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On Folk Archaeology in Anthropological Perspective

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Michlovic (CA 31:103-7) criticizes colleagues' analyses of folk (or "cult") archaeology (see Cole 1980, 1982; Feder 990; Harrold and Eve 1987), saying that anthropologists should take it more seriously, but he dismisses those of us who have done so. His work on Minnesota folk beliefs about the Kensington Stone and pre-Columbian Minnesota Vikings pursues a research path I have long advocated and draws conclusions similar to mine (although I might note that more than one of the colleagues he cites and criticizes agree about the research agenda but reject his and my conclusion that the Kensington Stone is a fraud). His argument that Viking lore in Minnesota is not racist or ethnocentric is no doubt largely correct. I have stressed this point myself with regard to the majority of people attracted to such claims (Cole 1979, 1980, 1982) while noting that cult-archaeology beliefs *can* be more insidious. The "ancient astronauts" mystique, for example, plays on negative stereotypes of non-Europeans as hapless peoples needing outside help to accomplish anything impressive (according to von Däniken virtually no help from outer space was needed by Europeans). I have heard "America B.C." advocates speak matter-of-factly about their proof that America was settled by Christian civilization before "savages" temporarily disrupted the supposedly proper historic order of things. This is indeed a folk belief worth studying, but I consider it important that scholars deal with both the phenomenon *and* the specific details.

Folklore is fascinating, but its significance to participant/believers is different from its significance to analysts trying to explain it. Although it is confirmed by his own research, Michlovic seems to miss this point, arguing that "all ideas are equal" even while acknowledging that Viking colonization in Minnesota does not actually seem to have happened. Similarly, he attacks people who have worked to defend public-school teachers

against pressure from the so-called scientific-creationist camp. One can analyze creationist activities sympathetically and still criticize their politicized pseudoscience (Cole 1983, 1988; Cole and Godfrey 1985; Godfrey and Cole 1986). I would suggest that communicating skeptical analyses and publicizing scientific and scholarly approaches and conclusions are our public responsibilities as scientists/scholars/citizens.

I admit that I *do* associate with scientists, and enjoy it. In fact, as president of the National Center for Science Education, I try to organize their support for the competent teaching of evolution and other sciences in public schools and in the mass media. As the world struggles with science illiteracy, such efforts may be late, but I fail to see them as "scientism" or anything for which to apologize. Michlovic implies that there is an Establishment conspiracy to defend narrow official ideas. On the contrary, the fact that all of the "villains" he cites are more or less acquainted with each other is evidence of how lonely our approach really is. There is, unfortunately, a larger career insurance dividend available for looking the other way.

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Michlovic criticizes archaeologists in general and a group including me in particular for our response to what he labels "folk archaeology" (elsewhere called "cult," "fantastic," or "pseudo-" archaeology [Williams 1987]). He believes that we are engaged in what amounts to folk-archaeology bashing. He suggests that our motives are several: (1) we wish to monopolize the interpretation of the past, (2) we desire to monopolize funding for its study, (3) we want to further our careers, and (4) we worship the trappings of science. Such accusations are ill-conceived and puzzling.

Archaeologists have anything but a monopoly on the interpretation of the past. For example, De Camp (1970) has estimated that there have been over 2,000 publications on the question of the Lost Continent of Atlantis. A very small proportion of these have been written by professional archaeologists. Many books have been published on ancient astronauts, psychic archaeology, New Age archaeology, pyramid power, etc. (see Feder 1984, 1990), but I count only five responses by professional archaeologists, historians, or other scientists in the past 30 years or so (Wauchope 1962, White 1974, Cazeau and Scott 1979, Stiebing 1984, Feder 1990), and the first two of these are out of print. Professional archaeologists direct most of their publication efforts toward other professional archaeologists. If anything, we have been remiss in not being more active in communicating our view of the human past to an interested lay audience. If archaeologists have monopolized interpretation of the past, it is only in the pages of *American Antiquity*, certainly not in the trade books and popular magazines that

most people read. We are not motivated by a folk-archaeology challenge to our "monopoly," because no such monopoly exists.

