For over a century, Americanist anthropologists have argued about whether their discipline is a historical one or a scientific one. Proponents of anthropology as history have claimed that the lineages of human cultures are made up of unique events that cannot be generalized into laws. If no laws can be drawn, then anthropology cannot be a science. Proponents of anthropology as science have claimed that there indeed are laws that govern humans and their behaviors and cultures, and these laws can be discovered. Interestingly, both sides have the same narrow view of what science is. The same sorts of debates over science and history were played out in evolutionary biology over a half-century ago, and what emerged was the view that that discipline and its sister discipline, paleontology, were both history and science—hence the term “historical sciences.” Anthropology and its sister discipline, archaeology, have only recently begun to realize that they too are historical sciences. © 2004 Wiley Periodicals, Inc.

In 1989, Stephen Jay Gould (p. 51) observed, “Science has been slow to admit the different explanatory world of history into its domain—and our interpretations have been impoverished by this omission. Science has also tended to denigrate history ... by regarding any invocation of contingency as less elegant or less meaningful than explanations based directly on timeless ‘laws of nature.’” During the twentieth century, anthropologists trained in North America—what we term Americanist anthropologists, including archaeologists—have argued with some regularity about whether their discipline is a historical one or a scientific one (Boas, 1936; Kroeber, 1935; Trigger, 1973; Watson, 1973a). The disagreement typically begins with a consideration of the differences between history and science. Rather than defining each in terms of its necessary and sufficient conditions, most discussants characterize science as generalizing and involving laws and characterize history as particularistic and having no laws. Depending on the personal whim of the author—whether or not he or she believes in the existence of laws that govern humans and their behaviors—anthropology is then declared to be either science or history. Two terms—“nomothetic” and “idiographic” often crop up in these discussions. “Nomothetic” was coined in 1894 by German philosopher Wilhelm Windelband to signify “those disciplines seeking to formulate general laws” (Carneiro, 2000, p. 149). In the same context he also coined “idiographic” to signify the ontology that events are “unique and specific, and therefore ungeneralizable” (Carneiro, 2000, p. 149).

We are Americanist archaeologists with deep interests in (1) the history of our discipline and (2) the adaptation of Darwinian evolutionary theory to archaeological problems. We were trained in the late 1960s and 1970s, during the rise of an archaeological paradigm known as Cladistics and Archaeology through the University of Utah Press.
processual archaeology. Processual archaeologists proclaimed themselves to be scientific and to be replacing a preceding paradigm in archaeology known as culture history (e.g., Binford, 1968; Flannery, 1967). Our take on the history of Americanist archaeology (e.g., Lyman & O’Brien, 2003a; Lyman, O’Brien, & Dunnell, 1997) leads us to explore a single aspect of the polemic attending the emergence of processual archaeology. That polemic involved highlighting the contrast between idiographic history and nomothetic science in an effort to justify the claim that processual archaeology had scientific and therefore loftier (nomothetic) analytical goals than its predecessor, (idiographic) culture history. In this article, we explore the history of the contrast as discussed by select twentieth-century anthropologists who had a major intellectual influence on the architects of processual archaeology.

To provide an analytical context for the rhetoric of the processual archaeologists, we first consider the field of evolutionary biology, where there has been considerable discussion over the past half-century as to whether it is a historical or a scientific discipline. Our brief consideration allows us to distinguish between two ontological viewpoints—a distinction that is critical to understanding how and why disciplines such as evolutionary biology and anthropology can simultaneously be historical and scientific. Some of the views of Franz Boas—arguably the founder of American anthropology—around the turn of the twentieth century serve as an introduction to our discussion of the views of subsequent anthropologists on what they took to be the differences between history and science. We focus on A. L. Kroeber, a student of Boas’s who was the leading anthropologist in the country between 1930 and 1950, and Leslie A. White, whose ideas strongly influenced the architects of processual archaeology. Next, we consider what some Americanist archaeologists thought about science and history as the processual archaeology of the 1960s and 1970s was emerging. We conclude with a brief discussion of anthropology as a historical science that is theoretically informed.

**Evolutionary Biology as Historical Science**

Evolutionary biology and its sister discipline, paleontology, are historical pursuits because, at a minimum, they trace stability and change within organisms over spans of time. In arguing that paleontology (and by extension biology) was not only historical but scientific as well, George Gaylord Simpson drew an important distinction between what he termed “immanence” and “configuration”:

The unchanging properties of matter and energy and the likewise unchanging processes and principles arising therefrom are *immanent* in the material universe. They are non-historical, even though they occur and act in the course of history. The actual state of the universe or any part of it at a given time, its configuration, is not immanent and is constantly changing. It is *contingent* ... or *configurational*... History may be defined as configurational change through time, i.e., a sequence of real, individual, but interrelated events. (Simpson, 1963a, pp. 24–25)

What Simpson was getting at is this: Various chemical and physical processes are immanent and thus produce predictable outcomes, but the particular spatiotemporal and geological contexts in which they are active produce unique, unpredictable configurations. The critical adjectives here are *particular* and *unique*: “Configurations are the unique expression of particular combinations of immanent processes in operation in more or less unique sequences at particular intensities on particular phenomena” (Wolverton & Lyman, 2000, p. 234). Each biological organism is the result of immanent processes such as genetic transmission and ontogenetic development of the phenotype, but it is also the result of its genetic lin-
gage’s “history, the configurational sequence by which [the organism] arose” (Simpson, 1963a, p. 27). Genetic mutation will occur—it is immanent—but which gene will mutate is unpredictable, as is the form the mutation will take and whether the selective environment in which it occurs will be favorable or not. These are all configurational properties.

