bits and pieces” of data on the material remains of a culture. Why else do excavators record all that technical stuff about ceramics, for example, except in the hope that someday they can make comprehensive sense of the initially enigmatic details? My “bits and pieces” which Matheny laments are similar, noting data about supposedly absent metals or unexpected animals in anticipation that someday the current paradigm may shift enough to make someone grateful that my “anomalies” are on record and seem to fit. How disappointing, then, to find Matheny urging the contrary; she prefers to settle for a picture constructed from “the archaeological evidence as it is now understood by most professionals in the field” (p. 270, my emphasis).

The havoc wreaked on the search for truth by devotion to authority has been widely documented. For instance, for a case she knows well, Michael Coe’s recent book, *Breaking the Maya*
Code," relates in detail how costly the authority mode of thinking was in the quest to decipher the Maya hieroglyphs. For a long time "most professionals in the field" rejected Soviet linguist Yuri Knorozov's approach to translating the glyphs. The Big Scholars (hereafter abbreviated B. S.) accused him of having come up only with unrelated and illogical "bits and pieces" of decipherment. As Coe tells the story, the archvillain was Eric Thompson, doyen of the "Mayanists" of his day. Thompson attacked numerous fellow scholars, including Matthew Stirling, Benjamin Whorf, Knorozov, and our teacher, M. Wells Jakeman, for reaching conclusions of which he disapproved. 4 Thompson's professional status was such that other B. S. followed him meekly. Not only did Thompson's fluent tongue and acid pen serve to defend his personal intellectual positions, also an ambitious program of popular publication intimidated those unable to match his effect in shaping the public's notions about Maya civilization. Marshall J. Becker has discussed the latter side of Thompson: "The prodigious output of J. E. S. Thompson, the persuasiveness of his style, the persistence of his theme, and his great production of popular works were all factors in disseminating" his theory that Maya civilization ended because peasants revolted against their priests. 5 In the long run, however, Knorozov's ideas on decipherment proved almost completely right and Thompson's erroneous, while Thompson's peasant revolt model for the Maya collapse has been completely discarded.

In archaeology, the same problem of B. S. intimidation is widespread. William N. Irving is distressed about it in relation to "Early Man in the Americas," "where authority may masquerade as a reasoned conclusion, [and] boldly stated opinion may pass for authority." 6 Irving is pessimistic about the supposed

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4 Thompson effectively terrorized Brigham Young University's Jakeman with a review of his book, *The Origin and History of the Maya*, so slashing that MWJ refused ever again to try to publish a professional piece "outside."
authoritativeness of what Matheny respectfully terms "the archaeological record." "The majority of our practitioners and interpreters continue to depend on a normative paradigm in the analysis of artifacts, in which newly found objects must fit in established categories or they cannot be accommodated." Following closely "the archaeological evidence as it is now understood by most professionals in the field" tends to lead to stodgy conservatism based on comfortable but outdated information. That is what Matheny urges on us by playing the "professional acceptance" card.

Her discussion of geography previews what recurs frequently in this critique—failure to appreciate our dependence on the Book of Mormon text, which must lie behind all research on the Nephites (pp. 270–71). In this case she looks to ecclesiastical, not scientific, authority, but the result is similar—diversion from the task scholars face of investigating questions for which the authorities do not know answers. In this case anything that Church authorities—including Joseph Smith—have said about "Book of Mormon geography" is irrelevant if it conflicts with what is in the Book of Mormon itself. Joseph Fielding Smith soundly taught, "It makes no difference what is written or what anyone has said; if what has been said is in conflict with what the Lord has revealed [in scripture], we can set it aside. My words, and the teachings of any other member of the Church, high or low, if they do not square with the revelations, we need not accept them. Let us have this matter clear." Better to drink water from the original spring than to take it from downstream. Anyhow, repeated statements by Church leaders have made clear that there has never been a solution by revelation or fiat to any questions of "Book of Mormon geography." Meanwhile, all serious investigation of the statements on geography in the text

7 Ibid., 532. Or, as William Dever recently put it about the Holy Land, "The archaeological data are unbiased ... only until we (archaeologists) begin to interpret them, and then we introduce our own biases." Authorities have the advantage, of course, that their biases are considered "authoritative." See William Dever, "How to Tell a Canaanite from an Israelite," in Hershel Shanks et al., eds., The Rise of Ancient Israel (Washington: Biblical Archaeology Society, 1992), 29.
itself\(^9\) has demonstrated that a limited—that is, much less than continental—geographical setting for the Nephites is required by the book itself. So the rhetorical posing Matheny engages in on these two pages, as though it mattered what opinions church leaders or members have had about geography, simply draws her and her readers off track.

Matheny’s footnote on a possible Andean scene for Nephite lands reveals innocence concerning the issues involved in any correlation (p. 272). Whoever aims to deal with this subject will save time, trouble and embarrassment by carefully reading my Source Book.\(^10\)

The concept “Nephite north” is not mine, consequently it is not appropriate on a map representing my views (p. 274, map 2, and p. 277). If “unfortunately Sorenson never gives an exact figure” for the difference between “cardinal north” and “Nephite north,” it is because our informant, Mormon, has given us insufficient basis for any specific figure. (It is possible that he thought in terms of a north quarter, not a north point—see below.)

Disappointing ethnocentric naiveté is shown in pages 274–77. For example, the statement is made that “The Book of Mormon account offers what appears to be a standard scheme of cardinal directions” or the “standard traditional interpretation of the direction system.” But the only directional scheme we can find is what we infer from incidental statements made by Mormon in the text, for he never consciously “offers” us the direction scheme in his mind. A greater lapse is the idea that the “standard scheme of cardinal directions” has a long history. That is folk thinking. This supposed “standard scheme” is actually a mental artifact of Western European culture developed largely since the rise of the compass and of science not many centuries ago.\(^11\) I should have thought that anthropologist Matheny would have been aware of the historical lateness and arbitrary nature of this cultural construct. The chief point of Appendix 3 in my Source Book has escaped her—every direction scheme is a cul-

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\(^9\) Beginning less than sixty years ago, as documented in my Source Book as well as in “Digging into the Book of Mormon,” Ensign 14 (September 1984): 29, 36–37.

\(^10\) See especially Parts 6 and 8, plus the summaries in Part 2 of several convoluted failures to fit Book of Mormon events into a South American setting.

tural accident; our European version is no more “true,” no more “obvious,” and hardly more ancient, than any other scheme.

From my perspective as an anthropologist, I can hardly stress too much the obstacle which cultural naiveté poses for scholarship on the scriptures. At point after point Matheny comes across like so many less-qualified writers on the Book of Mormon, unwary of that damning fault—ethnocentric thinking—the avoidance of which is a sine qua non of an anthropological approach. A John Kunich may be excused his naiveté from lack of appropriate training (p. 264: “The plain meaning of the Book of Mormon’s own words”). But Matheny, a professing anthropologist, should not have fallen into the same trap (p. 321: “the plain meaning of the words in the text of the Book of Mormon”). Decades of scholarly research and publication have shown remarkable subtlety in the literary and cultural forms used in writing the Book of Mormon. Had she examined that research literature, she might have avoided this problem. (I say “might” because some of those writing in the Metcalfe volume, while aware of those studies, still choose to assert that the language of the scripture came from nineteenth-century New York.) No, the language of the Book of Mormon may be considered “plain” only at severe intellectual peril. (The basic principles which the scripture teaches are another matter; they manage to come through plain enough—even though, no doubt, subtleties in the teachings still escape us.)

Innocence about the role of culture and its processes in the formation and phrasing of the Book of Mormon and of the lives of the people it treats is further shown by Matheny’s assuming (p. 277) that any Nephite directional scheme must have been brought direct from the Near East. Wider acquaintance with the anthropological literature on culture change processes could have warned her against assuming such simple continuity. The text of the Book of Mormon itself warns against a simplistic interpretation of cultural process. For example, Alma 11:4 tells us that the Nephite system of measures was not the same as that of the

12 She could have looked, for example, at my article, “The Book of Mormon as a Mesoamerican Codex,” F.A.R.M.S. paper, 1977; Donald Parry’s massive The Book of Mormon Text Reformatted According to Parallelistic Patterns (Provo: F.A.R.M.S., 1992); or pieces by Tvedtnes, Hardy, Szink, Goff, Ostler and others in John L. Sorenson and Melvin J. Thorne, eds., Rediscovering the Book of Mormon (Salt Lake City: Deseret Book and F.A.R.M.S., 1991).
Jews, then gives us Jaredite names for certain of the Nephite measures.\(^{13}\) And, as I pointed out in *An Ancient American Setting*,\(^{14}\) cultivation and culinary practices involving “corn” on the part of the Zeniffites and Lamanites (see Mosiah 9:9) had to be as native to America as the plant itself was.

“Picking up a line of argument advanced by Palmer, Sorensen singles out one Hebrew directional scheme” (pp. 277–79). Actually, what Palmer wrote on this point resulted from his reading the widely circulated manuscript of my book, as he made clear in his introduction, even though his volume reached print before mine.

The topic of directions still seems mysterious, not only to Matheny but to other critics and general readers of my work. I have tried several times to make the matter clear, but perhaps one more try here will make the crucial points unmistakable. Six ideas are worth noting.\(^{15}\)

1. All systems for labelling directions are arbitrary and spring from the unique historical, geographical and linguistic backgrounds of specific peoples.\(^ {16}\) Thousands of such schemes have existed in history, and large numbers still exist.

2. More than one system of direction labels is commonly used in a single culture.\(^ {17}\) The sun is involved in many of these, but in varied ways. After all, at best the sun “rises” or “sets” at the same point on the horizon (if that point can indeed normally be seen at all due to terrain, tree cover, clouds, etc.) no more than two days per year as it moves through its annual cycle, hence “where the sun rises,” for example, is indeterminate without further definition. In our society, as in nearly all others, a few specialists (astronomers) determine and tell the rest of us where, for example, “east” or “north” lies. Most people, even today, remain vague about how their culture’s ideal system of directions applies in daily life.

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15 Considerable documentation for them can be found in Sorensen, *A Source Book*, Appendix C.
17 See, for example, J. Kirk et al., “Captain Cook’s Problem: An Experiment in Geographical Semantics,” in M. D. Kinkade et al., eds., *Linguistics and Anthropology* (Lisse, Belgium: de Ridder, 1975), 445-64.
3. Various other criteria (e.g., the rising or setting of certain stars, seeing particular landmarks, or the prevailing wind) may take precedence over the sun.

4. When a people move from one location to another, their system of directions is quite sure to undergo change.

5. What exactly were the cultural and linguistic backgrounds of the directional terminology (or terminologies) used by Lehi's family in the land of Judah? The Book of Mormon never explains, and other sources such as the Old Testament fail to make the matter clear to us either.

6. The Book of Mormon refers to directions at many points, but no attempt at an explanation of their mental model, however brief, is ever given. In fact, almost half of Nephite history passes before the text first uses any direction term. We are left to infer what we can about their system.

As a small contribution to the final point, I here rephrase some "evidence from the Book of Mormon account" (which Matheny calls for) which indicates that they did not follow a direction scheme based on four "cardinal directions" translated as "plain" east, north, west and south. In my Source Book I presented word counts of directional terminology in the present English text. Compressing that information to the maximum, it shows:

"East" + "eastward" are used 38 times ("eastward" just twice). "North" + "northward" used 76 times ("northward" 45 times). "South" + "southward" used 50 times ("southward" 20 times). "West" is used 28 times ("westward" is not used at all).

A lack of symmetry in this scheme is obvious. Unique ideas and usages are implied. Aside from whatever these translated words for directions denoted in relation to the natural world, their use in the language of the Nephites does not seem to show that they paid prime attention to the sun's rising or setting.

Failure to read my book carefully is shown by Matheny's statement that I implied that "when Lehi's party landed, ... they were confused [about directions] by their new surroundings" (p. 277). To the contrary, I explicitly stated: "None of these considerations imply that the people involved did not understand directional realities. Ancient inhabitants ... knew as well as you or I ... where the sun rose. The problem was not one of ignorance but of difference in conceptual framework and language.

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between their culture and ours.”

The scriptural text is too brief to tell us how directions were handled during the early Nephite centuries, the crucial pioneering settlement period. The earliest statement involving directions does not occur until about half of Nephite history had passed (Mosiah 27:6; “east wind” in Mosiah 7:31 may be metaphorical; cf. Isaiah 27:8). So, in regard to the Nephites’ model of directions, we are left with a linguistic end product with no clear indication of how the model arose in the process of Nephite life. What I was doing was trying to explain the result without claiming to know how it came about.

External scholarly sources help us learn about the background of Nephite thought on directions. In my Source Book, I recapitulate data on directional models in the ancient Near East generally and in the land of Judah particularly. The Jews’ emphasis on orienting to the sun may not have come about until Hellenistic influence flourished, well after Lehi’s day. But already the temple at Jerusalem was oriented to the rising sun. However, that orientation was not to “cardinal east,” as Matheny assumes, but to the “east” where the sun rose on fall equinox—quite a different point on the horizon. Only on that one day were the first rays of the morning sun allowed to shine directly through the opened “eastern” door of the temple and into its holy of holies. But apparently Matheny does not understand that two different “easts” are involved, for she says, contradictorily: “This equinoctial orientation would seem to indicate that Lehi’s group was well aware of the positions of the standard cardinal directions” (p. 278). How’s that? Recognizing the sunrise point on the equinox day in no way requires recognition of the arbitrary point we call cardinal east. The east of the temple orientation was conceptually as distinct from cardinal east as “fiscal year” is from “leap year” among us.

