

Evolutionary Archaeology and Its Future Directions: A Rejoinder to Kehoe

By Michael J. O'Brien and R. Lee Lyman

Kehoe's comments are predictable given her dislike of evolutionary archaeology and her track record of misreading what evolutionary archaeologists have said (e.g., Kehoe 1997, 1998, 1999). There usually is little to be gained by responding to an article such as Kehoe's, but in this instance we feel compelled to rebut several substantive statements that she makes. First, the title of Kehoe's article has all the fanfare of a newspaper headline: "Evolutionary Archaeology Challenges the Future of Archaeology." It was our article in *Review of Archaeology* (O'Brien and Lyman 1999a) that spurred Kehoe to action, but that paper was titled "Meeting Theoretical and Methodological Challenges to the Future of Evolutionary Archaeology." Our modest goal was simply "to outline a few of the methods that we view as useful in attempts to retool and thereby allow the future application of Darwinian evolutionary theory to the material record" (p. 15). Our point was this: If evolutionary archaeology is going to make serious strides in providing explanations for the archaeological record being the way it is, there are numerous issues that still must be addressed. Our objective was to be upfront about unsolved problems and to offer a brief discussion of methodological approaches that might prove useful in overcoming those problems. The article was in no way a challenge to the future of archaeology as the title of Kehoe's article suggests.

Second, Kehoe took our article as the latest manifesto from a coordinated group parallel to the "Mafia" of the New Archaeology of the 1960s and the 1860s X-Club of Darwin's England. Although there are archaeologists, ourselves included, who collectively label what they do "evolutionism"—akin to "processualism," "behavioralism," "postmodernism," and other "isms" in archaeology—we cannot recall being summoned to the clubhouse to plot the future direction of archaeology in general. Do we publish? Yes. Do we publish in "major and second-tier journals," as Kehoe labeled them? We try to, as do most archaeologists.

Does this enhance "the appearance of authority"? Not in our opinion. Does publishing in highly rated journals deflect "recourse to other programs and positions," as Kehoe avers? Again, in our opinion, no. We compete with behavioral archaeologists, processual archaeologists, evolutionary ecologists, and archaeologists of every stripe through the peer-review process. If you believe what you have to say is important, then you ought to try to say it in highly regarded journals.

Third, Kehoe indicates that we seek to "garner funds by presenting [our] package as basic science, specifically opposed to competitors' 'unscientific' archaeology." By doing this, in her opinion, we further our chances, along with those of our students, of obtaining "academic posts in a tight job market geared toward grantsmanship." We have never sought grant funds to do evolutionary archaeology and probably never will. A few of our students have been co-authors of papers dealing with evolutionary archaeology, but none has produced a thesis or dissertation that could remotely be labeled evolutionary. Our students have instead performed studies of rock art, mortuary practices, pottery sourcing, ceramic technology, subsistence practices, site structure, and other traditional archaeological subjects that we find valuable to the discipline in general as well as to our own efforts to apply Darwinian evolutionary theory to the archaeological record.

Fourth, Kehoe refers to our reference to A. V. Kidder's (1932) goals for archaeology as "formulaic." We chose Kidder's remarks because they are more detailed than earlier, similar remarks made by other anthropologists. For example, one, if not *the*, goal of Americanist archaeology since before the beginning of the twentieth century was noted nearly a hundred years ago by Franz Boas (1904:521-522), who remarked, "the sequence of types of culture as determined by the artifacts of each period [is among] the fundamental problems with which archaeology is concerned. The results obtained have the most immediate bearing upon the general question of the *evolution* of culture, since the ideal aim of archaeology practically coincides with this general problem, the solution of which would be contained in a knowledge of the chronological development of culture" (emphasis

added). Kidder basically reiterated these goals in his paper of 1932 that we quoted. There is nothing formulaic about it.

Fifth, Kehoe implies that we got our history wrong because we credit Kroeber with invention of frequency seriation. Note that we said *frequency* seriation. Kehoe reports that "Trigger (1989:200) credits . . . Flinders Petrie nearly a generation earlier" with inventing frequency seriation, and she implies that Kidder had something to do with it because Trigger "(1989:188) notes Kidder studied with the Egyptologist George Reisner." Here is what Trigger (1989:202) really said: "Although Kroeber may have learned the basic principles of typology and seriation from Boas and known of Petrie's work, his technique of seriation was not based on the same principles as Petrie's." We have studied this particular aspect of the history of our discipline in some detail (Lyman and O'Brien 1999; Lyman et al. 1997, 1998; O'Brien and Lyman 1999b) and agree completely with Trigger.