Richard Watson (1966, p. 173), a philosopher of geology, found Simpson’s argument to be a “false thesis.” In Watson’s view, the root of the problem resided in Simpson’s confusion of a particular historical event such as a chemical reaction or an erosional event with a “type” of event such as a kind of chemical reaction or the process of erosion: “[T]ypes are abstractions and as such [are] part of our conceptual equipment, but as types [they are] not part of the actual world of events.... [P]articular events have specific coordinates in time and space, and as such are part of the actual world.... Types of events obviously neither occur nor recur, and particular events as such obviously cannot recur” (Watson, 1966, p. 175). Watson was correct on this point: There is a world of difference between kinds, which are ideational units (Dunnell, 1986; Lyman & O’Brien, 2002; O’Brien & Lyman, 2002a), and the individual things placed in them. Those “things,” regardless of whether they are objects or events, are empirical units. Thus erosion is an ideational unit, and a particular erosional event is an empirical unit. David Hull made the same distinction using the terms “individuals” and “classes”:

A unique event is one that happens to be one of a kind. A necessarily unique event is defined in terms that preclude any other instance of this event. The usual way of defining an event so that it is necessarily unique is by specifying particular spatiotemporal coordinates for it. No two objects can occupy the same place at the same time.... [S]cientific laws cannot refer to specific individuals, only to classes of individuals. (1974, pp. 47–48)

Watson (1966, p. 177) argued that sciences are amenable to laboratory experimentation and “concern subject matters ideally adjusted to investigation by man.” This possibility exists because sciences such as chemistry use general types. In contrast, history cannot do experiments in a lab because it employs very specific types. Laws can be written, but only if the specifics of a particular event are included in the law. The “doctrine of uniformitarianism implies only that the basic laws remain the same, and leaves open the possibility that given different values for the variables in those laws, [things] can change” (Watson, 1966, p. 181). In Watson’s view, laws with the values of their included variables unspecified underpin science, whereas laws with their values tightly specified underpin history. Therefore, Simpson’s distinction between immanence and configuration and the distinction between history and science are unnecessary and false.

Simpson (1970) was not impressed with Watson’s comments. History was important to Simpson, and to ignore its particularities was inconceivable. In Simpson’s view, Watson erred because he failed to realize that laws concern immanence whereas history concerns configuration. Watson (1966, p. 178) had argued that the evolution of Homo sapiens was a unique historical event only if we “deny the possibility of a similar evolution” at another time or place. Surely, Watson contended, there are laws that, if written in specific terms, would allow us to predict such an evolutionary event. Simpson (1970, p. 87) responded that certainly there are laws that govern such things as mutation and selection, but “it is surely obvious that those laws do not explain the actual [appearance and] structure of the [creature]. Those are the interests of the historical scientist, and the needed explanation is historical,” or configurational, rather than immanent.

Several decades after the Simpson–Watson exchange, Marc Ereshefsky (1992) examined evolutionary biology specifically to identify if and how science and history differ. He
found that in the case of evolutionism, which he took to be both a historical and a scientific discipline, they do not differ in the sense that both depend on ‘laws,’ ‘generalizations’ or whatever,” including what are labeled theories (Ereshefsky, 1992, p. 86). As Elliott Sober (1993, p. 15) later put it, historical scientists use laws, theories, and generalizations “only as a means” of figuring out history, whereas nomothetic disciplines have as a “goal ... to infer general laws; descriptions of particular objects are relevant only as a means.” Thus, in this respect Watson and Simpson were talking past one another because each envisioned a distinct goal for, in this case, geology and evolutionary biology. On the one hand, Simpson clearly held history to be the more important, with laws, generalizations, and theories—all of which fall under immanence—being used in the service of writing history. Watson, on the other hand, held law generation to be paramount: “Simpson is wrong to say that geologists should not look for laws” (Watson, 1969, p. 488). This is not at all what Simpson (1963a) said; it is abundantly clear that he was well aware of the value of laws to geologists: “Science, truly to be such, must center not on descriptions and names but on principles—that is, generalizations, theories, relationships, interconnections, explanations about and among the facts” (Simpson, 1963b, p. 82).

The modern theory of biological evolution provides a suite of statements about law-like processes or mechanisms that produce history. The processes include genetic transmission, which results in spatiotemporal continuity manifest as both formal and hereditary continuity; mutation, selection, and drift manifest variously as lack of formal continuity; and convergence and parallelism, which produce formal similarity without hereditary continuity. A phylogeny is the historical product of these processes; evolutionary theory “characterizes the [historical evolutionary] process[es]” (Hull, 1974, p. 50). Evolutionary biologists seek to find a level of generality that allows them to write laws of evolution that are sufficiently specific to permit inferences. If the laws are too precise, they become so particularistic as to not be generalities; if they are too general, they grant few insights into history.

Ereshefsky (1992, p. 85) noted that historically unique evolutionary events among biological phenomena can be made to recur analytically if they are “described” in the same way. We would say that they can be made to recur analytically if they are classified as members of the same kind of phenomena (Lyman & O’Brien, 2002; O’Brien & Lyman, 2002a). Ereshefsky (1992, pp. 90, 91) found that the difference between biological evolution and other sciences is in the “transmission” of (genetic) information “with great fidelity” from ancestral to descendant generations, which forms a unique “spatiotemporal connection” between succeeding generations. This provides a unique, historical explanation for the similarity of biological phenomena because “no such explanation is available for the similarities found among the units of physics and chemistry” (Ereshefsky, 1992, p. 91). This contrast has been variously labeled the difference between population thinking and typological thinking (Mayr, 1959) and the ontological difference between essentialism and materialism (Dunnell, 1982; Lewontin, 1974a; Sober, 1980).