Matheny’s discussion of “north” is equally problematic. She says, “It seems likely that travelers would have been aware of

19 Ibid., 42.
20 Ibid., 404–7.
23 Incidentally, she notes (p. 278) that where I use the term solstitial at one point in An Ancient American Setting, equinoctial would be correct. That change has been made in the last three printings of the book.
the rotation of the stars around the north celestial pole” (p. 278). But billions of people reared in large numbers of cultures have lived on earth without realizing this “fact” which she supposes "seems likely.” Anthropologists do not go by what “seems likely” (ethnocentrically and in retrospect) but by what empirical investigation reveals about culturally formatted “knowledge” which particular peoples are known to have controlled and utilized.

She falls into a nominalist fallacy (p. 278) by letting the translated word “compass” determine how she thinks about the “Liahona.” By considering the latter to fit into the same lexical domain as modern English “compass,” she supposes that the device must have pointed out (cardinal) directions to Lehi and Nephi. But magnetism could not have been the operative basis, since faith was (see Alma 38:40). If operative on the natural, magnetic principle, why would the Liahona have ceased functioning as soon as Nephi was tied up (1 Nephi 18:12) and start again when he was released? And how would a magnetic compass produce written messages (see 1 Nephi 16:26–27)? Clearly the Liahona was only vaguely like compasses we know; it was called by the same English word because, indeed, it was a direction pointer, but what it pointed to was the Lord’s choice of route, not a fixed direction based on magnetism. In any case “compass” is used only seven times, but “director(s)” ten times, “ball” five times, and “Liahona” once. Such diversity of terms indicates that Nephi or Mormon was dealing with a meaning not conveyed neatly by any one of the terms in English.

It is in relation to Mesoamerican directional systems, however, where her notion of a standard set of cardinal directions falls apart definitively. On page 279 she is still making it appear that a single conceptual scheme governed the native peoples of Guatemala, my suggested land of Nephi. The evidence offered won’t do, however. By citing Tedlock on Quiche Maya directions, she conveys to a reader unacquainted with my sources on the same subject that she is showing me something I had overlooked. And by quoting only this one work and construing the words as she has, she implies that contemporary Amerindian groups in highland Guatemala share a single view of directionality which coincides with her “cardinal directions” notion, and, by implication, with the Book of Mormon. But she ignores my Source Book, which quoted studies by Vogt, Nash, Neuenswander, Gossen, and Coggins on Guatemala to make the point
that "frameworks (for directions) vary in detail from locality to locality." Tedlock adds nothing new, just one more (incompletely reported) case. The materials I cited show that cardinality is not necessarily a major consideration and may not be involved at all.

There is more in the literature that may clarify the Nephite system. Franz Tichy, who has studied Mesoamerican directions perhaps more than anyone else, says that the four "standard" cardinal directions "appear to have little meaning in (ancient) Mesoamerica." Rather, "the times of sunrise and sunset on the horizon on the days of the solstices define, with zenith and nadir points, the six cardinal directions of Mesoamerica." I pointed out that the resulting angle between Mesoamerican "north" and "west" and between "east" and "south" in Tichy's sense is only about 50 degrees, not the 90 degrees of a "standard" cardinal setup. Tichy's view is now supported by many other studies. For example the Zoque people in a remote community in easternmost Oaxaca consider the world to be rectangular, with corners at the points where the sun emerges and sets on the solstice days.

In my Source Book I quoted no less an Establishment figure than Professor Evon Z. Vogt of Harvard on the noncardinal nature of highland Maya directions. I have since come across new information from him that rules out completely the idea that "standard" cardinal directions were used by the Maya. Since his results have been published only in an obscure volume, I quote him here as a convenience; perhaps it will once and for all exorcise the notion of a Mesoamerican system of "cardinal directions":

25 For a non-"cardinal," non-solar based system which I have not mentioned previously, see now José Fernandez, "A Stellar City: Utatlán and Orion," paper presented at an international symposium, "Time and Astronomy at the Meeting of Two Worlds," Warsaw, 27 April–2 May 1992, which shows that all major temples at the late Quiché capital of Utatlán were oriented to the heliacal setting points of stars in Orion, not to the sun at all.
26 Sorenson, A Source Book, 410.
27 Ibid.
28 See Carlos Muñoz M., Crónica de Santa María Chimalapa: en las selvas del Istmo de Tehuantepec (San Luis Potosí: Molina, 1977), 140.
Recent work by archeoastronomers, literary critics and art historians, and field ethnographers challenges the long-accepted anthropological conclusions that the Maya recognized the four cardinal directions (North, South, East, and West) found in European culture, and that colors, gods, animals, birds, trees, and flowers were associated with these directions. Since the ancient Maya lived in the tropics, they observed a different kind of sun behavior than did peoples living in higher latitudes. The Maya had a poor view of the Pole Star and no constant view of circumpolar stars; instead, the ecliptic included their zenith. Hence, an East-West line could not divide their sky into areas where the sun was and was not (Aveni, 1981; Brotherston and Ades, 1975; Coggins, 1980, 1982).

For the Maya, the rising and setting sun (for which there are words in all Mayan languages) formed the basic orientation in the universe, and the North-South line was not drawn as it was in the Old World. According to Brotherston (1976), the Mayas had no concept for ‘North and South’; rather, the two other ‘directions’ indicated in codices and glyphs most probably meant ‘moments in between’ sunrise and sunset. In 1983 Bricker demonstrated that the four directional glyphs can be read phonetically as ‘East, West, Zenith, and Nadir.’

Most likely the concept of cardinal directions described in the literature on the Maya derives from Spanish chroniclers and early anthropologists. (emphasis added)

Contemporary Mayas of Quintana Roo, Chiapas, and Guatemala (Gossen, 1976; Villa Rojas, 1945; Vogt, 1976; Watanabe, 1983), when speaking in Maya rather than Spanish, use only words for ‘rising’ and ‘setting’ sun and differentiate between the two sides of the path of the sun (what we call North and South) by right- and left-hand symbolism, or in some cases by speaking of ‘up’ or ‘down’ (Watanabe, 1983). The Watanabe data from the Mam area of northwest Guatemala demonstrate that this view of the cosmos is built into the very structure of the language.30

Vogt further found that the Hopi, Zuñi, and Tewa pueblos of the Southwest fail as much as the Tzotzil Maya to have any “precise notions of the location of the four cardinal points on our compass. Rather, the emphasis is on the rising and setting sun on the horizon and the solsticial positions of the sun. North and South tend to be regions rather than precise compass points. The four solstitial points (among the Hopi or Zuñi) and/or the rising and setting sun and zenith and nadir among the Tzotzil Maya present the only case to be made for four cardinal directions.”

William F. Hanks has published complementary information which can help us understand “north” in the Book of Mormon. Yucatan Maya shamans today, apparently following very ancient tradition, distinguish between cardinal places and cardinal directions. The former are rather generalized areas (agreeing with Aztec thought: “The directions south, east, north, and west were viewed not as distinct points, but as quadrants”). Of course the Book of Mormon refers to quadrants (“quartets of the land”). Hanks compares the manner of speaking about directions in the Yucatec Maya language to what Haugen found in Icelandic. For the two equally, “we must know the speaker’s destination as well as his current location in order to compute directional reference.” The “quarter” to which one was headed, not intermediate points en route, determined statements about a traveler’s direction. Certain journeys, heading literally west, for example, would be spoken of as going “north” if the destination fell within the defined north quadrant. On this logic, a Nephite headed to “the land northward” could be said to be traveling “north” even though his momentary path was to the east or west.

While it may be tiresome for me to present this material, a version of which was laid out in my Source Book, I have felt the

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31 Ibid., 493-94.
35 Hanks, Referential Practice, 303; Haugen is summarized in Sorenson, A Source Book, 403.
36 Hanks, Referential Practice, 303.
need to do it since, as far as I can tell, Dr. Matheny has failed to grasp its significance. Others have missed the point, too. I trust it will now be unmistakable for all that the only productive approach to understanding directions in the culture of the Nephites is to forget our own folk myth that the cardinal directions are "obvious" and look instead to real models from the historical Near East and Mesoamerica which are based on entirely different premises and to which the Nephite system was plausibly related. Nothing known to scholarship and science about direction systems hinders accepting a "limited Tehuantepec model" of geography.

I had expected Matheny to do better discussing specifically archaeological topics, but her undocumented mention of Tomb 12 at the site of Río Azul, Guatemala, immediately raises a problem. She says that a glyph which reads "sun" or "day" is there painted on the "east wall," while a glyph for "night" or "darkness" is on the "west wall" (p. 279). Actually the site has not been fully reported (that I am aware of), and in the absence of a definitive map, we cannot know to what "east" or "west" the walls may have been oriented. Tichy and Vogt would be surprised if they faced our cardinal points. And perhaps Matheny is unaware that actually eight glyphs appear on the tomb walls, four for intermediate (?) directions as well as the four she referred to. The result is an eight-fold partitioning of space, yet we remain unclear on exactly how any of these glyph markers relate to our directions.³⁷

The complaint is made that I "ignore" the Yucatan peninsula, a "large and important area containing some of the largest cities ever built in Mesoamerica" (p. 280). Rather, in my studies I simply looked at the text of the Book of Mormon and there found no reference to an area having the characteristics of the peninsula or of most of the rest of the lowland Maya zone. I realize that this offends the sensibilities of those (perhaps Matheny is among them) who think of "the Maya area" as hav-

³⁷ See David Freidel, Linda Schele, and Joy Parker, Maya Cosmos: Three-Thousand Years on the Shaman's Path (New York: Morrow, 1993). In the galley-proof copy I saw—lent by John E. Clark—the tomb glyphs were discussed on pp. 41 and 359, while figure 2:8c reproduced George Stuart's drawing of the eight glyphs. This fascinating new volume emphasizes how markedly different was/is the Maya conception of a stellar-marked cosmos from our modern solar-cosmic view. For a preview, see Archaeology 46 (July–August 1993): 26–35.
ing cultural primacy. The fact is, though, that the area was not particularly central to developments in Mesoamerica. Mayanist scholars have insisted on the area’s importance in the same way as the Classicists who long insisted that Greece and Italy were the centers of the “civilized world” or oikoumene of western Eurasia. Historians and archaeologists with a broader purview found, nevertheless, that for thousands of years the Egyptians had largely ignored what they considered the culturally retarded Greek and Latin speaking zones to their north (westward), while one Mesopotamian kingdom and empire after another knew them only as second or third order places. “The Maya area” was similarly peripheral through a good deal of Mesoamerican development. Now, if the Book of Mormon text were to refer to a tropical lowland and peninsula that seemed to fit the Yucatan peninsula, I would have included it in my correlation. But in my reading of the text, it omits practically all the Maya area from the Nephite mental map, so I omit it too.

An interesting contradiction appears at this point in the critique. Whereas the author had chided me for not paying attention to “the Maya culture,” now she finds fault with those of us who take “examples and analogies” from that area to apply to the Book of Mormon. But of course both the area to the south, which I consider the actual scene of Book of Mormon events, and Yucatan are parts of the same “culture area.” The concept of culture area involves the sharing of cultural features over a defined territory. The culture area on which I drew for comparisons constituted all of Mesoamerica. It is as logical for me to use analogies drawn from non-Nephite Yucatan to illustrate life in the Book of Mormon as it is for other scholars to apply cultural analogies from the recent Bedouin in Arabia to illuminate life among the ancient Israelites of Palestine. Had I cited Mayan cultural ways not as analogies to Nephite or Lamanite life but as direct consequences of or historical derivations from peoples specified in the Book of Mormon, then I could be methodologically culpable. But I did not do that.

39 For a theoretical discussion and examples, see A. L. Kroeber’s classic, Cultural and Natural Areas of Native North America (University of California Publications in American Archaeology and Ethnology No. 38, 1939).
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Archaeology

I had expected real substance in her section entitled “Archaeology” (pp. 281–82), since this is Matheny’s specialty. But she begins by outlining Mesoamerican culture history so simplistically as to give us almost a parody. For example, it has been years since active researchers divided the Classic into just Early and Late segments. Most Mesoamericanists have referred to a Middle Classic since the 1978 Pasztory volume popularized Parsons’ concept. She also fails to mention the Protoclassic and Epiclassic or Terminal Classic, other commonly employed subdivisions. Perhaps she is only writing simply for the benefit of nonprofessional readers.

Dates for the major divisions of the sequence are discussed both on pages 281–82 and on 317–20, but I will discuss the whole subject of chronology at this point.

Matheny says that when I date Mesoamerican pottery and accompanying agriculture-based village life back to about 3000 B.C., this “seems to be too early for the current dating of the beginning of ceramics in Mesoamerica.” She further states, “this beginning is dated no earlier than about 2400 B.C.E. at Puerto Márquez” on the coast of Guerrero and “about 2000 B.C.E. in the Tehuacán Valley of Puebla, Mexico.” “Corrections of the C-14 dates could push these dates back somewhat but likely not 600 years,” she concedes vaguely (pp. 318–19). However, only fragmentary excavations have been carried out so far on sites of this period. It is unjustified to think that these limited results have yielded a representative picture of life in that early era. Who can say what will be revealed when more serious work is undertaken? A parallel comes to mind from the Near East in the 1950s. Braidwood had recently discovered the neolithic agricultural village of Jarmo, Iraq, and it seemed daringly early, well before 4000 B.C.; surely nothing could be much earlier. Yet subsequently, sites like Çatal Hüyük in Anatolia were found which have pushed the neolithic time boundary back by thousands of years. For Mesoamerica much information has already appeared (which Matheny apparently does not control) that moves village agricultural life back earlier than she allows.

