

## Communications

### Evolutionary Archaeology Challenges the Future of Archaeology: Response to O'Brien and Lyman

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Michael J. O'Brien is Robert Dunnell's bulldog. Thirteen books or papers co-authored with Lee Lyman, seven alone or with T. D. Holland, all within this decade, are cited in the *Review of Archaeology* "Commentary" that elicited this review. A barrage indeed, bespeaking an evangelical fervor not seen in our ranks since the 1960s New Archaeology take-over (Binford 1972, Woodbury 1993:304-05). Again there is a "Mafia" [who] have all now received their degrees and are off teaching at universities all over the country" (Binford 1972:13). Shades of the 60s, we are lectured that "if we ever hope to place archaeology on a sound scientific footing," we should acknowledge that what currently passes "is *not* a unified body of principles and methods, but to be taken seriously it must become just that" (O'Brien and Lyman 2000:19). This time, the Mafia follow not one of their generation, but a senior don. There is an earlier parallel, too, in the 1860s X-Club's take-over of the Royal Society to advance Darwin's views, and not coincidentally their own reputations (Kehoe 1998:21).

*First*, the format of O'Brien and Lyman's essay. It is formulaic, which, as Sharon Traweek notes, is characteristic of scientific papers, minimizing "the agency of the scientists involved" (Traweek 1996:143). Opening with an invocation to patron saint Alfred Kidder, O'Brien and Lyman announce they stand within our hallowed tradition. This impression is reinforced when they describe Kroeber "inventing" frequency seriation (p. 17) although Trigger (1989:200) credits Flinders Petrie nearly a generation earlier and notes (1989:188) that Kidder studied with the Egyptologist George Reisner. Dunnell, O'Brien, and Lyman are cited as authorities so frequently throughout the essay that readers must conclude they have few peers. These are rhetorical ploys not to be ignored.

Beyond this essay, explaining why O'Brien

submitted it to *ROA*, is the Dunnell group's strategy—imitating that of Binford in the 1960s—of publishing in every major and second-tier journal, and presenting organized sessions in the principal meetings (O'Brien 1996:15 n. 1). Their extraordinary prolixity fills lists of references cited, enhancing the appearance of authority and deflecting recourse to other programs and positions.

*Second*, is "a unified body of principles and methods" (p. 19) necessary and characteristic of a scientific field? O'Brien and Lyman confuse a research program with science itself. There are and always have been a number of research programs simultaneously within any scientific field, a situation likened by Toulmin (1972) to Darwinian populations subject to selection. Joan Fujimura views research programs as "standardized packages . . . of ambiguous concepts and standardized tools, of theory and methods, . . . crafted tools . . . used by scientists to define their areas of expertise and power" (Fujimura 1992:204-205). She notes that methods are used to "instantiate and substantiate . . . theory. . . . It is through the use of standardized packages that scientists constrain work practices and define, describe, and contain representations of nature and reality" (1992:205). There are "other possible representations, other ways of knowing and practicing" (*ibid.*), a position reiterated by a range of sociologists and philosophers of science (e.g., Ross 1996, Harding 1996, Eisenhart and Finkel 1998). Even within evolutionary biology, different research programs admit differing concepts of what constitutes a species, leading Kitcher to comment,

*Pluralistic realism rests on the idea that our objective interests may be diverse, that we may be objectively correct in pursuing biological inquiries which demand different forms of explanation, so that the patterning of nature generated in different areas of biology may cross-classify the constituents of nature* (Kitcher 1992:336).

Kelley and Hanen's *Archaeology and the Methodology of Science* (1988) lays out, in straightforward language, contemporary understanding of the tentativeness crucial to science. Gibbon's *Explanation in Archaeology* (1989) maps the philosophical debates underlying efforts that "emphasize

explanation instead of simply interpretation" (O'Brien and Lyman, p. 19). These important works are not cited in any of the Dunnell sect's publications I have seen (of course I haven't seen all their output: the latest *Bulletin of the Society for American Archaeology* [May 2000] advertises yet two more new O'Brien-Lyman books, from Kluwer).