Aristotle sought the underlying essence—the essential characteristics—of phenomena, an ontology known today as essentialism (Sober, 1980). It influenced all classifications until Charles Darwin proposed an alternative, today termed materialism (Lewontin, 1974a). Darwin focused on the uniqueness of phenomena and thus, although the basic form of individuals within a set of similar phenomena can be captured by, say, a statistical average or type definition, such measures of central tendency are abstractions and in no sense real. Essentialism holds that types are real and fixed and that variation between particular phenomena within a kind has no analytical importance. Thus essentialism is advantageous when we want to formulate laws about how kinds of things interact. Those things and their interac-
tions, regardless of their positions in time and space, will always be the same because the essential properties of the things are the same.

Most interestingly in this context, Ereshefsky (1992, pp. 91, 92) argued that the uniqueness of information transmission between entities and the resulting spatio-temporal connection between them sets biological evolution apart from geology as a historical science, and, in fact, “the only science which parallels evolutionary theory in these respects is human history.” That is, the cultural transmission of information from one individual to another forms a spatio-temporal connection among hominids. That information can change (just as a gene pool does) over time. A connection via transmission, whether genetic or cultural, can be termed “heritable continuity” (O’Brien & Lyman, 1999, 2000, 2002b). How such continuity is effected in turn dictates the processes and variables that mediate the continuity—that is, cultural transmission and genetic transmission are not the same and demand different models (e.g., Boyd & Richerson, 1985). It ultimately is the mediation of heritable continuity and its resultant empirical expression that gives evolution, regardless of the phenomena evolving, its characteristic of historical contingency (Beatty, 1995).

This characteristic has led to discussion by philosophers (e.g., Brandon, 1997; Cooper, 1996) about whether there are in fact biological laws, the intention being to determine if evolutionary theory in particular is scientific. Further, because evolutionary biology rests on very specific kinds of methods and cannot predict the future, the status of the discipline as scientific has often been questioned, prompting various individuals to argue that the discipline is in fact a science but that it is a different kind of science than the archetypical, law-based physics and chemistry (e.g., Cooper, 2002; Gould, 1986, 1989; Lewontin, 1974a; Moore, 2002). We agree with these arguments and note that predictive capabilities are lost in evolutionary biology because the lack of correlation between mutations and selective environments—both of which themselves are unpredictable—demands that organisms “tinker” (Jacob, 1977) with what is available. This leads to a broad range of adequate solutions rather than to a limited range of optimal (and thus closely predictable) ones (Gans, 1993). This in turn gives the particular historical details of connected lineages of organisms—both the processes and the organisms themselves—their unpredictable (although historically contingent) character, just as it does any spatio-temporally connected set of phenomena that are causally interrelated. These are the general explanatory principles, if not “laws with specific details,” that allow us to explain the historical past but do not allow us to predict the future (Scriven, 1959). But even retrodiction can be imperfect for the simple reason that it rests on accurate construction of chronology and accurate reconstruction of history.

Robert O’Hara (1988) presented a clear statement of the distinction between a temporal sequence and history. He labeled the former a “chronicle” (1988, p. 144) and described it as a chronological order of descriptions of events without any “causal statements, explanations, or interpretations.” In contrast, what he called a “history” is the same as a chronicle except that it includes “statements about causal connections, explanations, or interpretations.” Thus a chronicle would read simply as event A happened before B, and B was followed by C. A history would say something like A happened and caused B to happen, and B in turn caused C to occur. In O’Hara’s view, historical explanations are statements about why chronicles are the way they are. In our view, when we produce historical explanations, we are doing science.

But a chronicle may be incorrect and lead to incorrect history. Conversely, even if a chronicle is correct, the history may be incorrect. As a result, O’Hara (1988, pp. 149–150) distinguished between “how possibly” and “how actually” explanations, characterizing the first as statements regarding “how a change may have taken place” and the latter as how a change “did take place.” His point was that the former are acceptable in retrodictive histori-
cal disciplines such as evolutionary biology because they are testable in light of new evidence. Thus, Robert Brandon (1990) argued that “how possibly” explanations are quite valuable to theory-rich historical sciences such as Darwinian evolution. When a “how possibly” explanation accounts for numerous observations and provides an empirically and logically coherent explanation, it attains the status of a “how actually” explanation yet remains testable. Brandon acknowledged that we may never know when we have truly attained the latter, although “no one can fairly describe [a theoretically informed ‘how possibly’ explanation] as merely an imaginative bit of story telling” (Brandon, 1990, p. 183). Importantly, even what might be thought of as historical experiments (e.g., Hammel, 1979) produce explanations of the “how possibly” sort.

The fact that historical explanations are testable is another indication that historians can do science. The tests do not, however, take place in laboratories. Historical methods are different from those of the archetypical sciences, particularly those dependent on experiments whose conditions can be manipulated and controlled. Experimental sciences test the predictions (implications) of hypotheses by exploring suspected cause–effect relations. A laboratory setting allows contingencies to be controlled, which safeguards against type I (rejection of a true hypothesis as false) and type II (acceptance of a false hypothesis as true) errors. In contrast, because history studies the past, its methods focus on effect–cause relations. That is, the data of historical disciplines are traces (effects) of past causes (Cooper, 2002; Gould, 1986; Moore, 2002). Multiple causes may be hypothesized, but the one that accounts for the most effects and the most kinds of effects tends to be the favored explanation (Cleland, 2002). Thus effect A’ may suggest a cause, A; a historical scientist then searches for effects A”, A”’, and so on as tests of the validity of A as the cause. Prediction is not possible, but retrodictive explanation of past events is possible.