An archaeologist undertaking this kind of critique should be up-to-date on dating methods, notably the radiocarbon method, but Matheny fails. Multifarious technical problems are now
known to best the C-14 technique, but new physical and statistical procedures have also expanded its possibilities.40 The dates Matheny refers to are evidently in “radiocarbon years,” which must be corrected or calibrated in order to fit our normal calendar. The dates to which she refers are incorrectly labelled “B.C.E.,” but according to the commonest convention in the literature on the radiocarbon method, she should have used “b.c.” to indicate that the dates have not been corrected or calibrated. Perhaps the reason she failed to be aware of this terminology is because she did not look beyond Adams’ archaeology textbook for her information on carbon 14. For the dates I give below, this correction has been done.41 Calibrated dates can be expressed only as statistical ranges, not as single years; this means that there is a 95% chance that the real age falls within the indicated range. A few of the dates that apply to early Mesoamerican pottery-users, and presumably agriculture-based villages, are:

Puerto Marquéz, Guerrero (Brush): 3765–3000 B.C., ± 140 years (This is the date Matheny gives as “2400 B.C.E.”)
Zohapilco, D.F. (phase containing the earliest ceramic figurine) (Niederberger): 4085–3645 B.C. ± 110
Cuicuilco, D.F. (presumed “Tlalpan phase”) (Heizer and Bennyhoff):

\[
3160-2635 \text{ B.C.} \pm 120, \text{ and } 2900-2325 \text{ B.C.} \pm 100
\]

Cerro Chacaltepec (Chalcatzingo), Morelos (Grove):

\[
2310-1735 \text{ B.C.} \pm 90
\]

Teopantecuánitlán, Guerrero (an “Olmec city”) (Martínez Donjuán):

\[
2115-1640 \text{ B.C.} \pm 110
\]

San Lorenzo Tenochtitlan, Veracruz (Bajío phase) (Coe and Diehl):

\[
2310-1735 \text{ B.C.} \pm 90, \text{ and } 2900-2325 \text{ B.C.} \pm 100
\]


1910–1435 B.C. ± 120
San Mateo Atlatongo, Mixteca Alta (Zárate):
2100–1500 B.C. ± 130, and
1920–1670 B.C. ± 130

Others could be cited in the same range, but these are sufficient to show that village life and ceramics were widespread in Mesoamerica during the period I assign to the early Jaredites.

To the contrary, the bracketing dates she uses (e.g., “Early Preclassic,” “1500–1200 B.C.E.”) are not critically acceptable today. As a result, her argument on pages 318–19 in regard to early village life and the Jaredites is passé. This is ironic, since she makes a point of presenting herself as the experienced archaeologist (on p. 291 especially)—implying a contrast with me, who at best claims to be a former archaeologist.

Next she chooses to discuss “a core of cultural problems” which she thinks a correlation between the Book of Mormon record and Mesoamerica must resolve (p. 282). Nowhere does she give a frank explanation for this dominant concern with “problems.” She might first have drawn attention to the sizable body of cultural information in the Book of Mormon which patently agrees with Mesoamerican culture. Nowhere does she let readers know that such a positive corpus exists. For example, she could have cited my paper, “The Book of Mormon as a Mesoamerican Codex,”42 or my article, “The Significance of an Apparent Relationship between the Ancient Near East and Mesoamerica,” in the standard scholarly volume on its topic.43

Her insistent emphasis on “challenges” and “problems” may invite some Latter-day Saint readers to question her opinion of and approach to the scripture. But, of course, problems are what stimulate any research. I raised my own share of problems in An Ancient American Setting—scores of them for the first time. Moreover, I welcome all serious efforts to define and solve remaining problems in interpreting the Book of Mormon in the

light of external sources. My preference, however, is to provide a balanced picture of the status of Book of Mormon studies by pointing out problems that have already been successfully dealt with at the same time that I direct attention to those yet facing us. Matheny's treatment is unbalanced.

One result of her publishing this piece with such emphasis is that her non-Latter-day Saint archaeologist colleagues who see it will quickly classify her apart from the handful of us Latter-day Saints who are overtly trying to reconcile our knowledge of ancient America with what we know from the Book of Mormon. Many secular archaeologists (if they have an opinion at all) feel either scorn or embarrassment for those of us who see the Book of Mormon as a genuine ancient record. (They themselves have not studied the volume as a cultural record in any depth, of course.) Many of them view faith in Mormonism as a bar to doing reliable research on Mesoamerican or any other civilization, just as Thompson impugned the quality of Knorozov's scholarship by carefully labeling him "Marxist." By publishing this article, Matheny escapes the onus, whether that was her intention or not. I wish she had not distanced herself so markedly from my position but had indicated willingness to assist positively in the remaining tasks that a responsible scholarly approach to the scripture will entail.

Inadvertently perhaps, her focus on "problems" while omitting the positive side, puts her methodologically in the same as J. E. S. Thompson's critical methodology in terms that recall her approach in this critique. Thompson attacked "three of [linguist Benjamin] Whorf's weakest cases [of proposed glyph decipherment], . . . worrying them to death, while at the same time deliberately skirting the truly important part. . . . On the unwary or unwise, this methodology makes a great impression—you attack your opponent on a host of details, and avoid the larger issues."44 This looks to me very much like Matheny's ("new"?) approach here.

In her treatment of metals (pp. 283–84), she gives no hint of recognition that words for "metal" existed in nearly all the Mesoamerican languages which linguists reconstruct as going back to Book of Mormon times. In An Ancient American Setting I had said, "comparative linguistics shows that metals must have been known, and presumably used, at least as early as 1500

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44 Coe, Breaking the Maya Code, 139.
That date extends back to the time of the Jaredites, for which so far we have not a single specimen of actual metal. Does it not seem likely that specimens are going to be found someday?" Instead of acknowledging this significant information, she gets hung up with a narrow view of archaeology, insisting that, "No evidence has been found that metallurgy was practiced by the Olmec civilization" (p. 288). By "evidence" she means physical remains, ignoring the names for metals.

She goes on, "[If metals were used by Book of Mormon peoples in Mesoamerica] somewhere there should be the mining localities and their associated tools, processing localities and the remains of the metal objects that were produced" (p. 288). Indeed there should be. Meanwhile, until archaeologists figure out how to find and identify those remains, there is the undeniable presence of a term for metal in the language widely considered that of the Olmecs, Proto-Mixe-Zoquean, as well as in all other major proto-languages of early Mesoamerica. Is linguistic evidence to be excluded from the study of archaeology when it is inconvenient? Shouldn't we be trying to shed maximum light instead of defend status quo interpretations?

She makes much of the fact that metal processing sites are known in the civilized portions of the Old World (p. 284). But as recently as fifty years ago the same lack of narrowly "archaeological" evidences for metal processing prevailed in the eastern hemisphere as for Mesoamerica now. But vastly more archaeology has been done in the central portions of the Old World—probably more in a single year than gets done in a decade in Mesoamerica. Experts have looked more, and they have found more (there was no doubt more to be found anyhow). Eventually many more "traces of such ancient metallurgy"

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will be found in Mesoamerica, for, as the names witness, some metal obviously was in early use.47

We may not need to find "new" specimens or sites as much as we need to reassess old ones, few of which have received more than limited attention by qualified experts. E. J. Neiburger recently applied xeroradiography to artifacts of the Old Copper Complex of Minnesota, where it has always been supposed that only cold-hammering of nuggets was used in making the more than 20,000 copper artifacts known from around the Great Lakes area. His study found, to the surprise of nearly all archaeologists, that some of the artifacts appear to have been cast, and at least one "provides firm evidence of casting."48 "Excavated," if it is clear, does not mean "studied properly"—in Minnesota or in Mesoamerica.

All this is no more a problem for the Book of Mormon than for ancient Mesoamerica and, indeed, the Americas generally. The West Indies area—where the Spanish conquistadors laid hands on so much "gold" that their appetite for it became insatiable and led them to the mainland—had yielded a total of only nine archaeological specimens of any kind of metal as of two decades ago.49 Daniel Rubín de la Borbolla made the same point about the weak representation in museums of what the Spanish records emphasize was a great deal of Tarascan "gold."50 Bray emphasizes for the Americas generally "how inadequately the archaeological discoveries reflect the actual [ancient] situation" regarding metalworking. But he puts the onus of clarification on the archaeologists rather than casting doubt on the accuracy of historical traditions: "If we are ever to get an accurate picture of aboriginal metal technology, archaeologists must be persuaded to look for foundry sites."51 Unfortunately Spanish eyewitness

47 Note that Ross Hassig relies on linguistic evidence—"the Maya word for sling going back as far as 1000 B.C."—to counter the lack of archaeological evidence for that weapon in the Maya area; see Ross Hassig, War and Society in Ancient Mesoamerica (Berkeley: University of California Press, 1992), 205, n. 51.


accounts show that such craft sites were small, unobvious and apparently rarely located within the types of settlements routinely investigated by archaeologists. Compare the statement by Earle R. Caley and Dudley T. Easby, Jr.: "Direct archaeological evidence of smelting operations is rare in pre-Conquest Peru and unknown in Mexico for all practical purposes." This does not mean there were no smelting operations—quite surely there were—but that their locations have yet to be discovered due to inadequacies of archaeological strategy and technique.

Matheny also states that "complex technological processes generally leave traces in the archaeological record" (p. 284). While logically that is true, in reality little useful information has been recovered so far by Mesoamerican archaeologists about most "complex processes," not just metals. Obsidian working is an example—though not particularly "complex"—where archaeologists, by minute examination of the artifacts and waste fragments produced by ancient and experimental flint-knappers, have achieved considerable knowledge of the methods used. But how stone monument carving, textile manufacturing and dyeing, wood carving, jewelry crafting and many other processes were conceived and performed is known only imperfectly, and that virtually never by the discovery or excavation of workshop sites. Thus Matheny's rhetorical expectation that archaeology should reveal direct evidence of technical methods is out of touch with the realities of today's archaeology. Again, this is not a "Book of Mormon problem" but one for professional archaeologists broadly.

It is a mistake to look for complications where there is no need (p. 285). Yes, brass is an "alloyed metal," usually intentionally made by mixing copper and zinc, yet sometimes the alloy results from smelting ore which naturally contains both copper and zinc, hence mention of "brass" objects does not necessarily imply "a sophisticated development of non-ferrous . . . metallurgy among the Jaredites" but perhaps only a modest knowledge. The Book of Mormon text says almost nothing about metallurgical techniques, and what is said need not be interpreted as involving particularly complex operations. Consider the case of Peru, whose museums display abundant metal arti-

factors, yet Bray emphasizes the “rudimentary nature” of the equipment and methods used for processing, while Peruvian miners, he says, employed only “the simplest possible technology.” A lesson that Matheny needs to learn from this case and others like it in her discussion is that problems and explanations, in archaeology as well as in reading the Book of Mormon text, are best phrased in terms no more complicated than necessary.

Still, even limited by a metalworking technology that was quite basic, Mesoamerican smiths eventually produced a lot of metal and crafted it with great skill. For example, Cortez was given whole bars of gold when he landed in Veracruz. But of the “immense riches” and “huge quantities . . . of golden objects” the Spaniards found, “the number [surviving in American museums] is negligible compared to the great quantity” sent by the Spaniards to Europe, where “most of the metal objects were melted and made into bars.” Estimates are that at least 350 kilograms of silver and 4,000 kgs. of gold were looted from Mexico at the time of the Conquest, and 61,000 kgs. of silver and 8,000 of gold from Peru.

Despite the simple means they employed, the metalworkers did remarkable work. Albrecht Dürer, the son of a European goldsmith, saw Aztec metal artifacts in Brussels in 1520, and praised the results roundly: “I have never in all my days seen anything that so delighted my heart as these things. For I saw amazing objects and I marvelled at the subtle ingenuity of the men in these distant lands.” Clearly the “curious workmen, who did work all kinds of ore,” among the Nephites (Helaman 6:11) or the like among the Jaredites, need not have had “a sophisticated development of . . . metallurgy” nor have involved “complex technological processes,” as Matheny puts it, beyond what the Aztecs knew. By exaggerated language she has made a technological mountain out of a molehill.

The principle of avoiding unneeded complications applies also to the reading of texts, here with reference to the “abundant”

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metals reported by the Nephites. “Abundant” is what anthropologists call an “emic” concept, a word whose meaning has to be construed in the culture’s own terms. The statement in 1 Nephi 18:25 on discovering ores refers to a point in time when Lehi’s party had just landed. Those men available to explore could not have exceeded ten in number. Consequently their search for and discoveries of ores would only have been cursory and local, extending at the maximum 25 miles from the landing site. The same caution applies to interpreting “great abundance” in 2 Nephi 5:15 and “abound” in Jacob 2:12 and Jarom 1:8. Those expressions reflect the viewpoint of small communities, perhaps a single village. We must not distort the record by transforming the “emic” sense of “abundance” in the minds of the first few Lehites and Nephites into “etic” (i.e., objective, geological) abundance on a scale of hundreds of miles throughout Mesoamerica.

Here again is an unjustified reading of the Book of Mormon text (pp. 285–86). Matheny first refers to the Jaredites’ manufacture of “swords of steel” (Ether 7:9). Whatever this statement may have meant to the original writer, they are never again credited with using “steel.” Millennia later, Mosiah 8:11 informs us, Zeniffite explorers brought back from the zone of the final Jaredite battle “swords, the hilts (of which) have perished, and the blades (of which) were cankered with rust.” Matheny supposes that the reference to “rust” means that those objects were “of ferrous metal,” that is, by implication, some form of real “steel.” But they could just as well have been copper, which also rusts. On the slim basis of these two time-bracketing statements, she supposes that “metal swords” were “the weapon of choice” over the intervening thousands of years, since no other material is mentioned. Maybe so and maybe not; the short text does not permit settling the matter; however, to get so much inferential mileage out of a single verse followed by silence from the text is unjustified treatment of the document.