We are indeed concerned about research funding, not for our own sakes but for the sake of the archaeological record. I think that, as colleagues, we would presume that an ethnographer working to protect the rights of a tribal group was doing so out of a genuine concern for the people rather than as part of a cynical attempt to preserve a database for his or her own benefit through government grants or career advancement through publication. I would hope that, as colleagues, we would afford archaeologists the same courtesy. The outrage that archaeologists feel over, for example, the recent choice by Australian local authorities of an "electro-magnetic photo-field survey" (essentially dowsing) of a historic farmstead over a more expensive genuine archaeological survey (see Feder 1990 for details) rests on the reasonable fear that because of it an important archaeological site may never be adequately studied. It is certainly appropriate to ask why a group might be convinced of the reality of a procedure based on measuring with bent wires a phenomenon not known to exist. Michlovic is welcome to initiate such a study; I only ask that he recognize the value in showing that the technique itself is dangerous nonsense.

That "many archaeologists who have joined the attack on folk archaeology are also apparently motivated by the opportunity for professional advancement" (pp. 103–4) is a serious charge. Citing Harrold and Eve's (1987) assertion that it will take a lot of "work" to counter the claims of the cult archaeologists and the fact that, in the literature examining cult archaeology, "the same names appear repeatedly" (p. 104), Michlovic concludes that many of us concerned about the misuse of archaeology are involved, at least in part, to advance our careers. In fact, it is quite common for us to receive a negative response from our colleagues; we are often asked why we waste our energy thinking, researching, and writing about nonsensical claims. Colleagues on promotion and tenure committees are far more likely to ask why we don't spend all of our time conducting "real" archaeological research than to applaud and reward our efforts in this regard. I became concerned about unsubstantiated claims about the human past quite simply because I care passionately about that past. Archaeologists who ignore public misperceptions about the past are, I believe, abdicating their professional responsibility. I and others devote energy to cult archaeology *in spite of* colleagues' opinion that we are wasting our time.

The criticism pointing to "archaeology's embrace of scientism" (p. 104) is unfathomable. Should we hang our heads because we "hope to represent the past scientifically and respectably to a larger public" (p. 104)? Are we to be ashamed (and is it really the case) that we "prefer to associate" with "geologists, biologists, ecologists, chemists, and the like" (p. 104)?

The research that Michlovic is conducting on notions of a pre-Columbian Viking presence in the American upper Midwest seems to be precisely the kind of study

many of us have been exhorting people to do—and similar to what we have been doing (Feder 1984, 1987; Harrold and Eve 1987; Hudson 1987). I applaud it while reminding him that there is plenty of room in the discipline for those interested in countering nonsense along with those interested in explaining why some are susceptible to it.

Michlovic criticizes archaeologists for ascribing belief in the Viking presence in the upper Midwest to "ethnocentrism." He has a far more anthropologically sound, culturally relativistic explanation for why the descendants of Vikings wish to believe that their ancestors got here before Columbus: "ethnic pride" (p. 105). Is his argument based, after all, on semantics? Finally, he criticizes archaeologists, in essence, for not conducting ethnographies of the folk archaeologists, opting instead simply to attack their beliefs. If he had only heeded his own advice and conducted an "ethnography" of archaeologists—asked us why we think it is important to respond to what we perceive to be pseudoscience—he might have written a very different commentary.

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Michlovic finds much to criticize in the response of archaeologists and others to the fanciful beliefs about the past known as "cult archaeology" or, in his term, "folk archaeology." We think his allegations merit a reply.

His charge of career opportunism is ironic, for cult-archaeology studies by no means constitute a "hot" field full of career possibilities. Most of us work mainly in conventional research areas. Indeed, some of us have been cautioned by colleagues that our careers could be sidetracked by work in what some consider a fringe area. What is more fundamental, it is puzzling to see our work on a subject we think important characterized as careerism. If this is really so, then very few anthropologists (or other social scientists) are innocent of such careerism.