In summary, historical science as practiced in biology and paleontology encompasses a theory of change resting on a materialist ontology; on mechanisms of change such as genetic transmission, differential replication, and mutation; on units of analysis such as species, genes, and lineages; and on empirical implications. In the following sections we show that one or more of these elements is missing from almost all discussions of history and science offered by Americanist anthropologists.

Boasian Views

The distinction between history and science may reside in Aristotelian logic (Bock, 1952), but in North American anthropology, the distinction grew to the form that concerns us here during the fourth quarter of the nineteenth century. Beginning in about 1880, a favored view of historians was that scientific history “consisted of a search for facts alone, with no laws or generalizations”—a strong allegiance to “objectivity” was the hallmark of science (Holt, 1940, pp. 357–358). This meant that one took no preconceived notions into historical research. Instead, “the facts, thus scientifically established without benefit of hypothesis or of generalization, would ‘speak for themselves’” (Holt, 1940, p. 358). This view of scientific history “dominated scholars both in Europe and in America” from about 1880 until the 1930s (Holt, 1940, p. 358). Such a description fits the work of anthropologist Franz Boas.

Originally trained in Germany as a geographer at the same time that Windelband was working, Boas brought a particular view of the sciences to anthropology. Whereas the natural sciences were deductive, generalizing, and law-based, the human sciences focused on the mind and were particularistic and inductive (Lewis, 2001; Stocking, 1965). Boas was well aware of Windelband’s distinction between nomothetic and idiographic disciplines. In an
early paper, Boas (1887) evaluated geography in terms of the distinction between particularistic disciplines and generalizing ones capable of generating laws. He originally thought laws of cultural evolution might eventually be written (Boas, 1896), but near the end of his career he had grave doubts about such a possibility, writing that “cultural phenomena are of such complexity that it seems to me doubtful whether valid cultural laws can be found” (Boas, 1932, p. 612). In his latter view, anthropology must be “a historical science, one of the sciences the interest of which centers in the attempt to understand the individual phenomena rather than in the establishment of general laws” and even if the latter could be established they would be “of little help to a real understanding” (Boas, 1932, p. 612). Thus he held that anthropology was (and should be) primarily particularistic (Buettner-Janusch, 1957; Kroeber, 1956; Wax, 1956). If it had a generalizing aspect to it at all, it was in terms of identifying a commonality across the mentalistic underpinnings of individual cultures—the Enlightenment notion that became known as psychic unity (see Shore, 1996, for review).

Not surprisingly, then, Boas (1904, p. 514) drew a distinction within anthropology between what he called “two distinct methods of research and aims of investigation: the one, the historical method, which endeavors to reconstruct the actual history of mankind; the other, the generalizing method, which attempts to establish the laws of its development.” He noted that what early in the development of anthropology began as the historical method eventually “interpreted” individual cultures in terms of what was perceived as “a change from the less valuable to the more valuable,” with “the present serving as the standard for comparison” (Boas, 1904, p. 515). Thus late nineteenth-century anthropology took on what Boas (1904, p. 515) called “an ill-conceived teleological tinge.” That is, the development of the social sciences at the hands of Edward Burnett Tylor, Lewis Henry Morgan, Herbert Spencer, and others was geared toward building a kind of study of humans that was distinct from history. Social scientists sought generalizations whereas historians kept their focus on the unique, the particular, and the specific (Bock, 1952).

In Boas’s (1904) view, the historical paradigm of the late nineteenth century was converted into the generalizing method at the hands of the social scientists. Now, particular historical events were important only in their service to the search for general laws and as exemplars of those laws. One result of this conversion that Boas could not accept was that “the regularity in the processes [products would be an equally good word] of evolution became the center of attraction even before the processes of evolution had been observed and understood” (Boas, 1904, p. 516). Under the generalizing method, regularity was manifest as “one type of evolution from a primitive form to the highest civilization” that was “applicable to the whole of mankind” (Boas, 1904, p. 516). This was not only Boas’s view:

The emerging social science disciplines of the nineteenth century were dominated by the conviction that a search for regularity in human affairs must go behind events in the experiences of peoples and find the forms, forces, or functional requirements that determined events and made things happen as they did.... The point of departure of nineteenth century anthropologists and sociologists alike was the rejection of the data and procedures of traditional historians, or at least the drawing of a careful distinction between historical and “scientific” investigation. (Bock, 1952, pp. 489–490)

Those subscribing to the notion of universal cultural evolution did not need to know the particularistic historical processes and events behind the development of any particular culture because the psychic unity of mankind was assumed to have directed how each culture responded to a necessity. As Boas (1896, p. 903) remarked, “the sameness of ethnological phenomena found in diverse regions [was taken as] proof that the human mind obeys the same
laws everywhere.” Boas (1896, p. 903) did not accept this generalization and noted in the next sentence that “if different historical developments could lead to the same results ... then this generalization would not be tenable.” That is, a test of universal evolution could be made with particularistic historical data, and as Boas argued throughout his career, virtually every test showed the generalization to be untenable.