I believe she also misconstrues 2 Nephi 5:14: “I, Nephi, did take the sword of Laban, and after the manner of it did make

many swords." The next verse continues: "And I did teach my people . . . to work in all manner of wood, and of iron, and of copper, and of brass, and of steel, and of gold, and of silver, and of precious ores" (2 Nephi 5:15). Verse 16 uses language parallel to verse 14: "I, Nephi, did build a temple; and I did construct it after the manner of the temple of Solomon save it were not built of so many precious things; for they were not to be found upon the land. . . . But the manner of the construction was like unto the temple of Solomon." As I read verse 14, "after the manner of" does not refer to the material used but to the "manner of construction." That is, the general pattern or form of the Judahite temple, and no doubt its function, were copied, but different materials were necessarily used. So when the phrase "after the manner of" is applied to copying Laban's sword, should we not construe it similarly? That is, Laban's weapon was replicated in function and general pattern, but different material could have been used for the new weapons (Matheny offers helpful citations on the use of hard wooden "swords" in Mesoamerica). The copies might have been of metal, but need not have been. The text fails to settle that question. Note also that the statement about weapons (2 Nephi 5:14) is made before that about working metals (2 Nephi 5:15) and no attempt is made by the writer, Nephi, to connect the two; had a connection been intended, one would have thought the statement about metal-working would have come first, then the mention of weapons preparation. It seems a sound rule to pay as much attention to what the text does not say as to what (we think) it does say.

Matheny appears not to consider the Hebrew language meanings of the word translated "sword" in the King James version of the Bible. "Sword" does not have to be of metal, hence the Book of Mormon is indeterminate about the material used when it is read as a translation from Hebrew.

Matheny discusses Mesoamerican ore sources but inexplicably refers to "mineralogical maps of Mexico" based on present-day commercial exploitation of minerals (pp. 287–88). I would have thought she would follow her training in the documents from the period around the Spanish Conquest to find out where the peoples of Mesoamerica then obtained metals. The location of modern mines is irrelevant. Contrary to the

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geographical picture she offers, placering, the commonest pre-Columbian method employed, was used in Veracruz, Oaxaca, Tabasco, and Chiapas states in Mexico and in Belize, El Salvador, and Guatemala. Furthermore, Clair Patterson argues that ores in ancient times were easier to locate and exploit than in late pre-Spanish times, by which time many surface sources were likely to have been exhausted. Hence even the ore locations known to the Indians at the time of the Conquest might not reflect fully the wider sources accessible in the Book of Mormon era.

Matheny expects that metal objects would be found in tombs of the Olmec era if such objects existed then. The number of known tombs of that age is very limited, and those that have been dug typically contain few artifacts, for whatever reason. If we are going to speculate, and we are all forced to do so at present for lack of concrete information, it is at least as reasonable that valuable metal objects would have been passed carefully down to heirs rather than being stuck into tombs where, experience would have shown, they would in short order “canker with rust” like the sword blades of the Jaredites did after less than 400 years. Anyway, the linguistic data going back to the Olmec period assures that metal was in use, whatever the tombs show.

Iron ore used in the manufacture of Mesoamerican mirrors (p. 289) could have been included within the general category of “precious ores” sought and worked by the Nephites (cf. Helaman 6:11).

Matheny cites K. Bruhns to the effect that “all Classic period metal objects found in Mesoamerica are obviously southeastern in manufacture” (p. 290). That is not obvious at all. Neither Bruhns nor anyone else has technically examined a significant number of the known metal artifacts. Rather she is making an assumption on the basis condemned by renowned expert Dudley Easby: “The majority of scholars, relying on circumstantial evidence, believe that fine metallurgy in ancient Mexico was limited to a few centuries before the arrival of the Spanish,” (my emphasis) but “it seems to me that their theory leaves much to be

explained.”63 Bruhns’ opinion is based on circumstantial evidence, not analyses. I’ll be very interested in what hard tests reveal, if they are ever done. There is no question that some early metal pieces were prepared locally and in local styles. That alone vitiates Matheny’s statement that “The few [specimens in the list in Metals, Part 4] that are genuinely Early Classic or slightly earlier seem to be trade pieces not produced in the area” (p. 291).

Matheny’s treatment of the rings from La Libertad (p. 291) underlines the problem I have faced of getting satisfactory information on apparently early metal specimens. On the basis of limited information in the only source I knew, an unpublished report, that described the objects from La Libertad, I suggested the rings likely dated to the Late Classic, for the report said nothing about Post-Classic materials being present at the site. I listed the rings in my evidence category “I,” “incomplete information,” and tentatively assigned a date of A.D. 600–900 in my table of “Probable and Possible Pre-A.D. 900 Mesoamerican Metal Specimens.”64 Now, eighteen years after the dig, Matheny is able to report more about the circumstances, but only on the basis of a private communication from the responsible archaeologist; the formal site report is still “in preparation.” I appreciate the additional information thus dug out. The cavalier reporting so typical of most archaeologists responsible for specimens that I reported in my list is one reason little firm data are at hand about early metalworking. Given the glaring gaps in professional communications on this matter, I feel confident that additional early metal specimens have come out of the ground but have not reached print. All clarifications are welcome.

“Sorenson suggests that use of metals among Book of Mormon peoples was primarily ornamental” (p. 292). It is more than a suggestion. Examination of the scriptures on metal use, listed for convenience in Part 5 of my “Metals and Metallurgy” paper, shows that in every case where a conceptual/social context is indicated for metal use, which is a majority of the statements, it is associated with terms like “rich,” “enrich,” “ornament,” etc. This is true of Jarom 1:8 also, which Matheny

64 Sorenson, “Metals and Metallurgy,” 58.
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has taken as dealing with practical implements. The text says that the Nephites "became exceedingly rich in [1] gold, and in [2] silver, and in [3] precious things, and in [4] fine workmanship of wood, in [5] buildings, and in [6] machinery, and also in [7] iron and [8] copper, and [9] brass and [10] steel." Donald Parry shows that in this verse all these products are linked under "rich" in a Hebrew literary form called "synonymia." The verse is not, then, a description of utilitarian artifacts but a poetic expression of the culture's "emic" classification of "riches." (Incidentally, Patterson, cited above, believes that, in ancient America generally and Mesoamerica particularly, metals were used mainly for ornamentation and social symbolism rather than for utilitarian artifacts.) This leaves only 2 Nephi 5:15, referring to Nephi soon after the initial landing, to speak of utilitarian metalworking (the Jaredites aside). Perhaps difficulties of access to, or technological problems in treating, the local ores made it difficult for craftsmen after Nephi's day to continue some of the technical practices which he optimistically initiated. (There are cultural parallels among historical immigrating parties elsewhere.)

Weapons and Tents

Matheny's discussion of the *macuahuitl* as a sword is helpful, though not exhaustive (pp. 292–93). Artistic representations of Mesoamerican armed men include weapons going beyond currently recognized categories. Bernal Diaz mentioned a kind of "sword" among the Aztecs in addition to the *macuahuitl*. More careful work needs to be done to complete the inventory of arms used in Mesoamerica. Only then will a full discussion of how Book of Mormon weaponry fits with that of Mesoamerica be possible.

67 Hassig's *War and Society* moves that project ahead, but more must be done. He is neither complete nor accurate on southern Mesoamerica particularly nor on the early periods throughout the area. See, for example, Francis Robicsek, "The Weapons of the Ancient Maya," in Bruno Illius and Matthias Laubscher, eds., *Circumpacifica. Band 1: Mittel- und Südamerika. Festschrift für Thomas S. Barthel* (Frankfurt: Lang, 1990), 369–96.
While noting the *macuahuitl* for the Spanish conquest period, Matheny questions whether swordlike weapons existed at all in Mesoamerica during Book of Mormon times and whether they were present in the "proper areas" to fit my correlation model: "There is very little evidence from the archaeological record to support these latter two assumptions" (p. 293). Elsewhere I have discussed the fact that little research has been done on most aspects of Mesoamerican warfare. In two articles, I have pointed out the deficient state of studies of warfare. Armillas (with whom I worked) and Palerm said years ago, and Webster more recently, to largely deaf ears in the profession, that war was much more common and earlier than acknowledged by the vast majority of Mesoamericanists. The point has been gaining ground. I was able to show that fortifications, the most obvious archaeological evidence for war, date throughout all but the earliest part of the Mesoamerican sequence. The same point could be made by studying representations of martial figures and captives in art.

Meanwhile the power of a single lucky dig to reshape our picture of warfare in the past is underlined by a University of Michigan project under Charles Spencer and Elsa Redmond. They found direct evidence from the period 200 B.C.–A.D. 200 for the presence of a *tzompantli* or skull display rack, the same device used about 1500 years later by the Aztecs to psychologically terrorize subject peoples. Until this surprise find, nobody had imagined that this feature extended so far back in time. So I do not believe it matters if, at this moment when hardly anybody

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70 Robert Rands's Ph.D. dissertation on this subject showed that captive figures were present from the outset of Classic Maya art, but I never see his interesting results even cited in the literature, let alone seriously utilized.

has studied the subject, "there is very little evidence from the archaeological record to support these [i.e., my] . . . assumptions" that swordlike weapons were used in the Pre-Classic. Be a little more patient. Recognize the selectivity of "the archaeological record." Only a fraction of the total record has been, or likely ever will be, dug up.

The advice applies to the question about how early the macuahuitl was in use (p. 294). As long ago as 1938 S. J. Morley published Stela 5 from Uaxactun, which shows a macuahuitl.72 Philip Drucker even reported "the depiction of an obsidian-edged sword" at Olmec La Venta,73 in art dating to Jaredite times. Given these early examples, there was no reason for Matheny to prolong the discussion about the unclear weapon shown in the scene from Loltun Cave, which is much like the one Morley showed. I expect that fuller mastery of the technical literature, like what she has missed on these early macuahuitl examples, will probably relieve her mind about more "problems" which she still sees in relations between the Book of Mormon text and archaeology.

She also suggests that if Nephi's descendants had changed from "metal swords" to a form of weapon like the macuahuitl, this would represent a "fundamental change" that ought to be "reflected in the language" (pp. 296-97). This reasoning is erroneous on two grounds. As I showed above, the text does not tell us that metal swords were used on a wide scale. Note that Ammon, son of the Nephite king, possessed an effective sword (not necessarily metal), while none of his Lamanite opponents or companions (commoners) had such a weapon (see Alma 17:37), even though Lamanites (certain elite only?) are said to have had swords (see, e.g., Alma 60:12, 22, although Alma 49:2 omits any hint of them). Perhaps there was no "fundamental change" because most people lacked metal swords from the beginning. Yet even had there been a change, we do not have the language of the general populace in the record that has come down to us. The records were kept only by the elite lines springing from the houses of Nephi and his brother Jacob. We do not know how

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72 S. J. Morley, "The Inscriptions of the Petén," Carnegie Institution of Washington Publication 437, vol. 5, pl. 60b; tentatively dated A.D. 366, see Hassig, War and Society, 218, n. 27.
the language used in keeping the record related to contemporary speech generally.

Matheny is correct that "no case has been made that metal swords existed in Mesoamerica before the Spanish conquest" (p. 287). Neither I nor anyone else has seriously attempted to do so, yet. This does not mean it might not be possible. I wish Matheny had tried it by delving exhaustively into the recondite sources on Aztec-period warfare that ought to be known to her instead of pointing to another "problem" that may be only an uninvestigated bogey-man. The bow and arrow provides a parallel case. It has commonly been said that this device arrived or developed in central Mexico "late." 74 This is an error based on inadequate examination of the archaeological record, as Paul Tolstoy has shown. He has found "prima facie evidence of the limited use of the bow and arrow in central Mexico since early agricultural times." 75

Rather than deal with particular points Matheny raised about tents (pp. 297-300), I will proceed directly to the results of a bit of research I completed in little more than a day by poking about in the Mesoamericanist literature (benefitting from suggestions by John E. Clark). The results respond to Matheny's central challenge: "Archaeological, ethnographic, and linguistic records from Mesoamerica provide no evidence of a tent-making or tent-using tradition and, even more problematic, suggest no available material for making tents" (p. 299). I found on the contrary that tents were in regular use by Aztec armies, and when the Spaniards saw them, they immediately labelled them tiendas, "tents." This fact is easily documented as well as logical, the cultural pattern was widespread in Mesoamerica, and it seems to me that Matheny ought to have known this because of her training.

She could have begun in Hassig's Aztec Warfare: Imperial Expansion and Political Control. 76 While she listed it in her bibliography, she did not study it carefully enough. Perhaps she

74 So Hassig, War and Society, 137-38.
only scanned the index, which fails to list “tents.” Yet Hassig notes, “The [Aztec military] camp itself was constructed of tents and huts (xahcalli) made of woven grass mats. These mats were usually carried as baggage from the home cities, but some tribute labor gathered en route was also allocated to carry them to the battlefield and set up the camp.”

I did not see Hassig’s statement (his book was published after my An Ancient American Setting) until after I had turned directly to Durán, an obvious fundamental source on Aztec war customs. Durán arrived in New Spain in 1542, only twenty-one years after the Conquest. He saw for himself a way of life changed only in part since Cortez arrived. He lived amidst Indians who acted as detailed informants, he had access to and utilized many native manuscripts, and he read reams of Spanish reports of visits and administration. From these he synthesized a history of the Aztecs colored with fascinating ethnography; it was completed in 1581.