Since Michlovic never details how archaeologists' scientific aspirations are folly, we will not pursue this criticism at length. We will note, though, that archaeologists are joined by many of his colleagues in cultural anthropology (e.g., O'Meara 1989) and most other social scientists in considering human cultural behavior amenable to scientific study. Furthermore, he nowhere shows how this alleged scientism is a motive (as opposed to a heuristic device) in the response to cult archaeology. As he himself points out, archaeologists were calling these beliefs nonsense before the days of processual archaeology.

Michlovic's principal charge is that instead of shouldering the burden of understanding the cultural contexts in which cult beliefs thrive and the cultural needs they serve, archaeologists flail away with self-serving rhetoric at believers. In suggesting that cult-archaeology beliefs

are not analyzed in cultural context, however, he ignores the considerable topical range of the literature, which is produced by sociologists, psychologists, and historians as well as anthropologists and is of several types. Much of it is written for the general public with the aim of refuting cult claims by providing both information and principles of critical thought so that readers can see their shortcomings for themselves (e.g., Feder 1990, Cazeau and Scott 1979, Stiebing 1984, Sabloff, 1982, White 1974). The authors of such works are indeed strongly critical of cult archaeology and its producers, but they seldom berate ordinary believers, who after all are among their intended audience.

Then there are publications meant to alert fellow professionals to these beliefs, their popularity, and their dismal implications for the state of education and skeptical rationality in American society (e.g., Cole 1980; Williams 1987, 1988; McKusick 1984; Engler 1987; Feder 1984; Almquist and Cronin 1988). Sometimes their language is strong, but the denunciations are again usually directed toward the producers of such pseudoscience rather than its consumers.

Finally, a significant amount of the literature on unsupported beliefs about the past in fact does analyze these beliefs in their sociocultural context (e.g., Bainbridge 1978, Womack 1982, Hudson 1987, Gray 1987, Harrold and Eve 1987). Michlovic manages to ignore such work even while citing it. When he says, for example (p. 103), that we "associate creationism with racist attitudes about intelligence," a reader might well infer that we were simply slinging another insult at creationists. In fact, we were reporting the findings of a survey of 979 college students in three states (conducted with Kenneth Feder and Luanne Hudson). What we found was that in each sample there was a statistically significant positive correlation (Pearson's r from .21 to .35) between a scale measuring creationist belief and acceptance of the statement "Some races of people are more intelligent than others." We suggested that this finding was explicable in terms of the tendency (which we also documented) for creationists to be found among sociopolitically conservative evangelical Protestants. Our study was concerned with understanding creationism in relation to such sociocultural variables as education, ideology, attitudes toward science, and demographic background. Michlovic may agree or disagree with our methods and findings, but he misrepresents our work when he implies that it consists of diatribes against folk beliefs. Similarly, when he cites (p. 104) our statement that "here is plenty of work for scholars of all disciplines, work that is urgently needed" (Eve and Harrold 1987:150) as a call for archeologists to "set the record straight," he again misrepresents us. The passage in question calls for neither calumny nor refutation but the kind of explanatory, contextual research which Michlovic says he wants.

Our own and similar research has found important variation among "cult" beliefs about the past in terms of who holds them and why. Only some such beliefs fit Michlovic's characterization of the Viking legend—folk

beliefs perpetuated because they serve to reinforce ethnic and community identity and solidarity. Barry Fell's tales of ancient European wanderers in North America play a similar role for some believers (Cole 1980). Biblical creationism serves an even more central function as the foundation for a religious community's whole world view (Harrold and Eve 1987, Eve and Harrold 1990). In contrast, acceptance of most cult-archaeology claims (e.g., ancient astronauts) seems to be unconnected to fundamental values or to ethnic identity and is probably related to exposure to mass entertainment media and lack of the intellectual tools for recognizing their shortcomings. These latter claims originate among writers more often than among ethnic groups. They differ in intensity from creationism or Scandinavian-American folk archaeology and are probably more susceptible to change.

