# FOREWORD

..my wife Margaret..suggested that I read a book by Robert Dunnell. However with a title of Systematics in Prehistory (Dunnell 1971), it didn't seem to have much to do with evolutionary theory nor did it seem relevant to my interests in paleolithic prehistory and Pleistocene environments. As I have long since learned, however, I should have heeded her advice. (Barton 1997: iii).

*Systematics in Prehistory* was written over thirty years ago by someone only recently out of graduate school and completely naive to the ways of academia. As one of my new colleagues at the University of Washington put it: A hardbound archaeology book what a novel concept. Indeed, books about archaeology, rather than the archaeology of particular places, were rather novel. Taylor's *A Study of Archaeology* (1948) was pretty much an isolated analytic effort (one that got its author ostracized from mainline professional circles for decades). Watson, LeBlanc, and Redman's *Explanation in Archeology: An Explicitly Scientific Approach* (1971) was written at the same time as *Systematics* and without knowledge of *Systematics* or vice versa. While it claimed linkage to Taylor's work, this claim is best understood as a legitimizing myth rather than any intellectual connection given the radical differences in approach. *Explanation in Archeology* (EA) and *Systematics* differed in most other respects. EA advocated, in fact largely anticipated, a particular approach to archaeology that the authors dubbed “scientific.” *Systematics*, on the other hand, was an effort to explain archaeology, as it then existed. And unlike the Taylor book a generation earlier, *Systematics* focused on trying to account for why archaeology actually “worked” in those few areas where it did work demonstrably rather than how it failed to meet new expectations anthropology in the Taylor case and “science” in the EA case. Furthermore, *Systematics* took as its target the “language of observation,” the generation of “kinds,” of “facts,” a “bottom up” sort of approach to understanding archaeology. The others took a rather different, top-down approach proceeding from assumptions about what answers should look like and thus focused on the “language of explanation.” And for the most part, the language of explanation was drawn then, as now, from Western common sense in archaeology. As a result EA went on to become the textbook for the “new archaeology” (later and more distinctively, processual archaeology). Even the Taylor book enjoyed some belated success in that role. *Systematics*, on the other hand, while it did well enough in the bookstores, remained something of an enigma as suggested by Barton's initial reaction related in the quotation, not embraceable by the new archaeology and clearly not conformable with culture history that was the rhetorical target of processualism. It was regarded as difficult to read, hard to understand, and/or of doubtful practical utility. People were not yet ready to question the “objectivity” of science nor the “reality” of “facts.” It was in many respects frightening and still is.

Yet the argument that *Systematics* makes is a simple one, germane to all science. Investigators must separate the categories they use to make observations from “natural kinds,” or empirical kinds, if such can even be said to “exist.” We cannot know the world apart from the templates we use to observe it, a view that even Taylor would come to embrace (1968). The only choice we have is to be aware of the templates and therefore their effects on “facts” or not. When we opt for the latter, the claim is always one of natural kinds, that x is so “because it is real.” This is precisely the rationale for folk kinds as well. On the other hand, if the existence of observational templates is admitted, then and only then does the possibility of controlling their effects arise. Only when we know the template used can the artifacts of observation be separated from the phenomenological world. Theory, specifically what *Systematics* calls “formal theory,” exists to do this work, explicitly creating a language of observation to generate “facts” that can be explained by the processes and mechanisms of a particular theory. This lies at the heart of the “group”/”class” distinction that is the core of *Systematics*: Groups are sets of things; classes are categories for things. The bulk of *Systematics* is a review of the means available for the construction of categories, the choices made by archaeologists when successful, and even anticipating the quantitative course that archaeology would pursue later in the 1970s and 1980s. The key point is, however, that the purpose, the function of classes is to create groups that can be explained, that are meaningful within some theory. As Richard Lewontin (1974: 8) explained to biologists shortly after *Systematics* was written: “We cannot go out and describe the world in any old way we please and then sit back and demand that an explanatory and predictive theory be built on that description.” That he needed to make this point to zoologists is telling. Scientists, real scientists, get trapped by the objectivity myth from time to time, even though their daily practice and disciplinary history belie it.