The sociology of scientific knowledge has focused on the question of whose interests are served by promoting one or another method or theory. "Expertise and power" go together, conferring status that generally carries with it practical rewards of income, prestige, and continued funding for the expert's research program. Within our present conservative political climate, "basic science" is favored over "social science" (Nelkin 1996:121), selecting for research programs that look like such "basic science" as evolutionary biology. If Dunnell, O'Brien, Lyman, and their group can garner funds by presenting their package as basic science, specifically opposed to competitors' "unscientific" archaeology, they can obtain academic posts in a tight job market geared toward grantsmanship.

*Third*, is the evolutionary archaeology of Dunnell and his followers "a sound scientific footing" for a research program? This must be the crux of the issue, for if their research program is soundly based, it should prevail. From the standpoint of evolutionary biology, unhappily, their program fails to engage fundamental questions.

Let us begin with Mayr's classic definitions of organic evolution: "change in the genetic properties from generation to generation owing to differential reproduction." Immediately we see the problem with evolutionary archaeology if it is modeled on evolutionary biology: non-living artifacts don't have genes. Genetic properties are directly replicated and until this decade, were beyond human manipulation. Not only natural, but also human, selection operated on independently occurring variations and has been constrained by the given (Williams 1992). "Descent with modification" means an awesome continuity of germ plasm. Non-living artifacts do not replicate themselves, they do not lie in a relationship of descent; instead, their common property may lie in the agency of the human who constructed them. Evolutionary biology deals with agent-

less processes. Archaeology deals with the residue of human agency.

Selection within organic populations tends toward greater fitness. Biologists therefore worry over identifying the constituents of fitness. On the one hand, fitness holds only contingently, within a particular locality and time; it refers to a historical moment. On the other hand, fitness is highly constrained by genetic properties, thus likely to be imperfectly realized. Furthermore, fitness is recognized by the reproductive success of its bearers. O'Brien has claimed (1996:12) that "There is no reason . . . not to apply the same procedures archaeologically that biologists apply to examine fitness." Then he states, "The best means for doing so would appear to be through the analysis of engineering design of phenotypic features such as pottery" (ibid., citing four of his publications). Ah yes, intelligent design (whoops! he surely doesn't mean that, shades of the Kansas State Board of Education)—but that has nothing to do with reproductive success. Bruce Trigger, in his recent survey of sociocultural evolutionary theories, dismisses Dunnell and his group in a few paragraphs (Trigger 1998:135-137).

Artifacts are more or less functionally well designed, their designs more or less constrained by raw materials available, by technologies, and by *iconic status* (the meaning of the artifact [Linton 1936:402-404]). Their reproduction depends on human agency, with meaning possibly overriding other characteristics in selection—witness Victorian dress, or the red coats that made British soldiers such easy targets. Human agency can render a fit artifact extinct, as in the case of the Tiwanaku *suka kollu* raised fields, which whatever the cause of their abandonment, were not returned to use when environmental conditions warranted it, their function having been forgotten (until recognized by archaeologists) (Kolata 1996).

O'Brien and Lyman use the language of evolutionary biology, for example "heritability" (p. 17). Heritability refers to factors producing stability, i.e., the constraining effects of genetic replication (Sober 1984:151). Biologists test for heritability by comparing parents and offspring and calculating the percentage of similarity in characteristics (e.g., Hoffmann and Parson 1991:67-68). An example would be, "Crossbill [bird] off-

spring produced in captivity resemble their parents in bill and body measurements . . . suggesting that these measurements are highly heritable in crossbills" (Thompson 1994:150). O'Brien and Lyman say, "heritability ensures that we are examining change within a lineage rather than an instance of convergence" (p. 17). Yes, if the lineage results from continuity of germ plasm. Let us take as an example of the problem in archaeology, the debate between Bradley and Stanford, on one side, and Straus on the other, over whether Solutrean and Clovis form a lineage (Straus 2000). Bradley is convinced that blades evince "heritable" knapping technique, Straus (2000:224) that "technological convergence" occurred. Bradley emphasizes the difficulty and rarity of the overshot technique with occasional side hinge fracture (in a lecture in Milwaukee), Straus the numerous differences in artifact inventories. There seems no possible way to decide the issue on the basis of lithic technology alone.