The fact that culture was transmitted over time and across space provided the warrant for the test implications and “induced investigators to trace the distribution and history of customs and beliefs with care so as to ascertain empirically whether they are spontaneous creations or whether they are borrowed and adapted” (Boas, 1904, p. 522). The research program that became known as “historical ethnology” (Goldenweiser, 1925) or “historical particularism” (Harris, 1968) emerged from Boas’s teachings. Boas eventually abandoned the particularistic historical ethnology he spawned in favor of a more psychological anthropology, but, with few exceptions, his students and others sought to develop it into a robust research program. Historical ethnologists took on virtually any competitor, as Robert Lowie (1912, 1918) did against the vestiges of universal cultural evolutionism and Alexander Goldenweiser (1916; see also Boas, 1911) did against diffusionism. Although there were problems with the basic method of historical ethnology (Radin, 1933), anthropologists continued to try to work out the history of the particular culture(s) they studied and to improve various analytical techniques (e.g., Clements, Schenck, & Brown, 1926; Steward, 1929).

Writing at the close of the Boasian period, Clyde Kluckhohn (1939, 1940) argued that anthropology could certainly be historical—all it had to do was to continue doing what it had been doing throughout much of the twentieth century—but if it were to be scientific anthropology had to develop its own explicit explanatory theory. Two anthropologists who dominated much of the anthropology of the early and middle twentieth century argued about this at some length—an argument not simply about how to make anthropology scientific but about whether such a thing was even possible. The fallout from that debate between A.L. Kroeber and Leslie White would set much of the course of anthropology throughout the twentieth century.

### Anthropology after Boas

Kroeber, an early student of Boas’s at Columbia, wrote that the “methodological requirements of history” include “continuity (with context as corollary) and uniqueness” (Kroeber, 1935, p. 542). He viewed uniqueness as a necessary characteristic of history because historians—whether historians per se or anthropologists doing historical ethnology—practice “an extreme caution of generalizations savoring of the universal” (Kroeber, 1935, p. 544). This was a direct link to Boas’s (1896, 1904) earlier-stated dichotomy between particularistic history and generalizing science. Kroeber (1935) argued that science is an experimental discipline that takes place in the laboratory and seeks universal processes that typically are reduced to “physico-chemical explanations.” In contrast, Kroeber (1935, p. 546) believed that the “historical approach” might also call on those same universal processes, but, “given the nature of things,” it cannot similarly reduce explanations of “the field of human societies” to physico-chemical processes. The following year Kroeber (1936/1952a, p. 69) identified what he termed the “essential qualities of the genuine scientific approach.” It was a means to understand how the world works; it was empirical; and it classified empirical phenomena in order to build abstract concepts, laws, and the like. Experimentation is not, in Kroeber’s view, a “fundamental characteristic” (p. 69) of science; the “end-results [of science] are timeless and spaceless formulations, in the sense that they are independent of specific or
particular time and place” (p. 70). History, on the other hand, retains the spatio-temporal co-
ordinates of its subject phenomena and although it “conceptualizes, it does not wholly ab-
stract” (p. 71). Establishment of chronology is “not the fundamental feature of the historic ap-
proach” but rather the “reconstructing” of phenomena or events “into significant conceptual 
relations and groupings” (p. 71). The last in turn reveals, inductively, the processes and causes 
of historical change and can eventually lead to generalizations. Thus not only is history nomo-
thetic but history and science are two sides of the same coin (Kroeber, 1952b).

But history is also idiographic, as Kroeber well knew, and he attempted to synthesize the 
 particulars and to produce generalizations based on patterns, although he admitted he had lit-
tle success and had found no “true law in the [cultural] phenomena dealt with” (Kroeber, 
1944, p. 81). One who agreed with Kroeber’s self-evaluation was Leslie White (1946), an-
other leading American ethnologist of the period. The two men differed considerably on the 
matters of history, science, and evolution. For example, in contrast to Kroeber, White held that 
history and science were separate and distinct and had minimal overlap. Both Kroeber and 
White viewed cultural change as involving transmission and heritable continuity and referred 
to the history of a culture as a continuum. Further, they agreed that events varied formally and 
each occupied a particular position in time and space. But White and Kroeber viewed history 
and evolution differently. The manner in which the form of an event and its spatio-temporal 
coordinates are analytically interrelated were the criteria by which White distinguished be-
tween history and evolution. White (1938, p. 375) noted that “events are related to each other 
spatially, and we may deal with [them] in terms of spatial, or formal, relationships, ignoring 
the aspect time” (see also White, 1945, p. 222). A form of event could be “repetitive” (White, 
1945, p. 229), meaning that events could be classified as members of a type based on shared 
formal features and despite each one occupying a unique time–space position. In White’s 
(1945, p. 222) view, history concerns “non-repetitive” events and deals with events strictly “in 
terms of their temporal relationships alone. Each event is unique. The one thing that history 
ever does is repeat itself” (White, 1938, p. 374). Thus, the “temporal process [is] a chrono-
logical sequence of unique events, the study of which is history” (White, 1945, p. 222).

Whereas the “historic process is merely temporal, the evolutionary process is formal as 
well: it is a temporal-sequence-of-forms” (White, 1938, p. 379). The evolutionary process in-
volves “new forms grow[ing] out of preceding forms” (White, 1938, p. 380; see also White, 
1945, p. 224). Although “the historic process and the evolutionist process are alike in that 
both involve temporal sequences[, they differ] in that the historic process deals with events 
determined by specific time and space coordinates, in short with unique [empirical] events, 
whereas the evolutionist process is concerned with classes [types] of events independent of 
specific time and place” (White, 1945, p. 230). Recall Richard Watson’s (1966) comments 
cited earlier.