Motecuzoma (popularly known as Montezuma in English) and his spokesman told the Mexican army while they were en route to Chalco, “on this plain [where they were stopped] are many straw houses and huts (‘casas pajizas y chozas’) where we are staying until this business is finished.”

The combined armies of the Mexicans prepared for an expedition against the city of Tepeaca by getting their encampment set up, “pitching their tents and huts (‘armando sus tiendas y jacales’)—that is what they call their war tents—very nicely ordered and arranged, placing the squadron or unit of the Mexica by themselves, [that of] the Tezcocans by themselves, the Chalcas by themselves, the Xochimilcas by themselves, and the Tepanecs by themselves.”

Preparation for a campaign involved ordering barrio leaders in the capital city to furnish supplies, including “many tents and

77 Ibid., 73. His citation is to Fr. Diego Durán’s massive Historia de las Indias de Nueva España e Islas de la Tierra Firme (Tomo II, Biblioteca Porrúa 37, ed. Angel Ma. Garibay K. México: Editorial Porrúa, 1967).
79 Durán, Historia de las Indias de Nueva España, 2:147 (cap. XVII, para. 15).
80 Ibid., 2:157 (cap. XVIII, para. 27).
huts (‘tiendas y jacales’) for the war, with much other apparatus and munitions of war.”  

“...And when morning came, they left there, and they did the same thing in whatever place they reached. And one day’s journey before they arrived (at their destination), they sent ahead those charged with logistics to the place where they were going to set up the camp, and they pitched the tents (‘tiendas’) and erected the huts (‘chozas’) and quarters (‘cuarteles’) for all the lords of the provinces, so that when they arrived they had nothing more to do than each one to go to his place that the advance party had got ready, and they did the same thing along the road when night was coming on.”

In preparation for war, Montezuma ordered surrounding cities to furnish stores of food and “sleeping mats (‘petates’) to make tents (‘tiendas’) and houses (‘casas’) of those mats (‘esteras’) in which they would dwell [while] in the field.”

When they didn’t stay in the towns, they pitched their tents and shelters made with mats (‘tiendas y casas de petates’) in spots arranged by the advance party.

In An Ancient American Setting, I had cited Bernal Diaz as mentioning that the Aztec soldiers “erected their huts” in the field.

At least five types of field military shelters are distinguished here, and several of them were labelled “tiendas,” tents, by the Spaniards:

1. “casas pajizas,” houses of straw;
2. “chozas,” huts, sometimes of unspecified material but suitable for leaders to occupy;
3. “jacales” (from Nahuatl xahcalli) huts; the material utilized is not clear, for at least some were collapsible and movable; some leaders occupied these; mats were probably the usual material. It is unclear how these differed from “chozas;” perhaps the latter were made from materials such as brush scrounged in the field;
4. “tiendas,” tents; of unspecified material but perhaps of (ixtle or henequen?) cloth, given the normal Spanish sense of “tiendas”; some were good enough to house leaders;

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81 Ibid., 2:179 (cap. XXI, para. 15).
82 Ibid., 2:180 (cap. XXI, para. 18).
83 Ibid., 2:156 (cap. XVIII, para. 21).
84 Also ibid., 2:180 (cap. XXI, para. 19).
85 Sorenson, An Ancient American Setting, 161.
5. "casas de petates," houses of mats; the cheap, light, readily portable mats could be combined with, say, spears, to make a simple "tent" for ordinary soldiers, or anybody in an emergency;

6. "cuarteles," quarters, barracks; these may refer to commandeered housing in communities along the road, or they might have been collapsible multi-person shelters.

Only the variety of military housing should surprise us. After all, every army in the world has had to find culturally and ecologically effective ways to cope with the problem of shelter in the field. As long as there are armies, there must be cross-cultural equivalents of "tents." The only questions in relation to a specific culture have to do with form, materials, and names.

It is to Matheny's credit that she (p. 300) detected a reference to "tiendas" in Tezozomoc (a contemporary of Durán; she might easier have followed up my reference to Bernal Diaz). The Durán material should, however, have been obvious given that she had studied with Prof. Dibble at Utah, an expert on this set of materials.

She raises another difficulty about tents. After all, she says, the tents mentioned in Tezozomoc "were found in central Mexico rather than in the area of the Limited Tehuantepec model," farther south. The answer is obvious. The Aztecs fought or had garrisons in many parts of Mesoamerica, including Chiapas. No groups who interacted with them could have failed to know about their tents. Furthermore, if the Aztecs, who were great cultural copycats, were smart enough to figure out field shelter for their soldiers, were other Mesoamericans so benighted that they had never solved the same problem over millennia of warfare? Hassig's answer is self-evidently correct: "Given Mesoamerican technology, any material innovation in warfare could diffuse rapidly and came within the grasp of every group."

As an added witness look in the Motul dictionary. This is, of course, a classic sixteenth-century work that scholars automatically turn to for supplementary light on pre-Spanish Yucatec Maya language and culture. The definition for the Maya word pazel is "choza o tienda en el campo, o casilla pequeña de paja"
Mesoamerican farmers have long and widely used a similar type of hut. For example, the Zoques of Santa Maria Chimalapa in the Isthmus of Tehuantepec still construct "very small chozas of palm fronds and grass, almost level with the ground, where they sleep during the days when they work in the fields" away from home.89

Matheny conjures up still another problem, though—the Aztec "tiendas" of Tezozomoc "were known [only] at the time of the conquest, about one thousand years after the end of the Nephite civilization" (p. 300). The only evidence we have of their presence even for the time of Tezozomoc and Durán is in historical documents; it is not archaeological. What archaeological evidence would one expect that would establish the presence of overnight "tiendas," "chozas," or "jacales," even among the Aztecs less than five centuries ago? Then what hope has an archaeologist of finding the still slimmer traces of a temporary encampment dated two thousand years before that? I have no idea how these tents would show up in an archaeological dig; I suspect they would be completely undetectable. Until archaeologists come up with an operational solution to this dilemma, it seems sensible to me to accept the Book of Mormon as documentary evidence of tents in the first century B.C. on a par with Durán's or Tezozomoc's testimonies for the sixteenth century A.D.

Matheny says, "It seems unlikely that such a practical tradition as tent-making would die out in Mesoamerica" (p. 299). While, as we have just seen, that did not take place in the case of tents, the extinction of many former cultural patterns that appear "practical" retrospectively to moderns is a well-known phenomenon.90

Plants and Animals

Regarding plants, Matheny again needs to read the Book of Mormon carefully (pp. 300-301). Olive trees are mistakenly said to have been cultivated by the Nephites; not so the text.

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88 Ibid., 732.
89 Muñoz, Crónica de Santa Maria Chimalapa, 14.
90 A brief introduction can be found under "Lost Arts," in John W. Welch, ed., Reexploring the Book of Mormon (Salt Lake City: Deseret Book and F.A.R.M.S., 1992), 101-4.
Continuing, she mentions three “products” which “imply the existence of specific plants, including ‘fine linen,’ vineyards, and wine presses.” All those terms imply is the existence of cultural products which the author supposes to involve “specific plants,” namely flax and grapes. But, as I have pointed out, perhaps to the point of tiresomeness, the Spaniards did not make the same assumptions as Matheny. They encountered and referred to what they considered “linen” or linenlike cloth made from plants other than flax. They also spoke of “vineyards,” not planted in grapevines but in maguey plants, from which pulque, which they termed “wine,” was manufactured. Half a dozen different types of “wine” made from fruits other than grapes were identified by the Spanish explorers.

The English term wine is itself unclear. A standard anthropological source uses the terms beer and wine without clear distinction, and the author, LaBarre, supposes that none is needed, for the difference is not consistent in English. Nevertheless, grapes were known and used in ancient America. LaBarre reports the Opata of northern Mexico used a drink made from native grapes. Terrence Kaufman lists a word for “wild grape” in the Proto-Mayan language, which he calculates began to break up into daughter languages in highland Guatemala before 2000 B.C.

By the way, of interest as a functional parallel (i.e., an analogy) to the Lamanite and Nephite use of “wine” to prepare themselves for combat (see Alma 55:8–32) is a “wine” made and consumed by the Maricopa Indians, according to LaBarre; blood

91 See, for example, Sorenson, An Ancient American Setting, 186–87, and John L. Sorenson, “Possible ‘Silk’ and ‘Linen’ in the Book of Mormon,” in Welch, ed., Reexploring the Book of Mormon, 162–64.


93 Ibid., 232.

red, it was made of cactus fruit and consumed at a certain celebration—"When they were drunk, they thought of war."95

Without explaining her basis, Matheny assumes that "Old World plants" would have been grown among the Nephites (p. 302). This view could come from only two points in the text: (1) 1 Nephi 18:24 mentions that upon arriving in the promised land, Lehi’s party planted the seeds they had brought from Palestine or Arabia, and these flourished; and (2) mention of Old World names for two grains, "wheat" (Mosiah 9:9) and "barley" (Mosiah 7:22; 9:9; Alma 11:7, 15). The two phenomena are not, however, connected by the text.

Historical cases of plant transfers do not give us confidence that imported seeds would prove viable in a new environment in the long run. In An Ancient American Setting, I documented how millet, introduced by the Spaniards in Yucatan and said in the sixteenth century to grow "marvelously well," could not be located at all in the Carnegie Institution’s botanical inventory of the area early this century.96 The same might have been the case with the seeds brought with Lehi’s party and planted (but, realize, only after at least nine years of being hoarded through the Arabian desert; they may or may not have been healthy by then, and the new moist tropical environment would hardly welcome desertic Near Eastern grains). Yet realize that nothing is said in the text about the species those seeds represented; perhaps they were rye, emmer, and dates. We have no warrant to assume, in the absence of textual reference, that they included the plants later called by the Nephites “barley” or “wheat.”

Many historical cases assure us that plant names can change under new circumstances. When new plants are encountered, old names commonly are applied to them. For instance, after the Conquest, many Spanish names were applied to plants found in Mexico because of their similarities to those of Europe, such as “ciruelo,” plum (tree), applied to the nonplum genus Spondias.97 Various other naming puzzles also occurred. The fruit of the prickly-"pear" cactus was called by the Spaniards "fig," even though a real native fig was present (as Matheny noted, p. 302). Some Spaniards used the word “trigo,” wheat,  

96 Sorenson, An Ancient American Setting, 139.
for maize (French peasants in recent times still called it “Turkish wheat” or “Roman wheat”).

Within the Book of Mormon itself we discover an interesting case of a plant name changing. Mosiah 9:9 mentions “sheum” in a list of plants. The name rather obviously derives from Akkadian (Babylonian) “she’um,” barley (Old Assyrian, wheat), “the most popular ancient Mesopotamian cereal name.”99 A Jaredite source is logical, for that group departed from Mesopotamia, although the Book of Mormon reference is to a plant cultivated by the Zeniffites (a Nephite—“Mulekite” group) in the second century B.C. The term could not have meant “barley” or “wheat” among the Nephites because “sheum” is listed along with “barley,” while “wheat” is named elsewhere without hint of any connection with “sheum.” (Incidentally, careful reading of Mosiah 9:9 indicates that while “corn,” “barley,” and “wheat” were classified as “seeds,” “neas” and “sheum” may be implied to be other than seeds.) Whatever crop was called “sheum,” it is unlikely to have meant to the Zeniffites what it once had in Mesopotamia, barley or wheat, but had come to be applied (by the Jaredites?) to something else.

Plenty of other cultivated grains in ancient Mesoamerica might have been called sheum, or “wheat,” or “barley.” Some possibilities are:

1. amaranth (*Amaranthus leucocarpus* and *A. cruentus*);100
2. huauzontle;101

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3. chia (Salvia hispanica or S. chian, used in greater quantity by the Aztecs than even amaranth);102
4. Setaria or fox-tail millet (S. geniculata Beauvais);103
5. 40-chromosome “perennial corn” (Zea perennis, a form of teosinte);
6. 20-chromosome “perennial corn” (Zea diploperennis, also a teosinte); and
7. Chalco teosinte (probably the food plant mentioned in Codex Vaticanus 3738 as “accentli”).104 These materials are cited to make the point that the archaeological inventory of Mesoamerican grains still remains to be completed, as well as to point to the problem of naming.

Matheny cites an archaeological study by Martínez M. who recovered plant remains in Chiapas. The limited inventory discovered in that study is supposed to pose a problem for the Book of Mormon, whose peoples I believe inhabited that area. However, when Martínez’s short list of remains is compared with the extensive inventory of plants already known to have been in use in Mesoamerica as a whole,105 it is apparent that a sampling problem exists. Archaeologists in particular regions, let alone at single sites, are not going to discover the full range of plants used anciently throughout the entire culture area. Martínez’s list is only a small portion of Heiser’s inventory. (I find it amusing that when Matheny wrote out the names of some of Martínez’ plants, she put down “vitis,” apparently unable to bring herself to say “grape!”) Accidents of sampling, preservation and identification all contribute to the problem of straightening out botanical history. For example, C. Earle Smith, Jr., was dismayed to find maize absent at a huge Peruvian site, except for

a single cob (pp. 150–51). And while the pineapple is known to be old in the New World on distributional grounds, the only archaeological record for it consists of seeds and bracts found in coprolites from Tehuacan Valley caves dating between 200 B.C. and A.D. 700.