#### THE CRUCIAL ISSUE

The hubris of Dunnell, O'Brien, Lyman and their group in flooding our journals and meetings with their claim to an exclusive understanding of science in archaeology is off-putting. They seem not to understand the fundamental significance of continuity of germ plasm, rendering their use of terminology from evolutionary biology metaphorical, not precise. The danger is not that they try to grab our trusty Marshalltowns and give them back, gilded, it is that they perpetuate the nineteenth-century assumption of a singular human culture: the phenomenon of cultural evolution (p. 20).

Hidden in the dark recesses of O'Brien and Lyman's edifice of publications on the history of American archaeology is the dragon figure of Leslie White. Carneiro had recalled, "We students had the feeling that we were being armed with powerful intellectual tools with which to go out and conquer the world for evolution and culturology. . . We never doubted for a moment that our views (White's views). . . would ultimately triumph" (Carneiro 1981:224). The connection between Dunnell and White is recondite, although obvious between White and the overlapping loose group represented in the volume edited by Geoffrey Clark (Barton and Clark 1997), a Michigan doctorate, and

of course that published by the University of Michigan (Rambo and Gillogly 1991). In the latter, editor Rambo credits Dunnell's contribution to the school (Rambo 1991:27). Earlier acknowledgment appears in the Whallon and Brown (1982) *Essays on Archaeological Typology*, especially in Dwight Read's chapter "Toward a Theory of Archaeological Classification;" Whallon was and is with the University of Michigan Museum of Anthropology and Brown is associated as a member of the Chicago New Archaeologists led by Binford. Dunnell's shift toward the New Archaeology group is an example of the difficulty in distinguishing lineage from convergence in human behavior, since Dunnell's lineage is from Rouse and Chang but his work converged toward the Michigan lineage founded by White.

White's influence on the Dunnell group appears in that use of the singular in referring to human cultural evolution, and in the implicit conception of artifacts, i.e., "culture," as humans' extrasomatic extensions, part of their phenotype (p. 20; O'Brien 1996:13). The same premise that "culture" is singular, that *it* evolves, is seen in the otherwise intelligent essay by Rambo (1991:92). In 1971, Dunnell stated, "Once the discipline has been defined and the notions of artifact and culture theoretically defined, the development of a model of prehistory's formal operations is relatively straightforward" (Dunnell 1971:191). He then defined "culture" as "a concept referring to shared ideas used as an explanation" (1971:201), accomplishing thus easily what Kroeber and Kluckhohn together (1963), attempting a satisfactory definition of "culture," failed to achieve. "Culture" is a fuzzy image and a word in some Indo-European languages: it does not exist *per se*, to premise it as superorganic is to fall into nineteenth-century romanticism, and to assume its significance is unproblematic is naive.

When Kidder, in the passage quoted by O'Brien and Lyman, and then the Michigan-influenced postwar archaeologists posited the study of cultural evolution to be their goal, they demonstrated the divorce between American archaeology and Boasian cultural anthropology identified by Pinsky (1992). This superficially amicable divorce, maintaining functioning structure despite lineage convergences and divergences (Williams 1992:122), masks the gulf

between the "American-born gentile" population (White 1966:16) to which belong Lewis Henry Morgan, Major Powell, Leslie White, and such contemporary archaeologists as Geoffrey Clark and Timothy Earle, and politically liberal and engaged anthropologists from Boas through Sol Tax, Oscar Lewis, David Aberle, and—to select one current exemplar—Jonathan Marks. Morgan and Powell's virulent racism beneath the calm surface of evolutionary archaeology can be perceived from the perspective of sociological studies of scientific knowledge.

Although Dorothy Ross's *The Origins of American Social Science* (1991) does not directly discuss anthropology or archaeology, her deep probing of her topic applies to our discipline's history:

In the American social science traditions, where the problem of historical change was of central concern, the modernist acceleration of historical time heightened traditional anxieties and intensified the desire for control. Social scientists sought permanence and value by reconstructing transitory historical experience into natural process and by exerting rational control (Ross 1991:318).

Henry Adams put it powerfully:

He was a Darwinist before the letter; a predestined follower of the tide. . . The ideas were new and seemed to lead somewhere—to some great generalization which would finish one's clamor to be educated. . . . Such a working system for the universe . . . enforce[d] unity and uniformity on people . . . ; the idea was only too seductive in its perfection (Adams 1931[1907]:224-226).