Kroeber could not understand White’s distinction between history and evolution because 
to Kroeber, history and evolution were one and the same (see also Bidney, 1946). For Kroeber 
(1935, p. 545), “the distinctive feature of the historical approach in any field [is] not the deal-
ing with time sequences ... but an endeavor at descriptive integration.” Integration signifies an 
effort to identify cultural “processes” or mechanisms prompting stability and change over time 
(Kroeber, 1935, p. 546). In Kroeber’s (1935, p. 567) view, good historians “concern themselves 
with finding patterns and putting them into their actual relations essentially on the phenome-
nal level.” Kroeber viewed historical research as tracing diachronic or synchronic relations be-
tween phenomena. The former consisted of determining sequences of events or elucidating an 
event’s historical context; the latter involved determining the interrelationships of phenomena 
at one point in time and in anthropology involved placing a phenomenon in its cultural con-
text. In Kroeber’s view, science studies processes whereas history studies patterns; in anthropology, a cultural pattern is the metaphorical glue—“those arrangements or systems of internal relationship which give to any culture its coherence or plan”—that holds a set of cultural traits together, making them more than a “mere accumulation of random bits” (Kroeber, 1948, p. 311). Relative to the “inorganic exact sciences,” the historical sciences demand a “fundamentally and qualitatively different type of interpretation” (Kroeber, 1935, p. 545). History involves “interpretation” and is “concerned with functional relations” (Kroeber, 1946, p. 3). Further, “real history is nothing but an interpretation by means of description in terms of context” (1946, p. 4), and “historical validation per se is achieved in terms of accord or fit with increasingly larger context” (1946, p. 11). The term “context” signifies the spatio-temporal coordinates of an event and the spatio-temporal and functional or causal relations between events.

Kroeber (1946, p. 11) noted that the varied contexts of events prevent “the historical approach as such from ever attaining to ‘laws,’ to general theory, to exactness of measurable findings, and to genuine verifiability, as by experiment.” The contexts give “historical findings their quality of uniqueness, their individuation,” although through close study, one can “recognize patterns” across historical phenomena (Kroeber, 1946, p. 11). Kroeber (1946, p. 11) contrasted history with science as follows: “Science ... abandons most of what history holds on to: position in the space–time continuum [and] singularity of quality. [It seeks] quantitative instead of qualitative precision. By systematic analytic dissolution of the concrete–nesses of phenomena, it achieves generalization, repetitive constants, verification.” To Kroeber, classification of phenomena as to type omits uniqueness and reveals pattern.

In advocating science (rendered as cultural evolution) over history, White was expressing his displeasure with Boasian historical particularism, especially the Boasian view that cultures were so variable and free as to take a virtually infinite number of forms. To the contrary, White thought one could write laws about how cultures work and change over time, and thus that anthropology could be scientific (Hatch, 1973). Kenneth Bock (1952) showed that White’s view of history was the traditional one of historians—that the unique events of history cannot be generalized, although similar ones might be placed in a category and dealt with intuitively. Events classified as to type would in turn reveal a historical problem and prompt comparisons among the members of a kind in order to identify—typically by way of induction—commonalities and causes (Teggart, 1942). A parallel procedure in twentieth-century anthropology was referred to as the comparative, or cross-cultural, approach. As George Murdock (1957, pp. 249, 250) noted, a “science of human behavior” cannot depend on “culture-bound generalizations” but instead demands generalizations that have been cross-culturally tested and verified. Murdock was explicit about the difference between particularistic history and the scientific value of multiple instances of a kind of phenomena:

The events which produce cultural change ... are invariably historical, i.e. specific with respect to time and place. Events occurring at different places and times may resemble one another, however, and exert parallel influences upon different cultures. It is thus possible to view changes in culture either in relation to their spatial and temporal setting or in relation to comparable events wherever and whenever they have occurred. The former or “historical” approach answers such questions as what? when? and where? The latter or “scientific” approach, by illuminating the processes by which change occurs, answers the question how? Both approaches are valid and completely complementary. (Murdock, 1956, p. 250)

Fifteen years after the debate between White and Kroeber, both commented again on the issue of whether history was a science or not. White (1959a, p. 114) reiterated that classification is critical to the science of evolution and that history focuses on particulars and there-
fore does not classify particularistic events into kinds. As a result, history cannot be scientific, and although evolution takes place over time, evolutionism is not historical but rather scientific precisely because it classifies events as to kind. Kroeber’s (1960) point was that Darwin’s theory of biological evolution had brought to historical inquiry a means of being scientific (see also Kroeber, 1957, 1958/1963). He noted that the Linnaean taxonomic system provided Darwin with data that he could contemplate and eventually explain with his theory. Anthropologists, in Kroeber’s (1960, p. 14) view, “do not yet clearly recognize the fundamental value of the humble but indispensable task of classifying—that is, structuring—our body of knowledge.” The Linnaean taxonomic system was not built with any theory of evolution in mind (see discussion in Lyman & O’Brien, 2003a; O’Brien & Lyman, 2003). Darwin supplied that, in the process showing why the Linnaean system had the structure that it did (Padian, 1999). The active interplay of constructing classifications and building theory to account for kinds of phenomena has long been acknowledged in evolutionary biology (e.g., Hull, 1970; Lewontin, 1974b) but was hardly mentioned in Americanist anthropology and archaeology during the twentieth century (see Dunnell, 1986, for an exception, and discussions in Lyman & O’Brien, 2002, and O’Brien & Lyman, 2002a).