Anyway, few really good studies of plant remains have been done in Mesoamerica. Heiser spoke of “the often-equivocal archaeological data” on which opinions about the age of plants in a given area have been based, while botanists still disagree widely on the systematics and areas of origin of many cultigens. The difficulty of the problem for archaeologists is shown by the fiasco of the famous Tehuacan Valley maize specimens. Accelerator (AMS) dating (the most sophisticated form of radiocarbon dating) was done in 1989 on a sample of cobs selected by chief excavator Richard MacNeish. He intended them “to represent the oldest maize in the collection and related to well-dated levels.” All the specimens were selected from his Coxcatlán phase (“5000 to 3400 B.C.”) except one cob from the succeeding Abejas phase. Instead, the actual, calibrated AMS dates of the “earliest” cobs stretched from the calibrated range 3860–3380 B.C. at the early end through 2540–2150 B.C. Yet two of the cobs proved to be as late as the time of Christ, another fell around A.D. 300–500, and a final specimen dated to A.D. 1500! Clearly, the archaeologists had made some major mistakes somewhere along the way. So botanical and archaeological methods still have a long way to go before they can be relied upon to give us firm data on the age, types, and distribution of ancient American crops. Not without reason did Heiser warn us that “detailed knowledge of the origin [and dating and distribution] of many of the cultivated plants of the Americas is lacking.”

108 Heiser, “Cultivated Plants and Cultural Diffusion,” 935–44.
111 Heiser, “Cultivated Plants and Cultural Diffusion,” 945.
It is marginally helpful for Matheny to remind us how far facile statements by some Latter-day Saint writers about the crops of the Nephites and Lamanites depart from what the botanists think they know. I too hope for improvement and caution in reading and interpreting both the scripture and the scientific record. But now consider the case of the discovery of New World barley, which Matheny construes as unimportant or negative in relation to the Book of Mormon account. What it actually teaches us is that changes in the scientific botanical inventory for ancient America must still be anticipated. Details of the case are as follows: I reported in 1984 on the discovery in Arizona—the first in the New World—of archaeological specimens of possible domesticated barley, and suggested that this could prove of interest in relation to Alma 11:7 and 15.112 Abundant samples of the same grain had also been discovered at sites and in collections from Illinois and Oklahoma. These led V. L. Bohrer to state cautiously, "it is reasonable to conclude that we are looking at a North American domesticated grain crop whose existence has not been suspected."113 But Nancy and David Asch were less cautious: "[Our] project reveal[s] a previously unidentified seed type now identified as little barley (Hordeum pusillum), and there are strong indications that this grain must be added to the list of starchy-seeded plants that were cultivated in the region by 2000 years ago."114 They added, "This barley is well-represented also at two other sites, one Late Woodland (A.D. 600–1050) and the other Middle Woodland" [early A.D. centuries].115 So here was a domesticated barley in use in several parts of North America over a long period of time. Crop exchanges between North America and Mesoamerica have been documented by archaeology making it possible that this

115 Ibid., 81.
native barley was known in that tropical southland and conceivably was even cultivated there. The key point is that these unexpected results from botany are recent. More discoveries will surely be made as research continues.

Meanwhile it is a red herring for Matheny to hedge that *H. pusillum* was, after all, not "an Old World import" but a native American plant. As I have pointed out above, the Book of Mormon says nothing about where its "barley" crop originated. It is not out of the question that *Hordeum pusillum* was Nephite "barley," even though it is not likely. Surely the discovery is not without relevance for the problem of identifying the Nephites' crops.

Matheny also states, "thus far no Old World plants have been identified by the presence of their pollens or other remains" (p. 302). This is a puzzling statement. She has told me that she has used the two-volume *Pre-Columbian Contact with the Americas across the Oceans: An Annotated Bibliography* in connection with her investigation of possible Semitic inscriptions in South America. By looking up "plant" or "crop" in the index, she would also discover a vast literature that would directly contradict her statement about "no Old World plants." A substantial number of Old World pre-Columbian crops have been identified in America. This is fact, even though diehard isolationist archaeologists and botanists (the B.S.) are uncomfortable with the point. Yet regardless of the fact that certain crop plants did obviously cross the oceans, we cannot confidently state whether any of those cultigens were, or were not, brought or used by Lehi's group. So it would make no direct difference to the question of the accuracy of the Book of Mormon either way, but certainly somebody brought some plants across, thus making it plausible that Lehi's group *could* have done so.

Noting various animals known from Mesoamerica, which I had suggested as possibly utilized by the Nephites and Lamanites, Matheny thinks that "many of these animals may have been considered unclean for consumption by Nephites" under "the Law of Moses" (pp. 302–4). She admits that we do not know from the text whether the Nephites knew of or kept "the dietary laws" of that code, yet she assumes that they did.

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117 See, for example, Carter 1974 (C-092), abstracted in ibid., vol. 1.
This reveals an uncritical view of the origin and development of those rules; it implies that the code that appears in today’s (King James?) version of the Old Testament existed at the time of Lehi’s departure from Jerusalem. We do not know that. Some of the devotees of “critical method” who contributed to the volume in which Matheny’s paper appeared would consider this naive. There are major scholarly disputes about what rules, if any, were known and applied in the land of Israel by the time of the diaspora, but many—maybe most—current scholars would question Matheny’s position. They consider it likely that some or all those specific restrictions on food were developed and codified by the Jews after the date for Lehi’s departure. It seems intuitively likely that some restrictions were in force by then, but not particularly the set listed in present scripture. Until more is known on the matter, there is no point speculating whether or not the Nephites had this or that particular ritual limitation on animal use.

In any case, we know that only some of the Nephites kept their version of the “law of Moses” some of the time. Otherwise there would have been no point in the text’s emphasis on how hard it was for the priests to hold the people to whatever the code was (Jarom 1:11–2). (Will no liquor bottles be found in the ruins of Mormon communities by future archaeologists!?) The same qualification would be true of other aspects of the “law of Moses.” For instance, performing sacrifices is not mentioned in the bulk of the Book of Mormon record, between Mosiah 2:3 and 3 Nephi 9:19. The latter verse tells us that some sacrifices were being practiced, but we are not told of what they consisted. It is unlikely that they approached the cultural centrality of the temple sacrifices in Israel during the same period, or more would have been said of them. We simply don’t know what was in the Nephite version of the “law of Moses,” hence Matheny’s objection about nonkosher animals is moot.

Matheny’s comments on animal names are not apt (p. 304). All kinds of complications have occurred in historical cases of animal nomenclature, the same as for “wine” or “barley.” Her generalizations will not work because they are not empirically based. She needs to look carefully at the extensive literature on

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118 Dr. Gordon Thomasson has pointed out to me in a personal communication that even the restrictions imposed in later Judaism had so many exceptions authorized in rabbinical reasoning that nearly any animal could be eaten under certain circumstances.
animal terminology in a variety of cultures. Some of this material is accessible by looking under “naming ambiguities” in the index in my Animals in the Book of Mormon: An Annotated Bibliography. My hope in putting out that piece was to increase the sophistication of discussions of the Nephite and Jaredite animals referred to in the Book of Mormon. After she studies it, her comments would be more to the point.

The fact that scientists generally doubt the presence of any animals other than those they have “authoritatively” agreed upon so far does not mean that they will not change their minds in the future (p. 305). A classic case involves the “chicken.” George F. Carter, emeritus professor of geography at Texas A & M University, is completing the editing of a volume of papers (assisted by a F.A.R.M.S. grant) to be published by TAMU Press that covers evidence for the New World occurrence of this fowl before the time of Columbus. He and others have published on the topic previously. He has assembled a wide range of evidence—from zoology, archaeology, history, linguistics and ethnography—that has been long ignored or resisted by conventional scientists, which demonstrates that at least one race, and probably more than one, of the Old World domestic chicken was present and used in the New World (mainly for sacrifice) before the Spaniards brought their birds from across the Atlantic. Actual chicken bones have been found over the last fifty years at several sites in the western United States without their being acknowledged in the formal literature. The bones exist and they were dug up by legitimate archaeologists, but they have been tucked away undiscussed—some for many years—because “everybody knows there were no chickens before the Spaniards arrived.” Carter’s volume will demand these be properly reconsidered. Yet this is only a little more scandalous than the neglect given the possibility that real horse bones have been found in Mesoamerica dating to the time of the great civilizations.

Matheny’s treatment of the horse illustrates, again, how carefully one must read the scriptural text before attempting to

121 For more information, see “horse” in the index to Sorenson, “Animals in the Book of Mormon.”
compare it with outside information. She assumes that the “Jaredites and Nephites . . . were well-acquainted with horses” in the Old World, hence they would not “have mistaken a deer or a tapir for a horse” (pp. 307–8). But we do not know whether or not the Jaredite party were “well-acquainted with horses.” The text says nothing about the subject in relation to their land of origin. No one knows from exactly what part of the Near East they began their journey to America. In general we suppose it was Mesopotamia, but even if that should be correct, were horses common, rare, or unknown there, or were they domesticated at all at ca. 3000 B.C.? Whatever the case for their homeland, the Jaredite party’s trip across Eurasia and the ocean consumed years, after which few if any of the pioneering generation in the new land may have survived long enough to tap their memories regarding animals in their original land as they encountered fauna in the New World. (The only mention of “horses” in their record, in Ether 9:19, comes generations after the landing.) As we have the Book of Ether through Moroni’s translation, I assume that the term “horse” in Ether 9:19 is from him and refers to the same beast to which the name is applied in Mormon’s record.

Of course Nephi and his cohorts certainly knew horses, yet keep in mind that the Hebrew term for horse, sus, means basically “to leap,” and other (“leaping”) animals, including the swallow, bore related names.122 The fact that deer are also leapers might have justified the early Nephites in applying to them a Hebrew name that had been applied to the horse in Nephi’s Jerusalem. (Compare Egyptian ss, “horse,” and shs, antelope; note also, in the Mixtecan language of Mexico, yi-su, “deer.”)123 But nowhere in the scriptural text do we get a definite answer to the question of how the Jaredite/Nephite “horse” relates to the animal kingdom as we know it. There are other thought-provoking examples of possible ambiguity in Nephi’s Hebrew nomenclature which Joseph Smith’s English translation of, say, 1 Nephi 18:25 may not adequately reflect: the word for ox, in Hebrew aluph, was from a root meaning “tame” or “gentle,” which could also be applied to a friend. (Could it apply to a tapir?) Another Hebrew word was teo, “wild ox,” but it also applied to a species of gazelle.124 One of nine Hebrew words

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122 Ibid., 33.
123 Ibid.
124 Ibid.
for sheep, zemer, is translated in different versions of the Bible as both “mountain sheep” and “rock-goat,” while one Jewish scholar believes it to mean an antelope.125 And if someone balks at the idea that Joseph Smith may not have translated every term “correctly,” consider the enigmatic statement in Enos 1:21, the Nephites “did raise ... flocks of herds.” As I noted, this is quite surely a Hebraism, for Hebrew baqar translates as “ox,” or “cattle,” or “herd.”126 I suppose that Joseph was “right,” although in English the translation is more than puzzling.

It is not just the Book of Mormon text that is obscure, however. The Spaniards were very unclear about some of their encounters with newly discovered American animals. They left behind in their historical records a mishmash of names for animals which we know today by other labels.127 Were they “mistaken,” as Matheny thinks the Jaredites would have been, when the Europeans called bison “cows,” the turkey a “peacock,” pronghorn antelope “animals like flocks of sheep,” or the tapir “a species of buffalo of the size and somewhat looking like an ass?” If the Spaniards made ad hoc, puzzling naming decisions when they discovered and labelled New World animals, I grant the same option to the people of Lehi. We reveal our ethnocentrism if we demand nice natural-science logic on their part when we see the strange names applied by the Europeans.128 Those like Matheny who question my interpretations for Book of Mormon animal names at least ought to become informed on the topic by mastering the literature on documented cases of terminological ambiguity. I’ve shown where to begin, not how to conclude.

My critic goes on to doubt that deer were ridden in Mesoamerica—an interesting possibility that I suggested. She turns to a selection of representations of human-animal pairs, all from the Maya lowlands, outside the Book of Mormon area I recognize. She cites guesses by archaeologists about what those scenes might or might not mean. Her result is that the question of whether deer were ridden is left up in the air. But she ignores ethnohistoric information laid out by Professor Dibble in the

125 Ibid., 36.
126 Ibid., 42.
127 See, among others, the article by W. George abstracted in ibid., 12.
128 Cf. Stocker et al. in Sorenson, Animals in the Book of Mormon, 34.
department where she graduated, which tells us about the Aztecs' encounter with Spanish horses. They spoke of "the deer-which-carried-men-upon-their-backs, called horses."\textsuperscript{129} Such information shows that there is nothing inherently implausible in the idea. (In Siberia deer have been ridden for centuries.)

But if one is going to try to make sense of Nephite or Jaredite animal use, the need—once more—is to read the Book of Mormon text meticulously. So I hasten to note that the Book of Mormon says nothing to suggest that deer, or any other animals, were ever ridden. The only reason I raised the matter in An Ancient American Setting was to show that the role of animals in Mesoamerican cultures was probably more varied and extensive than routine scholars have supposed.\textsuperscript{130} Two references in Mosiah suggest that "burdens" were placed on an animal called an "ass."\textsuperscript{131} But all verbs and adjectives in the Book of Mormon text relating to animal use need careful study. Neither "domesticated" nor an equivalent term occurs, for example. The Jaredites are said to have "had" certain animals,\textsuperscript{132} and the Nephites "did raise" flocks, according to Enos 1:21.\textsuperscript{133} "Horses and chariots" were used to "conduct" (what an enigmatic verb!) a party from place to place within the general land of Nephi (Alma 18:9–12). Then 3 Nephi 4:4 lumps "horses" with "provisions" and "cattle, and flocks of every kind"—as food supply—which the Nephites accumulated "that they might subsist." Clearly, we need to get on with the basic textual study on this topic. To that end I included in "Animals in the Book of

\textsuperscript{129} A. J. O. Anderson and C. E. Dibble, trans., The War of Conquest: How It Was Waged Here in Mexico (Salt Lake City: University of Utah Press, 1978), 35; cf. other sources indexed in Sorenson, Animals in the Book of Mormon under "riding animals."
\textsuperscript{130} Stimulating papers by Dillon and Puleston, abstracted in Sorenson, Animals in the Book of Mormon, are to the same point.
\textsuperscript{131} See Snarskis in Sorenson, Animals in the Book of Mormon, and the Termer reference in Sorenson, An Ancient American Setting, 394, regarding animals in Central America which are pictured laden with burdens; at least the presence of the concept of burden-carrying animals is thus demonstrated.
\textsuperscript{132} Implications are discussed in Sorenson, Animals in the Book of Mormon, 41.
Mormon" an exhaustive appendix, "Animal References in the Book of Mormon." I wish Matheny had done some of that spadework instead of just giving opinions.