In short, it is not news to us that cult beliefs do not result simply from stupidity or racism; Michlovic has overlooked the work that shows this. Such work is paralleled, by the way, by a considerable body of research examining the gamut of pseudoscientific beliefs from New Age creeds and astrology to UFOlogy (see, e.g., Hines 1988, Singer and Benassi 1981, Abell and Singer 1981, Eve and Dunn 1990).

Our view of cult archaeology is clearly less sanguine than Michlovic's. It is unclear just how he thinks we should treat it. On the one hand, much of his article reads like a plea for cultural relativism. On the other, he agrees that archaeologists have a better claim than cult archaeologists to knowledge of the past and are "right to set the record straight insofar as they are able" (p. 106). But he does not make clear how archaeologists are supposed to correct that record without using rhetoric implying that they know better than cult believers.

We think that adopting a completely relativist position with regard to one's own society is neither possible nor desirable. We value archaeology's hard-won knowledge of the past, believe that it can add a valuable perspective to the education of any thinking person, and are concerned that many people (including some of our students) accept fanciful beliefs without regard for their lack of intellectual warrant. If we value the integrity of our professional work, some boundaries are worth demarcating. Presumably, cultural anthropologists who encounter beliefs that non-Western peoples are "savages" in need of being "civilized" can be forgiven the urge to engage in boundary demarcation, as can social scientists facing folk beliefs among American whites that depict blacks as inherently inclined toward laziness and crime. Beliefs like these have deep cultural roots and serve identifiable cultural functions. Should social scientists therefore avoid pointing out their falsity? We see no contradiction between working as social scientists to understand cult archaeology and working as educators to show how it is fallacious.

In sum, Michlovic represents incompletely the spectrum of cult-archaeology beliefs and distorts the response to them by archaeologists and other social scientists while misreading their motives. Unfortunately, his

appraisal of our work shows little evidence of the "spirit of open inquiry" that he urges upon us.

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24 I 90*

Michlovic lumps three quite distinct phenomena in his discussion of professional archaeologists' alleged scorn for "popular archaeology," and he has not kept up with recent work on the question of medieval Norse in the western Great Lakes region.

He lumps the political movement known as scientific creationism with what could be termed entertainment ("fantasy," "cult," "psychic") archaeology and with the serious claims to Norse entradas into the Great Lakes system between the 11th and the 15th century. In contrast, *Science's* reviewer for Harrold and Eve's (1987) *Cult Archaeology and Creationism*, William Sims Bainbridge, had no difficulty following that book's title in distinguishing "cult archaeology" from "creationism." The first two chapters in the book, by William H. Stiebing and by me, emphasize an anthropological understanding of the two phenomena, as Bainbridge makes clear (1988:1048). My chapter dealt only with scientific creationism, which the United States National Academy of Sciences characterized as a "challenge to the integrity and effectiveness of our national educational system and to the hard-won evidence-based foundations of science." The Academy stated, in 1984, that to "remain silent . . . would be a dereliction of our responsibility to academic and intellectual freedom . . . as . . . designated advisor to the Federal Government" and called scientific creationism's teaching "contrary to the nation's need for a scientifically literate citizenry" (quoted in McCollister 1989:35). The well-funded campaign of the radical Christian right to turn its fundamentalist tenets into law is very different from the sci-fi paperbacks of fantastic archaeology or the small cult groups such as that led by Barry Fell. All can be understood anthropologically, but only if they are not lumped.

The question of Norse entradas is entirely different. As Michlovic reports, Scandinavian-descended communities in Minnesota have used claims for medieval Norse exploration to promote community pride and tourism, but this "popular archaeology" is neither cultic nor fantastic and is political only in a small, benign way. The presence of the Norse in northeastern North America during the period of their Greenland settlements is well accepted now and includes at least one building on Newfoundland (L'Anse aux Meadows). The sudden appearance of widespread and severe tuberculosis among western Great Lakes Indians in the 11th century can be best explained, in my opinion, as infection from contacts with Norse either along the Gulf of St. Lawrence (traveling up the river, Indian to Indian) or in the Great Lakes region. The latter is supported by a careful reappraisal of the Kensington Runestone by

Nielsen (1988), who argues that its supposed "errors" are variations consistent with the Bohuslän dialect of Middle Norwegian in the 14th century. Michlovic is welcome to heed his own advice on following the spirit of free inquiry and join those of us who have been working with the medieval Norse boat nail and similar finds from the western Great Lakes, Nielsen's work on the Runestone, and the tuberculosis problem.