Myopia over the political resonances of a philosophical position darkens its claim to objectivity. Steven Shapin points to

a great paradox that lies at the heart of modern science and that was, arguably, put there in the seventeenth century. . . . The more a body of knowledge is understood to be objective and disinterested, the more valuable it is as a tool in moral and political action (Shapin 1996:164).

Dramatic illustration of this paradox, directly relevant to American archaeology, occurred on March 8, 1991, when Chief Justice McEachern of the British Columbia Supreme Court ruled in *Delgamuukw et. al. v. The*

*Queen et. al.* against the Gitksan Wet'suwet'en claim to its ancestral lands and practices. Mr. Justice McEachern based his opinion on experts he admitted to testify, these including archaeologists but *not* cultural anthropologists because he supposed cultural anthropologists would be advocates for the plaintiffs (the Gitksan) (Asch 1992:235-236). Archaeologists, by implication, would be objective, unbiased. I have already (Kehoe 1998:185-187) demonstrated the inaccuracies and distortions in Timothy Earle's picture of Northwest Coast societies, of which the Gitksan would be one; Earle's evolutionist model conforms to McEachern's judgment. That the Supreme Court of Canada overturned McEachern's judgment in 1997 does not nullify the pernicious potential of unexamined conventional models.

#### CONCLUSION

There is certainly "room for theoretical and methodological challenges" (O'Brien and Lyman 2000:19), and a number of significant critiques and substantive studies have been published under the evolutionary archaeology banner—not least O'Brien's essay in *ROA* 16 (1995, not cited in his *ROA* 2000 paper). The fundamental problem with the evolutionary archaeology program is that its adherents remain locked into an obsolete nineteenth-century version of science, exactly the problem with the New Archaeology of the 1960s. From the point of view of a sociology of scientific knowledge, it is interesting that the same sort of unitary cultural evolutionism was promoted to consolidate the power position of the Victorian X-Club, 1960s New Archaeology, and 1990s evolutionary archaeology.

David Hull, surveying discussions of evolution from his standpoint of philosophy, emphasizes, "the most important feature of [evolutionary processes] is that at all levels it is spatiotemporally localized. Evolution is a local, not global phenomenon" (Hull 1989:221); "the only sorts of explanations possible are highly particularistic statements of the operative particular circumstances" (Hull 1989:240). This is what archaeologists are good at, this is what we have been doing at least since seventeenth-century surveys began mapping distributions. Culture histories are pauperized history and only roughly congruent with societal "cultures" ("forms of life" in Wittgenstein's phrase adopted by

several sociologists of science to indicate participants in shared behavioral and imaging patterns [Barnes, Bloor and Henry 1996:116]). Nevertheless, they are what we can build from stratified sequences and geographical distributions of artifacts. Culture histories are enriched with ecological and geological data, documentary texts (*sensu lato*—art as well as literate), and principled ethnographic analogies, and inferences may be drawn from them regarding possible or probable causal events. Many such hypotheses may be tested. Evolutionary biologists study the beaks of finches and crossbills to infer the histories of avian groups and discern factors that influenced those histories. Archaeologists study artifacts to infer the histories of human groups at various geographic and temporal scales, and discern factors and events influencing these histories. Why we do these studies, how we do them, the language we use, are socially constructed and fundamentally ethnocentric, as numerous encounters with First Nations citizens are teaching archaeologists.

O'Brien and Lyman, and their mentor Dunnell, ("Today prehistory is rapidly becoming a science" [1971:185]), deny the scientific methods and understanding demonstrated by Thomas Jefferson, Daniel Wilson, and Will McKern, not to mention British and other archaeologists outside the United States. When one of their acolytes (Maschner) changes his mind about their research program, they pooh-pooh his work (pp. 15, 19). When Stephen Jay Gould veers from the early-1980s essays that struck Dunnell's fancy, Dunnell declares, "Gould's more recent work shows none of the early spark and often is simply bad biology" (Dunnell 1996:ix-x). One can bow down before such authority, challenge it titan to titan, or sidle out of the room. Excuse me, I'll be looking at fabric impressions on sherds. I've been looking at quite a few sherds over quite a number of years, and at existing twined bags and cloths. I've noticed that twined bags are used for ritual and medicine objects among First Nations people in the Northwestern Plains and Plateau. I think I've come a little bit to know something. It feels scientific. It isn't evolutionary archaeology. □

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