The note about biological characteristics of American populations in relation to the Book of Mormon (p. 310) shows overconfident reliance on "mainstream" physical anthropology. Matheny could well engage in broader study of the subject, going beyond the selective "top 40" lists of acceptable literature favored by standard American physical anthropologists. Of particular value would be reading in the history of this sub-discipline, starting perhaps with Juan Comas.134 He makes it apparent that U.S. "mainstream biological anthropology" is paradigm-(and clique-) limited so as to include certain researchers, like the trendy, much-published Christy Turner, but to exclude arbitrarily an Andrzej Wiercinski (and, with a condescending smile, most other physical anthropologists outside the USA).135

Incidentally, while it is true that "most features of cranial morphology are considered (to be very responsive to environmental change" by physical anthropologists today, that has not been demonstrated but largely assumed.

Zarahemla

It is greatly overdoing it to say that "Sorenson has examined what is known archaeologically about each of the areas within the scope of his model" (pp. 310–11). As I repeatedly said in An Ancient American Setting, the archaeological information referred to there is highly summarized and painfully simplified. For either of us to talk about "what is known archaeologically" about the sites or areas listed on page 311 would be impossible because of the scale of the endeavor. There is already too much relevant information in print to summarize, although of course we would like much more. Anyway, with one or two exceptions ("probable"), the sites on that list I had labelled only plausible candidates for Book of Mormon sites. Had I supposed that they were definitely "the" Book of Mormon sites, I would have begun the second stage in a full research program on the Book

134 Juan Comas, Antropología de los Pueblos Iberoamericanos (Barcelona: Editorial Labor, 1974).
135 For a wide range of non-"mainstream" writers, check under "race" and "migration" in the index in vol. 2 of Sorenson and Raish, Pre-Columbian Contact.
of Mormon which John Clark has referred to, that is, making rigorous comparison of each site with all relevant Book of Mormon passages.136

When the time comes that a systematic comparison can be attempted between sites/regions and textual cities/lands, however, it ought to be done on a far more critical and comprehensive basis than Matheny has been able to show us in her example, Santa Rosa, Chiapas. On page 313 she states that the excavations done at Santa Rosa by the New World Archaeological Foundation in 1956 and 1958 were “adequate” to give us a picture of what is at the site. Actually, they were embarrassingly inadequate. The two publications issued can hardly be read in parallel because of philosophical, methodological and data differences between the archaeologists who wrote them. The limited aim of the work was to establish a ceramic sequence and to determine the scale and dates of inhabitation at the site. Yet the published data do not yield more than a basic sequence. The ceramic analysis behind the “phases” offered by Brockington is confusing and questionable in its details. Meanwhile Delgado’s report is minimally useful in sketching the history of the community. The extent of the site was never clearly established for any phase. Overall, the reports on Santa Rosa are an example of a very limited type of archaeology, which was all that could be expected when this project was undertaken. We can be glad for the information the NWAF obtained at Santa Rosa, but it is not at all “adequate” to answer most of the questions which interest Matheny or me. Consequently, her sketch of the history of the place offered on page 313 is only a first cut. For example, she fails to mention that crucial phase 3 seems to be divided by a surge in building activity at around 100 B.C., which could relate to what I called “the expansion of Zarahemla.”137 But the information furnished in the reports is too thin to do more than hint at such nuances which might relate to the Book of Mormon story.

By the way, the fact that the bi-lobed residential pattern at the site goes back before the date for the arrival of Mosiah’s people in no way robs it of significance for Mosiah’s day; we could easily suppose that the two ethnic groups—Nephites and people of Zarahemla—would be fitted into the preexisting settlement pattern upon Mosiah’s arrival. But the scriptural text in Mosiah 2

136 Parts 4, 5, and 6 in my A Source Book begin the task as far as the text is concerned.
137 Sorenson, An Ancient American Setting, 190.
describes only the separated seating pattern of the two groups on one ceremonial occasion, Benjamin's final speech. It says nothing definite of bilateral sitting of houses. My comment about the twofold division remains worth thinking about even though I made no specific claim about residential areas. Finally, nothing learned about Santa Rosa's history that I am aware of is in conflict with the picture of Zarahemla I drew from the scriptures, although overall the information is limited.

Matheny focuses attention next on the fortification wall constructed around the city of Zarahemla in the first century B.C. (pp. 315-16). Where is any archaeological trace of it, she asks, if Santa Rosa is Zarahemla? "Discernable walls and fortifications dating to the Late Preclassic period have been found at a number of sites in the Maya area," so why not at Santa Rosa? Her comparison to the Maya area is apt in a way she did not anticipate. Generations of archaeologists worked in the latter area without finding those walls she mentions. What led to their recognition is interesting. For years a few scholars had cited evidence for Classic period warfare in the Maya area but were resisted by mainstream archaeologists until a dramatic discovery in the field made the old picture of peaceful theocrats impossible to maintain. The result opened up room in the paradigm to accept fortification walls when their remains were found—even dating to the Late Pre-Classic, the core Book of Mormon period. But the change in thinking was not easy.

The crucial discovery came during investigation of Tikal, the great Maya center. What at first appeared to be merely a hillock and adjoining arroyo several miles from the site turned out to be weathered remnants of an earthen fortification wall and parallel ditch that stretched for miles. The find was accidental; the little elevation was too slight to show up on an aerial photograph even had anybody had the (then) strange notion of looking for a wall when "everyone knew" that the Maya did not engage in wars. Once the cat was out of the bag, searching began to reveal more walls in the Maya lowlands and elsewhere. But Delgado and Brockington did their work at Santa Rosa too early to benefit from this new perspective on war, nor did they share my idea that the site might be Zarahemla. (They are not Mormons.) They did not have any expectation that there might be a wall, so they

138 Ibid., 156.
did not look for one. Given the prevailing attitude of Mesoamericanists in the 1950s it is farfetched to think, with Matheny, that at Santa Rosa the archaeologists would automatically have detected remains of whatever wall was there—even had they once sat on the eroded pile to eat lunch.

A look at Zarahemla’s wall according to the text is instructive. It must have been generically of the form described in the book of Alma. I did not “postulate” any form of wall beyond what is stated in Alma 50:1–6. “Heaps of earth” were said to have been piled up by the Nephites to form walls, “round about every city in the land.” Additional detail is given at Alma 53:4. (There could, of course, have been unmentioned regional differences based on availability of materials.) Excavated soil was thrown against a wall of upright timbers, yielding a cross-section, from outside to inside, of ditch, sloping face (glacis), and vertical inner face. The timber was to “the height of a man,” say six feet high, or perhaps a bit more on the basis of Alma 62:21–22, which speaks of needing cords and ladders to let down arms-laden men silently to the inside from atop the wall. Such construction allowed Samuel the Lamanite to ascend/descend the wall up the sloping outer face while his pursuers were hindered, if not prevented, from leaving the city except via a “pass”/gateway. The general picture sketched by the text is consistent and logical, and the construction methods seem obvious, yet we are nowhere told how long the wall at Zarahemla stretched nor how far it lay from the center of the city.

What remains would be left if archaeologists should locate it today? After the Amalickiah/Moroni wars no mention is made of renewed need for walls, although of course it is possible that they kept them up. (The failure of Zarahemla’s wall to protect against Coriantumr’s lightning attack, Helaman 1:21–22, could have persuaded the Nephites that some of the walls were no longer worth the cost to maintain.) As soon as the timbers rotted without being replaced, the earth piled against them would have slumped inward. Erosion would subsequently spread the earth both into the ditch and over the inner surface until the remaining earthen bank would be only on the order of three feet high. Now note that, concerning the much larger wall at Becán in the center

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of the Yucatan peninsula, built centuries later, David Webster reports, "Because the parapet has suffered so much erosion and purposeful destruction it is quite low, varying between 1 and 3.6 m. in height. It is now the least visually striking feature of the defensive system, especially where overgrown. . . . In fact Ruppert and Denison [the original discoverers, in 1933] failed to identify the parapet as a formal construction at all, noting merely that 'A series of low mounds (not shown on the plan), 1–3 meters high, lines the inner bank of the moat.' 141 When excavated the Becán moat had filled up with an average 2.8 m. of sediment (p. 20). If the same phenomena were manifested at Santa Rosa, where the wall and ditch were much smaller to start with, the remains would never be noted nowadays unless someone specifically, carefully searched for them, and the NWAF people did not.

I previously referred to the "potentially ephemeral nature of walls,"142 citing as an example the huge stone wall built by the Spaniards in colonial days in the Valley of Mexico. They utilized over two million people in the project. But despite its historical recency and huge scale, no surviving traces of the structure have been noted by archaeologists or historians. The same is true of the six-mile long wall which the Tlaxcalans had built between them and their Aztec enemies and which Cortez described. It was huge, nine feet high and 20 feet thick with a breastwork atop it.143 Yet no archaeologist has discovered any remnant of it, as far as I am aware. So while Matheny may find a problem in the lack, at this time, of evidence for a wall at Santa Rosa which would qualify it as Zarahemla, to me it seems like just one more difficulty for the archaeologists, not for the Book of Mormon.

The same goes for the question of "evidences of fire" at Santa Rosa-if-Zarahemla. How do we know what archaeological evidence to expect that might show the reported burning of the city? Was the conflagration chiefly a matter of thatched roofs blazing rather than wholesale destruction of buildings? Third

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141 David Webster, "Defensive Earthworks at Becan, Campeche, Mexico: Implications for Maya Warfare," Tulane University Middle American Research Institute Publication 41, 1976, 14.
143 H. H. Bancroft, Native Races of the Pacific States, vol. 2. (London: Longmans, Green, 1875), 416.
Nephi 8:8 merely says, "the city of Zarahemla did take fire." We have no way to tell the extent of the destruction. Of the besieged city of Jerusalem, Jeremiah (21:10) prophesied that the king of Babylon "shall burn it with fire" (Revised English Bible: "burn it to the ground"), yet archaeological work has not yielded evidence that a massive destruction by fire took place. Archaeologists have trouble picking up on what evidence of burning there is.\textsuperscript{144}

A final point Matheny makes about Santa Rosa is that it is smaller than the site of Chiapa de Corzo, some distance away. She thinks this relative smallness does not comport with Zarahemla's having been the ruling city over a land of Zarahemla which included all central Chiapas, as I proposed. In the first place the archaeological work done at both locations was incomplete and inconclusive. We do not know the extent of habitation at either site for the specific times referred to in the Book of Mormon. (As has been evident for some time, and as John E. Clark, director of the New World Archaeological Foundation, confirms, even the much-cited ceramic sequence at Chiapa de Corzo is questionable at points and needs substantial revision. The extent of that site at any given time period cannot be established until the sequence has been clarified. The same is true for Santa Rosa.) But if we indeed suppose, as seems likely on several grounds, that Santa Rosa was smaller than Chiapa de Corzo, a reasonable interpretation of Zarahemla's political relations with its (or any) larger neighbor can be offered based on that situation. The Chiapa de Corzo area (suggested by me as "Sidom") could be seen as a spawning ground of "dissenters" and rebels, such as the Amlicites, against the Nephite rulers at Zarahemla precisely \textit{because} the "dissenters" were from a larger and richer zone than Santa Rosa/"Zarahemla." As such, the Sidomites could have thought that they should be cut a much larger piece of the political pie.\textsuperscript{145} Chiapa de Corzo being larger is also consistent with the statement in Helaman 1:27 that "the

\textsuperscript{144} On the sketchy, disputed traces of a possible burning of the ceremonial center that marked "the fall of Teotihuacan," see Evelyn C. Rattray, "Evidencia cerámica de la caída del Clásico en Teotihuacan," in J. B. Mountjoy and Donald L. Brockington, eds., \textit{El Auge y la Caída del Clásico en el México Central} (México: Instituto de Investigaciones Antropológicas, U.N.A.M., Serie Antropológica 89, 1987), 84.

\textsuperscript{145} See Sorenson, \textit{An Ancient American Setting}, 195–97, where I discuss this point.
most capital parts of the land” lay down the Sidon River from Zarahemla.146 Anyway, what smaller city is likely to acknowledge in its own annals that some other place is bigger and better; would Sparta have granted that to rival Athens even though it was true?

Overall, I agree with Matheny’s statement, “It is difficult then to find [certain specific] evidence for the correlation between Santa Rosa and Zarahemla” (p. 316). The reason she gives for not discussing Hauck’s correlation applies equally to my proposed site correlations, “Until the information concerning the sites [he involves] is available, it is impossible to evaluate how well they fit the descriptions of Book of Mormon sites” (p. 317). Indeed, I am in favor of getting more information on all the sites concerned, after which what Matheny sees as remaining issues will be nearer resolution, although we will never wrap up everything, I expect. I said this many times in An Ancient American Setting.147 So far, I find as much reason to consider a Santa Rosa-Zarahemla equation plausible as I did before Matheny wrote her critique.