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On Middle Paleolithic/Middle Stone Age Hominid Taxonomy

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We accept the notion that "the pattern in the evidence for symbolic behavior is the same whether the hominids associated with Middle Paleolithic/Middle Stone Age archaeological assemblages are archaic *H. sapiens*, Neanderthals, or morphological moderns" (see Lindly and Clark, "Symbolism and Modern Human Origins," CA 31:233–40) and want to discuss one of the implications Lindly and Clark suggest (p. 239): that "the taxonomic units themselves are unreliable." It is our position that the morphology of the hominids at the Middle Paleolithic/Middle Stone Age sites with large samples provides support for this suggestion. In particular, we draw attention to the samples from Mount Carmel, Qafzeh, and Klasies River Mouth. These sites are large enough to allow the assessment of *sample characteristics*, which are found to be a more valid source of taxonomic information than isolated individuals, especially when the comparisons are between closely related groups whose ranges of morphological variation are likely to overlap markedly.

After McCown and Keith's comprehensive study, the Mount Carmel remains were divided into three parts, now deposited at the Rockefeller Institute (Jerusalem), the Harvard Peabody Museum (Cambridge), and the British Museum of Natural History (London). The specimens have never been reunited, and therefore the McCown and Keith monograph (1939) is based on the only study of the sample as a whole. There are three important points to be made about the conclusions they drew from their analysis. First, they regarded the Skhül and Tabün specimens as representing the same population ("we had before us the remains of a single people" [p. 12]), and Tabün was not always the more archaic (see discussions of the Skhül IV nasal process, IX orbit shape, VI nuchal area, V relative radius size, II coronoid process, and the midshaft femur shape of specimen 7). Second, the Mount Carmel folk were thought to be "in the throes of evolutionary change" (p. 14) because numerous individual intermediate features and sets of intermediate characters "bridge the structural hiatus lying between the Neanderthal and the Neanthropic types" (p. 372). Third, they contended that "the Mount Carmel people are not the actual ancestors of the Cromagnons but Neanderthaloid collaterals or cousins of the ancestors of that type" (p. 17). In our opinion, a half-century of additional discoveries and comparisons provides no basis for regarding the Mount Carmel remains as any more modern than McCown and Keith did (in fact probably less so, since the subsequent discovery of Amud shows that the Skhül sample overlaps even more with the Levant Neandertals than they observed).

There is some thought that a similar interpretation cannot be applied to Qafzeh, even though it appears to be earlier if the thermoluminescence and electron-spin-resonance dates can be accepted as valid. Some workers consider Qafzeh to be even more modern than Skhül, explaining the later Levantine appearance of Neandertals (or of a mixture including Neanderthal morphology as is described for Skhül) as the consequence of interbreeding of local "moderns" with European immigrants forced southeast by the deteriorating climate of the last glaciation. But is Qafzeh more modern, or even less Neanderthal-like? Our research suggests that it is not. Qafzeh 9 is the specimen inappropriately treated as the "type" for this site. However, it is between adolescent and adult in age, according to Vandermeersch, and a teenager of 15–16 (dentally) by our analysis. For a specimen so young it has unexpectedly great supraorbital development (unfortunately, the entire glabellar region is missing); the bulk of male supraorbital growth is in the late teens and early twenties. In our opinion the "modern" status of the Qafzeh sample is overstated for other reasons as well. For instance, the averages for the dental metrics (including the anterior teeth) are similar to or even greater than in the earlier Würm European Neandertals. The Qafzeh 3 female is far too archaic to be conceivably regarded as a "morphological modern," and the surprising amount of cranial plaster suggests that many of the Skhül 5–like features of the Qafzeh 6 male may be more the consequence of decisions made during reconstruction than reflections of the specimen's biology.