Failure to recognize this point has led archaeology into a fatal spiral. *Ad hoc* categories were drawn from English common sense. Archaeology was thus compelled to adopt a reconstructionist methodology that conceived the archaeological record in everyday terms. Common sense, our own culture's implicit values and conventions, could then explain it. Reconstitution required creating “attributes” that were not empirical, and archaeology lost its testability. Testing was replaced by examination of how a conclusion was reached. Quantification, as foreseen in *Systematics*, came to be used to create qualities instead of describing variability. Not surprisingly processualism failed.

The gap left by the collapse of processualism as science was filled not by a scientific archaeology but the new relativism sweeping social science (for similar reasons). Yet almost ironically postprocessualist archaeology, as this newest archaeology is commonly known, is likewise tethered to the core thesis of *Systematics*. In attacking the “objectivity” of science, postprocessualists could have drawn heavily on *Systematics* to demonstrate their point. *Systematics* is nothing if not a debunking of the “objectivity” of science, all science, by demonstrating that its “facts” are constructions. The crucial distinction is not between objective and subjective, however, but between explicit and implicit formulation. Consequently postprocessualists miss the methodological point of *Systematics*: Recognizing the constructed nature of kind allows one to *control* the construction, to be able sort observation into artifact and “reality.” Rather they glory in being victims of their own unremarked delusions.

Perhaps the impact of *Systematics* would have been more far reaching earlier had it not come out of archaeology. Archaeology is not exactly a place a biologist or a chemist might look for insight into the working of science, especially in the early 1970s. Certainly science has moved in the directions foreseen by *Systematics* since then, even if the social sciences have not, yet everything from the use of cladistics to the “science wars” points out the need to take up the issues addressed in *Systematics* anew.

Looking back, *Systematics*’ greatest failure lies in its abstractness. This is not to fault what is there or how it was presented, but to lament that it was not firmly articulated with a particular explanatory theory. Consequently while one discusses how attributes are generated or types formed, there was no content to allow one to actually select attributes or form types. But then there was no explicit explanatory theory in archaeology at mid-century. Language of observation decisions were entirely implicit. Culture history did employ some elements of explanatory theory, but they were hardly explicit. Indeed, I ended up spending a goodly portion of my life trying to extract them from the descriptive literature. New archaeologists openly questioned if theory were necessary, passing that buck conveniently to anthropologists. My own commitment to evolutionary theory lay years in the future, contra to the implications of the opening quotation. Yet in retrospect I could never have come to evolutionary theory without first having understood unit formation. It may be that the resistance to evolution lies as much in failure to internalize the central thesis of*Systematics* as it does with any of the more usual excuses.

*Systematics* is not an easy “read” (to use the modern jargon) but that is only partly my fault. I have to own up to not having controlled the analytic tradition that did exist for such endeavors, but then my discipline did not encourage such analyses. Anthropological graduate curricula did not include these tools nor guide the archaeological apprentice in that direction-a deficit that I resent to this day and one that I attempted to correct in my own thirty-year teaching career. But many of the distinctions and concepts with which *Systematics* toyed were new, at least in the web in which they were presented; there was, and still is to a large degree, no well-oiled path to be shared by analyst and reader when building languages of observation. Finally, challenging the “objective” “existence” of “facts” that can be known through application of “correct” procedure has always been, and remains, a difficult business, crucial as it may be not only to science but to people. It is almost as if people do not want to own up to being sentient creatures. This is what I suspect we mean when we say something is intrinsically hard to understand—something that questions the very tools we use to know. And this is really what *Systematics* is all about.

Barton, C. Michael

1997 Preface. In *Rediscovering Darwin: Evolutionary Theory and Archaeological Explanation*, edited by C. M. Barton and G.F. Clark, pp. iii-v. Archeological Papers of the American Anthropological Association, No.7. Arlington.

Lewontin, R. C.

1974 *The Genetic Basis of Evolutionary Change*. Columbia University Press, New York.

Taylor, Walter W.

1948 *A Study of Archaeology*. Memoir of the American Anthropological Association No 69. Menasha.

1968 The sharing criterion and the concept culture. In *American Historical Anthropology, Essays in Honor of Leslie Spier*, edited by C. L. Riley and W. W. Taylor, pp. 221-230. Southern Illinois University Press, Carbondale.

Watson, Patty J., S. A. LeBlanc and C. L. Redman

1971 Explanation in Archeology: an Explicitly Scientific Approach. Columbia University Press, New York.