Boas had in fact remarked early on that the “development of ethnology is largely due to the general recognition of the principle of biological evolution. It is a common feature of all forms of evolutionary theory that every living being is considered as the result of a historical development. ... The development of ethnology is largely due to the adoption of the evolutionary standpoint, because it impressed the conviction upon us that no event in the life of a people passes without leaving its effect upon later generations” (Boas 1888/1940, p. 633). Kroeber (1957, 1960, 1958/1963) may have taken his inspiration for using biological evolution as exemplifying historical science from this statement. He clearly thought that sufficient particularistic data were now available in anthropology to begin generalizing, to “recognize repetitive regularities” that “themselves point to laws” (Kroeber, 1957, p. 286). Classification (Kroeber, 1960) was the means to recognizing the regularities. Processual archaeologists seem to have overlooked these key points.

**Idiographic and Nomothetic Archaeology Polemic**

Americanist archaeologist Walter Taylor (1948, p. 30) stated that historical disciplines are “non-experimental” and “deal with actual, non-repeating, unique, rather than ‘laboratory’ events.” In Taylor’s view, historians follow four analytical steps: (1) define a problem; (2) collect, analyze, and criticize data; (3) place the data in chronological sequence; and (4) integrate and synthesize the data such that interrelations among the phenomena in the temporal series are established. This sounds like Kroeber in the sense of searching for patterns and causal processes if not general laws. Taylor’s clarion call ushered in a period typically held up as the time when Americanist archaeology shifted from being a discipline concerned with descriptive, idiographic history to being a nomothetic science. A primary example of this is found in Gordon Willey and Philip Phillips’s (1958, pp. 5, 6) description of what they termed “processual interpretation” as “an attempt to discover regularities in the relationships given by the methods of culture-historical integration” and their claim that such interpretations, when based on discovered regularities, comprised causal explanations.

A year later, Joseph Caldwell (1959, p. 304) indicated that the “new archeology in America is tending to be more concerned with culture process and less concerned with the descriptive content of prehistoric cultures. There are now two kinds of problems, historical and general.” The former was concerned with “the identification of cultures” and the latter
with “the identification of culture processes” (Caldwell, 1959, p. 304). Caldwell (1959, p. 306) advocated the multilinear cultural evolutionism of Julian Steward (1955) as the model that would eventually allow archaeologists to “discover the workings of a finite number of general cultural processes.” A few years earlier, Betty Meggers (1955, p. 116) had examined “those sciences we consider to be models of the scientific approach” in order to determine if archaeology fell into the category. She noted that sciences have laws—both universal and probabilistic—as did anthropology and archaeology at the time, as seen in the cultural evolutionism of White (1949) and Steward (1955). Further, archaeology and anthropology used interpretive principles such as environmental determinism and diffusion to explain cultural phenomena. These principles had been based on generalizations derived from observed patterns, just as in the model sciences. Thus, Meggers argued, the suggestion was false that humans were too individualistic in their behaviors and/or that social behavior was too complex to subsume under law-like principles. Without using the words, Meggers summarized the then-perceived differences between nomothetic science and idiographic history and argued that archaeology was the former.

Meggers was not taking an isolated path in declaring archaeology scientific; Americanists had long thought of what they did to be science (Lyman et al., 1997), although until about 1950 what was thought of as scientific archaeology largely involved the construction, testing, and refinement of cultural chronologies. Beginning in the late 1940s, successively more archaeologists sought to explain those chronologies as well (Lyman & O’Brien, 2001). That is, there were two recognizable goals to archaeological research—chronicle and history, to use O’Hara’s (1988) terms. This, in our view, is what made archaeology scientific—the point made by Caldwell (1959) when he used the term “new archaeology.” But there were those archaeologists who would scoff at the idea that any archaeology done prior to 1962 could be classified as being scientific. That was the year that Lewis Binford, a student of White’s, wrote an article titled “Archaeology as Anthropology” (Binford, 1962) in which he emphasized the “need for developing a ‘scientific’ (usually as opposed to a ‘historical’) archaeology” (Bayard, 1969, p. 376).

Binford and his followers, of which there were many, became known as the real “new archaeologists,” also known as “processualists” because of their goal of understanding culture process. Their goal was nothing short of making archaeology scientific, but they had a narrow view of science. Patty Jo Watson (1973a, p. 48) pointed out that “science is not delimited by use of computers, test tubes, Pearson’s r, calipers, or lab coats,” but she and other processualists adopted a particular method of logic based on physics and chemistry, believing that this would make their discipline a science (Dunnell, 1982). Citing philosopher Carl Hempel (1942/1965), Binford (1968) argued that the discipline should seek to build laws of cultural processes. His call was picked up by John Fritz and Fred Plog (1970), who exposed archaeologists to the details of Hempel’s deductive-nomological (D-N) model of science. Their effort was almost immediately followed by Patty Jo Watson, Steven LeBlanc, and Charles Redman’s (1971) book-length treatise on the D-N model. Their efforts brought to the surface yet again the debate of whether archaeology, traditionally a historical discipline, could ever be scientific (Morgan, 1973, 1974; Watson, LeBlanc, & Redman, 1974). Watson, LeBlanc, and Redman (1971, p. 167) agreed with Bruce Trigger (1970, p. 27), who characterized “traditional”—that is, pre-Binfordian—archaeology as “particularizing” and “essentially descriptive” (following Taylor, 1948; White, 1945), whereas the then-emerging processual archaeology advocated by Binford, Fritz, and Plog, and Watson, LeBlanc, and Redman was, according to Trigger (1970, p. 28), “concerned with the formulation of general rules [read laws] of cultural behavior.” Trigger (1970, p. 30) pointed out that “history” has as its aim the explanation
of “individual situations in all their complexity rather than the formulation of general laws for indefinitely repeatable events and processes. That is what is meant by saying that history is idiographic, the social sciences nomothetic.” The social sciences are nomothetic because they seek to identify recurring variables and the relations among them.