The Jaredites

Her characterization of the Jaredites again misconstrues the text (pp. 317–18). It is not true that, “The Book of Mormon makes clear that this group was at a complex level of sociopolitical organization and that they brought with them [from the Old World] much of their knowledge and skill” (p. 317). As I pointed out recently,148 the initial Jaredite colonizing party consisted of on the order of eighty adults. How could such a small group transfer from their homeland to Mesoamerica “much of their [Old World civilization’s] knowledge and skill?” How would descendants of such a tiny band bring to bear whatever inactive knowledge their ancestors might once have possessed in order, after several pioneering generations, to construct a civilization recognizably like that they had left behind? Let us be realistic. To just what “complex level” does Matheny refer? Exactly what features of “sociopolitical organization,” were involved; and how does a group transfer “sociopolitical organization?” (Did the Pilgrims reconstitute the British parliament in

147 For example, Sorenson, An Ancient American Setting, 355.
Massachusetts?) Where exactly in the Book of Mormon text are indications of “their highly advanced culture” when they arrived? How much could their population realistically have grown before the deaths of Jared and his brother? Having only a small population and a not-very-complex sociopolitical organization does not stop some peoples from calling their ruler “king” (compare, for example, Ether 6:19–22 with 2 Nephi 5:18 and Mosiah 23:6). I challenge Matheny to prepare and publish a detailed analysis in which she musters every specific, relevant element in the text itself, leaving her own assumptions aside as far as possible, so as to clarify her proposition. (She mustn’t forget to stay aware of “emic” and “etic” distinctions in reading the text.) She should also share with us historical or ethnographic cases to convince us that it is possible for a handful of colonists, cut off from their homeland, to succeed in one or two generations in reestablishing “on a smaller scale” “the same level of sociopolitical complexity they were accustomed to” in their homeland. As an anthropologist, I am puzzled about the “how” of all this. But until I see the text of Ether mustered in a cogent manner, I cannot take seriously the straw-man terminology in Matheny’s critique of “incipient state” and “stratified society” and feel neither the need nor the ability to comment on her notions. I certainly do not believe the picture she offers that the Book of Ether reports near replication of Mesopotamian civilization in America. It makes no sense in terms of the scriptural text or cultural theory.

Matheny implies that I made an “equation” between Jaredites and Olmecs (p. 318). Not so, but perhaps my explanation in An Ancient American Setting was insufficiently clear. I said that, “identifying the culture in which the Jaredites were involved with the First or Olmec Tradition is very reasonable” (note my emphasis).149 I also took pains to explain how the Book of Mormon is a “lineage history” which recounts not the affairs of “a culture” as such but of a single descent group whose life develops intertwined with what may be a culturally and linguistically varied population that cannot be accounted for solely on the basis of the original Jaredite (or Lehite) colonizing party.150 Those who kept the records which Ether finally wrote up on his twenty-four gold plates and which Moroni eventually summa-

149 Sorenson, An Ancient American Setting, 119
150 Ibid., 50–55, 117, and 119.
rized for us were only Ether's ancestors. That is the record we have available, in severely abstracted form.

I treated the topic again from the point of view of population size, arguing that Ether's account does not make logical or anthropological sense unless we suppose that his Jaredite line lived among other groups, both other lineages from the original barges and different groups, too. I said, "there is no doubt whatever that many—perhaps most—aspects of culture in both the First [Olmec-age] and Second [Nephite-age] Traditions clearly did not come from the Old World. A unique [inherently Mesoamerican] configuration of distinctive, ancient patterns of life and thought characterizes this area at a fundamental level; no later introductions by diffusion [i.e. brought by Jaredites, "Mulekites," or Lehites] would have changed those much." So I never have equated Jaredites with Olmecs but have seen the Jaredites as one social element in a complex situation that included cultural, ethnic, and linguistic variety—some immigrant and some "native."

The Olmecs were bearers of an especially interesting early culture centered in tropical lowlands near the Gulf of Mexico. But the Jaredite lineage inhabited an area in the highlands (Moron, their continuing ruling seat, was "up" from the coasts). As far as the brevity of the record allows us to judge, Ether's lineage dwelt in Moron all along. My judgment was that this place was located in the state of Oaxaca (alternatively, I would now say that portions of Guerrero, Puebla or Veracruz might qualify). In those areas there were cultures related to but earlier than the coastal Olmec development, although scholars do not have a convenient cover term comparable to "Olmec" for the highland group(s). I used the term "Olmec Tradition" to encompass the whole Early and Middle Pre-Classic development, lowland and highland, which culminated in the classic Gulf Coast Olmec manifestation. Eventually Jaredite rulers and their rivals were also active in the east sea lowlands, where their extinction finally occurred. I suppose that Ether's lineage, originating with Jared, held a significant measure of rulership while "involved in" groups bearing Olmec-period cultures.

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151 Sorenson, "When Lehi's Party Arrived," 33-34.
152 Cf. Sorenson, An Ancient American Setting, 117, 119
153 Ibid., 112.
154 Ibid., 109.
A parallel exists near at hand with “the Aztecs.” Fr. Sahagun reported in the sixteenth century that there were twenty-one major peoples or cultures in the Basin of Mexico and surrounding regions at the time of the Spanish Conquest. They had migrated and settled at many different times yet possessed a common language, Nahuatl, and they related to each other politically and economically in a fairly stable system. Still their apparent unity was deceptive, for their histories, traditions, calendars and beliefs differed complexly one from another, and in addition to the common language they spoke different tongues of their own.155 The recent “emperors” over the “nation” were all of the dominant Mexica (“Aztec”) tribe (in Cortez’ day the ruler was Moctezuma Xocoyotzin [“Montezuma”]); those people had arrived in the valley as nomads from western Mexico less than three centuries earlier, only to borrow extensively from and intermarry with local groups. Nowadays, the public and all but a few scholars refer for simplicity only to “the Aztecs.” The underlying complexity of the varied peoples, languages and cultures that once made up the society is masked by our use of the umbrella term that designates the rulers. I expect that “the Jaredites,” that is Ether’s own lineage, in their situation could have been just as complicated in their relationships with others.

From the explanation I have just given of my model of the Jaredites as one lineage participating in a larger tradition, it should be clear that I consider their status very different from what Matheny referred to as “Jaredite civilization” (pp. 319, 320), a term I would not use without serious qualification. Note again that we could hardly expect a highly “emic” documentary source (that is, the book of Ether), giving history from the perspective of one ruling lineage, to involve, mention or connote all the elements of “Olmec civilization” as reconstructed by archaeologists. Still there are enough overlaps to convince me that the two are connected though not congruent.

This problem of identifying a minority group within a larger society and culture recalls the Popol Vuh. Robert Carmack attempted to relate the “Toltecs,” who were the protagonists in this famous Guatemalan “Book of Counsel,” to the archaeological background of their setting.156 He found evidence from lin-

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156 See Robert Carmack, “Toltec Influence on the Postclassic Culture History of Highland Guatemala,” in Archaeological Studies in Middle
guistic examination of the text of the *Popol Vuh* that “small numbers of the Toltec ancestors must have [entered the area and] come in contact with large, autochthonous, well-established populations.” Later the language, and presumably the culture and genes, of the immigrant newcomers “were apparently absorbed by the . . . much more numerous indigenous populations.” What is of most interest here is that when Carmack looked for confirmatory archaeological evidence of the arrival of the Toltecs, the results only broadly supported the sketchy historical framework in the traditional account. A handful of specific, surviving Toltec cultural features were found—but some of those seemingly related to regions and times other than what the text indicates—along with a number of more general cultural parallels to the “Toltec” origin area. Still, “many . . . features which would be expected as a result of [the immigrant party] . . . are lacking” (p. 64). Note that this difficulty in identifying evidence for a connection comes after only five or six intervening centuries. How much less likely it is that we would find close alignment between the limited cultural features revealed by excavation of sparse remains left by the Olmecs 3000 years ago and what is mentioned in the succinct and selective Jaredite lineage record! In my view the cultural and historical parallels we can detect are remarkable, given the data and interpretive problems.

Matheny states that “no one has convincingly demonstrated a link between any” of five writing systems developed in Mesoamerica on the one hand and the Nephite writing system(s) as reported in the Book of Mormon on the other (pp. 320–21). (In fact, there were more like fifteen systems known, although some fall together into families and some are barely identifiable.) This sounds like a serious charge, but let’s look more closely. The apparent significance is vitiated in the first place when we realize that nobody has made a serious attempt to demonstrate any links. More important, it is not clear how one would go about doing so. Several halfway approaches have been made. Carl Jones showed convincingly that characters on “the Anthon transcript” relate quite remarkably with the writing on a one-of-

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*America*, Tulane University Middle American Research Institute Publication 26 (1970), 49–92.

157 Ibid., 71.
a-kind artifact from Tlatilco, Mexico.\(^{158}\) Linda Miller Van Blerkom found that "the six main types of signs of a wordsyllabic system" are used equally in the Mayan and Egyptian hieroglyphic systems,\(^{159}\) but more fingerprints and a smoking gun would be nice. What we need is a competent linguist who's willing to follow up on these projects? (By the way, what would "competent" constitute?)

Finally, Matheny returns to the point with which she began—Sorenson "has called up numerous examples of findings from throughout Mesoamerica and beyond to show that the record is not settled on such problems as the presence of horses, sheep, barley, and the early practice of metallurgy. However, most of the references Sorenson cites are problematic in some way or another" (p. 322). Well, of course any indication that the current paradigm held by Mesoamerican archaeologists has flaws or gaps is going to be considered "problematic" by those who prefer to maintain that paradigm. Yet talk of a "paradigm," a concept that was jargonistically fashionable in the 60s and 70s, nowadays is more likely to come out as "political correctness." It seems to me that what bothers Matheny with my "problematic" sources has little to do with their truth value but much with their "p. c." She may think that I should exhibit more delicacy than to suggest that, for example, the Olmecs used metals when the textbooks (which rely on the opinions of the B. S., who get paid a lot more than I do) contradict me. Hence the denigrative "problematic" and "bits-and-pieces approach" may concern my indelicacy.

I am reminded of a warning by philosopher Alfred North Whitehead:

> When I was a young man in the University of Cambridge, I was taught science and mathematics by brilliant men and I did well in them; since the turn of the century I have lived to see every one of the basic assumptions of both set aside; not, indeed, discarded, but of use as qualifying clauses instead of as major propositions; and all this in one life-span—the most

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fundamental assumptions of supposedly exact sciences set aside. And yet, in the face of that, the discoverers of the new hypotheses in science are declaring, "Now at last, we have certitude." 160

Challenging and changing ideas is what is supposed to take place in science, or so I was taught at five universities. And that is what should be happening continually in anthropology and archaeology, but it can only proceed in the face of continual B.S. pressure to maintain the accepted version of "certitude."

E. James Dixon gives a nice example of the resulting conflict. He recounts how Prof. Brian Fagan, master of archaeological popular books, lectured in Fairbanks, Alaska, to "packed auditoriums and large luncheon audiences," telling them that "there exists no unequivocal data supporting human occupation of the Americas prior to circa 12,000 B.P."

The same day Thomas Dillehay, excavator of the remarkably early Monte Verde site in Chile, was talking in the same city to a small group presenting hard data completely contrary to Fagan. Dixon notes, "Though consensus [represented by Fagan's textbookish position] provides the security and comfort most people require, it frequently may not reflect the truth. Fortunately, most scientific debates are not subject to final resolution by popular vote." 161

Truth will seem outrageous before it seems acceptable. Judith Remington has phrased the problem for Mesoamerica as, "determined and often defiant adherence [by the B.S.] to assumptions that were no longer tenable. . . . New discoveries . . . wreak havoc with old hypotheses. Nonetheless, the hypotheses were presented as theories and defended fiercely, to the detriment of . . . scientific knowledge of the inhabitants of prehispanic Mesoamerica." 162

We all should be willing to "be instructed more perfectly in theory" (D&C 88:78). I am willing to change my theories and hypotheses, when the need is demonstrated. When an old shoe is worn out, it deserves to be thrown away for a new one, even if the replacement does not feel com-


comfortable at first. Archaeological theories or interpretations are similar. About the time they get feeling totally comfortable, we may find embarrassing holes in them—they are worn out.

Whatever degree of fit feels good to us, scholars as a group are inevitably faced with what archaeological theorist Lewis R. Binford calls “ambiguity in the facts of the archaeological record.” That means, he insists, that we cannot expect a perfect fit between “the facts” and any hypothesis or theory, but only “plausibility,” for “what is advanced as true remains dependent on . . . judgment” about what is and is not plausible.163

Now to recapitulate, in Matheny’s critique she has drawn attention to some minor points of informational value and has pointed out once more that there are serious gaps in knowledge when we attempt to compare the Nephite scripture with scholarship. But factually, methodologically, logically and epistemologically, she has failed time after time to do damage to my positions or to convince me of a need to change. My own criticism of the weaknesses in my writings remains more telling and helpful to improvement than anything she has given me thus far.

In a helpful spirit I warn Matheny and other potentially productive researchers about falling into a pattern of intellectual activity that has wasted many good minds and chewed up the lives of many good people in the past. I hope she will back off and take a candid look at what her critique displays. Hugh Nibley has stated the problem aptly: “The normal way of dealing with the Book of Mormon ‘scientifically’ has been first to attribute to the Book of Mormon something it did not say, and then to refute the claim by scientific statements that have not been proven.” I join Nibley in urging: “Let us not oversimplify and take the Book of Mormon to task for naive conclusions and images that are really our own.”164

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