Sixth, Kehoe says that we confuse a research program with science itself—a conclusion she draws from our statement that evolutionary archaeology needs a unified body of principles and methods. Notice we did not say a *restricted* body; we said a *unified* body. Kehoe cites Joan Fujimura (1992) that in science methods are used to "instantiate and substantiate" theory, but Kehoe ignores the fact that those methods aren't just *any* old methods but rather a set picked specifically *for* a purpose. Because Darwinian evolutionism is a different *kind* of theory than that used in the physical sciences, it demands a particular epistemology, which in turn demands a particular set of methods. This was one of the points raised in our paper—that the kind of units one uses to measure heritable change over time is an important consideration in any evolutionary study. This point has been made repeatedly by evolutionary biologists, despite their squabbles over particular concepts and methods. The pluralistic concept of species noted by Kehoe is a case in point and attends the critical aspect of analytical units. Numerous species concepts are available (Hull 1997; Mayden 1997), but the important thing is that different concepts work better in different analytical contexts. We suspect these points will be lost on Kehoe given her claim that "it feels scientific

ic" to note that First Nations people in the northwestern Plains and Plateau used twined bags for ritual and medicine objects. Does making an observation turn what one does into science? Is something scientific because it "feels" that way? It either is or it isn't, and no amount of personal feeling is going to have an effect on its status.

Seventh, Kehoe states that evolutionary archaeology "fails to engage fundamental questions," particularly that "non-living artifacts don't have genes." This kind of statement harks back to J. O. Brew's (1946:53) complaint that "phylogenetic relationships do not exist between inanimate objects." Brew was merely stating the obvious; others had found related problems with applying Darwinian evolutionism to the archaeological record, one of the most frequently cited (e.g., Boas 1904; Gladwin 1936; Kroeber 1923; Steward 1941, 1942, 1944) being that biological evolution was diversifying only, whereas cultural evolution could be diversifying and also reticulate. We have considered this criticism in detail elsewhere (O'Brien and Lyman 2000a:134-139) and here merely note that the model of biological evolution as simply and only diversifying is founded largely on vertebrate evolution. Studies of molecular phylogenetics in plants (Endler [1998] and references therein) and bacteria (Doolittle [1999] and references therein) suggest reticulate evolution is much more common than once thought. Irrespective of this, modern paleobiologists (e.g., Eldredge 1989, 1999) conceive of evolutionary change as occurring among an isolated daughter deme or population of a species; if evolution is tracked at this scale rather than at the scale of species, reticulate evolution is not uncommon.

As we noted in our paper, Darwinian evolutionary theory cannot be applied to the archaeological record without considerable work. The same statement can be made about the paleobiological record. Beginning with the work of George Gaylord Simpson (1944) and others in the 1940s, paleobiologists have made significant strides in writing modern Darwinian theory in paleontological terms, much to the consternation of some biologists who study the evolution of living organisms (see Bell [2000] and references therein for recent discussion of this issue). Kehoe's comment that artifacts don't

have genes is, in our view, a red herring. We hope that she is not denying the critically important role of cultural transmission in the evolution of cultural lineages, yet she says that "artifacts . . . do not lie in a relationship of descent," thereby denying the validity of a cultural tradition except as some vague product of human agency. Surely there is a better explanation of the archaeological record than that.

Eighth, Kehoe claims that because A. L. Kroeber and Clyde Kluckhohn "failed to achieve" a "satisfactory definition of 'culture,'" Robert Dunnell's (1971:201) definition of culture as "a concept referring to shared ideas used as an explanation" is not only problematic but "naive." Dunnell's definition, however, attends the fact that culture is transmitted—ideas are *shared*—and also underpins the explanation for such archaeological phenomena as horizons and traditions and why seriation as a basic method works to measure time (O'Brien and Lyman 1999b, 2000a). This underscores our sixth point above regarding the analytical value of concepts defined on the basis of theory. Further, that Kroeber and Kluckhohn failed in their endeavor is debatable. They suggested that culture is a "set of attributes and product of human societies, and therewith of mankind, which are extrasomatic and transmissible by mechanisms other than biological heredity" (Kroeber and Kluckhohn 1952:145). As Marvin Harris (1968:10) noted, this definition "is one step beyond a mere concept of culture; it is rather a theory of culture, namely, an explanation of how the features of a particular population's behavioral repertory are established, i.e., by learning rather than by genetic processes." Such a definition thereby makes culture an analytically useful concept.

Ninth, Kehoe states that "hidden in the dark recesses of O'Brien and Lyman's . . . publications on the history of American archaeology is the dragon figure of Leslie White." We have never "hidden" White's work (Lyman and O'Brien 1997, 1998). And contrary to Kehoe's comments regarding "White's influence on the Dunnell group," we have been quite explicit about our opinion of the nonusefulness of that form of evolutionism for archaeological research (Lyman and O'Brien 1997; O'Brien and Lyman 2000a). Kehoe's efforts to find an

intellectual lineage between White and Dunnell are thus strained because there simply is no such linkage.

Tenth, Kehoe indicates that we "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." Perhaps Kehoe makes this accusation because, as she admits, "I haven't seen all [O'Brien and Lyman's] output." There is no particular reason that Kehoe *should* have read all of our output, but if she had she might realize how unfounded her accusation is. We explicitly acknowledge the contributions to the discipline in general as well as to our own thinking made by Jefferson, McKern, Harold Colton, Gordon Willey, James Ford, Lewis Binford, and a host of other Americanists (Lyman et al. 1997) as well as Europeans such as David Clarke (O'Brien and Lyman 2000a).

Eleventh, Kehoe is correct that "human agency can render a fit artifact extinct," and we find interesting her example of the abandonment of raised fields and the failure of humans to return to using them. This example underscores not only the contingency-bound nature of adaptation but also the fact that humans may not always make the "best choices." Human intent, as we have indicated before, seems of little explanatory value in an ultimate sense, although in a proximate sense it does (Lyman and O'Brien 1998). Yet Kehoe's apparent advocacy of human agency as the *only* mechanism of cultural change—she mentions no others—echoes White's (1943:339) appeal to human "urges" as the "motive force as well as the means of cultural evolution." Is *this* a hidden dragon?

Twelfth, Kehoe claims that when one of our supposed "acolytes (Maschner) changes his mind about [our] research program, [we] pooh-pooh his work." Herbert Maschner, as far as we know, is no one's "acolyte," certainly not ours, and we did not "pooh-pooh" his work. We might disagree with him, but we've had disagreements in print with many other archaeologists—Michael Schiffer and James Boone, for example. This, in our view, is part and parcel of how science works. What Kehoe may not realize is that we have published chapters in books that both Maschner and Schiffer have edited (O'Brien 1996b; O'Brien and Lyman 2000b).

The point is, they knew we viewed some things differently than they did, but they still graciously asked us to participate. This, too, is supposed to be how science works.

In closing, we note that Kehoe cannot even write a simple book review without getting her facts wrong. For example, in reviewing *Evolutionary Archaeology: Theory and Application* (O'Brien 1996a), Kehoe (1997:1540) stated, "This festschrift for Robert Dunnell includes seven of his papers, two by the late David Rindos, two coauthored by editor Michael O'Brien, and one each by Hector Neff, David Braun, and R. D. Leonard and G. T. Jones. All but the essays by Braun and Rindos appear in *Evolutionary Archaeology*, ed. by Patrice Teltser (1995)." There are two problems here. First, the book was in no way, either explicitly or implicitly, a festschrift for Dunnell. Inclusion of his early papers was based strictly on the fact that they were the seminal publications on evolutionary archaeology. Second, not a single paper in that volume appeared in Teltser's earlier book. If someone is going to hold herself up as a critic of an approach, she would gain credibility were she to get her facts correct. □

REFERENCES CITED:

- Bell, M. A. (2000) Bridging the Gap between Population Biology and Paleobiology. *Evolution* 54:1457-1461.
- Boas, F. (1904) The History of Anthropology. *Science* 20:513-524.
- Brew, J. O. (1946) Archaeology of Alkali Ridge, Southeastern Utah. *Peabody Museum of American Archaeology and Ethnology, Papers* 21.
- Doolittle, W. F. (1999) Phylogenetic Classification and the Universal Tree. *Science* 284:2124-2128.
- Dunnell, R. C. (1971) *Systematics in Prehistory*. Free Press, New York.
- Eldredge, N. (1989) *Macroevolutionary Dynamics*. McGraw-Hill, New York.
- _____ (1999) *The Pattern of Evolution*. Freeman, New York.
- Endler, J. A. (1998) The Place of Hybridization in Evolution. *Evolution* 52:640-644.
- Fujimura, J. H. (1992) Crafting Science: Standardized Packages, Boundary Objects, and Translation. In *Science as Practice and Culture*, A. Pickering, ed., pp. 168-211. University of Chicago Press, Chicago.
- Gladwin, H. S. (1936) Editorials: Methodology in the Southwest. *American Antiquity* 1:256-259.
- Harris, M. (1968) *The Rise of Anthropological Theory: A History of Theories of Culture*. Crowell, New York.
- Hull, D. L. (1997) The Ideal Species Concept—and Why We Can't Get It. In, *Species: The Units of Biodiversity*, M. F. Claridge, H. A. Dawah, and M. R. Wilson, eds., pp. 357-380. Chapman and Hall, London.
- Kehoe, A. B. (1997) Review of "Evolutionary Archaeology: Theory and Application," M. J. O'Brien, ed., *Choice* 34:1540.
- _____ (1998) *The Land of Prehistory: A Critical History of American Archaeology*. Routledge, New York.
- _____ (1999) Review of "James A. Ford and the Growth of Americanist Archaeology", by M. J. O'Brien and R. L. Lyman. *Choice* 36:1499.
- Kidder, A. V. (1932) The Artifacts of Pecos. *Papers of the Southwestern Expedition, Phillips Academy* No. 6.
- Kroeber, A. L. (1923) *Anthropology*. Harcourt, Brace, New York.
- Kroeber, A. L., and C. Kluckhohn (1952) Culture: A Critical Review of Concepts and Definitions. *Peabody Museum of American Archaeology and Ethnology, Papers* 47.
- Lyman, R. L., and M. J. O'Brien (1997) The Concept of Evolution in Early Twentieth-Century Americanist Archeology. In, *Rediscovering Darwin: Evolutionary Theory in Archeological Explanation*, C. M. Barton and G. A. Clark, eds., pp. 21-48. *American Anthropological Association, Archeological Papers* No. 7.
- _____ (1998) The Goals of Evolutionary Archaeology: History and Explanation. *Current Anthropology* 39:615-652.
- _____ (1999) Americanist Stratigraphic Excavation and the Measurement of Culture Change. *Journal of Archaeological Method and Theory* 6:55-108.
- Lyman, R. L., M. J. O'Brien, and R. C. Dunnell (1997) *The Rise and Fall of Culture History*. Plenum, New York.
- Lyman, R. L., S. Wolverton, and M. J. O'Brien (1998) Seriation, Superposition, and Interdigitation: A History of Americanist Graphic Depictions of Culture Change. *American Antiquity* 63:239-261.
- Mayden, R. L. (1997) A Hierarchy of Species Concepts: The Denouement in the Saga of the Species Problem. In, *Species: The Units of Biodiversity*, M. F. Claridge, H. A. Dawah, and M. R. Wilson, eds., pp. 381-424. Chapman and Hall, London.
- O'Brien, M. J., ed. (1996a) *Evolutionary Archaeology: Theory and Application*. University of Utah Press, Salt Lake City.
- _____ (1996b) The Historical Development of an Evolutionary Archaeology: A Selectionist Approach. In, *Darwinian Archaeologies*, H. D. G. Maschner, ed., pp. 17-32. Plenum, New York.
- O'Brien, M. J., and R. L. Lyman (1999a) Meeting Theoretical and Methodological Challenges to the Future of Evolutionary Archaeology. *Review of Archaeology* 20(2):14-22.

_____ (1999b) *Seriation, Stratigraphy, and Index Fossils: The Backbone of Archaeological Dating*. Kluwer Academic/Plenum, New York.

_____ (2000a) *Applying Evolutionary Archaeology: A Systematic Approach*. Kluwer Academic/Plenum, New York.

_____ (2000b) Evolutionary Archaeology: Reconstructing and Explaining Historical Lineages. In, *Social Theory in Archaeology*, M. B. Schiffer, ed., pp. 126-142. University of Utah Press, Salt Lake City.

Simpson, G. G. (1944) *Tempo and Mode in Evolution*. Columbia University Press, New York.

Steward, J. H. (1941) Review of "Prehistoric Culture

Units and Their Relationships in Northern Arizona" by H. S. Colton. *American Antiquity* 6:366-367.

_____ (1942) The Direct Historical Approach to Archaeology. *American Antiquity* 7:337-343.

_____ (1944) Re: Archaeological Tools and Jobs. *American Antiquity* 10:99-100.

Teltser, P. A., ed. (1995) *Evolutionary Archaeology: Methodological Issues*. University of Arizona Press, Tucson.

Trigger, B. G. (1989) *A History of Archaeological Thought*. Cambridge University Press, Cambridge.

White, L. A. (1943) Energy and the Evolution of Culture. *American Anthropologist* 45:335-356.