Many of these same arguments were repeated a few years later. Watson (1973b, p. 113) noted that some archaeologists distinguished “between archeology as history and archeology as science.” She argued that whereas Trigger (1970) indicated that archaeology could be both, he emphasized the former at the expense of the latter. Watson clearly favored science over history. She pointed out that “both nomothetically and particularistically oriented studies are necessary to advancing [archaeological] knowledge” and that “the empirical data of the archaeological record can be treated in an explicitly scientific manner by both nomothetists and particularists” (Watson, 1973b, pp. 121, 122). Apparently this involves the derivation of laws from multiple empirical cases and the use of those laws to explain additional empirical cases. This was Simpson’s (1963a, 1970) position. But the tone of her discussion made it clear that Watson, like her husband Richard, preferred the scientific, nomothetic version of archaeology rather than the historical, idiographic version. She in fact indicated elsewhere (Watson, 1973a, p. 51) that in order to provide explanations, we must, following the D-N model, have “a corpus of accepted archaeological laws.” Because such a corpus was then unavailable, archaeologists should focus their efforts on building those laws.

Trigger (1973, p. 95) echoed Watson’s position that “both idiographic, or particularizing (i.e., historical), and nomothetic, or generalizing, disciplines are vital components of a scientific study of human behavior.” He made the important point that “chronology and description are basic to both nomothetic and idiographic explanation, but, by themselves, they do not constitute history. The goals of historical research are as explanatory as the goals of the nomothetic social sciences” (Trigger, 1973, p. 97). He also stated that many historians and philosophers of science would agree that “if historical explanation means to explain historical data, [then] special techniques are required that lie outside the normally accepted logical structure of covering law theory” (Trigger, 1973, p. 99), or the D-N model. In the place of covering laws, one uses “generalizations” to account for what is typically the incomplete historical data associated with the phenomena of interest (Trigger, 1973, p. 99). This sounds to us much like Watson’s (1973b, p. 117) suggested use of a “law or lawlike generalization”—defined as a statement of “a relationship between two or more variables or sets of variables”—to explain phenomena.

Trigger (1973, p. 102) recognized the value of theory to scientific explanation but pointed out that the cultural evolutionism of White (1949, 1959b) and Steward (1955) was “overly general and trivial” given that historians are more interested in the unique features of events that recur than in the features shared by members of a type of event. This is so, in Trigger’s (1973, p. 103) view, because “the complexity of the variables involved in concrete sociocultural relationships explains the often claimed uniqueness of historical events.” Explanation in a nomothetic vein is attained by “concentrating on the features that classes of events have in common” (Trigger, 1973, p. 103). This largely echoed Simpson’s (1970) response to Richard Watson (1966). Trigger (1973, p. 108) did two things to make his case for the scientific nature of historical explanation. First, he noted that such explanations are of the “how possibly” sort (Trigger, 1973, p. 107). Second, he pointed out that historical explanations can be tested with “new archeological evidence” (Trigger, 1973, p. 108). These two characteristics are precisely those used by O’Hara (1988) and Brandon (1990) to describe historical explanations provided by evolutionary biologists (Cooper, 2002; Gould, 1986, 1989; Moore, 2002).
Trigger and Watson accepted that archaeology could be both history and science, yet neither acknowledged Kroeber’s early statements to this effect. Douglas Bamforth and Albert Spaulding (1982) later reiterated that archaeology in fact was both. In their view, science and history consist of “empirical inquiry,” which they characterized as involving “publicly ascertainable evidence [as] the method of testing hypotheses and theories” (Bamforth & Spaulding, 1982, p. 183). That is, history and science are not necessarily mutually exclusive. To distinguish history from science is “meaningless” because both involve building explanations on the basis of “explicit or implicit covering generalizations because there is no other way to explain empirical data” (Bamforth & Spaulding, 1982, p. 191).

Robert Dunnell (1982) introduced to Americanist archaeology the distinction between the ontologies of essentialism and materialism, but he did not reference Simpson (1963a, 1970), despite the fact that paleontology is closely related to archaeology (O’Brien & Lyman, 2000, 2002b). Dunnell, like Bamforth and Spaulding, highlighted the role of theory in archaeological explanation, but he was virtually the only archaeologist until the early 1990s to note the interrelations of theory, units, and laws in science: “Theory in the sense of explicit units and laws governing their interactions universally is the hallmark of science” (Dunnell, 1982, p. 5). He pointed out that “theory is a substitute, explicitly compounded and expressly limited in applicability, for indigenous, covert rules and units used in understanding” and also that “because of the constraints placed by theory on asking questions, making observations, and assembling linking statements, empirical conclusions are linked into internally coherent bodies of knowledge” (p. 6). Further, the units must be “measurable,” else “definitive empirical testing is not possible” (p. 7).

Many Americanist archaeologists working today will argue that they have a wealth of theory by which the archaeological record can be explained, and that they are doing science (Spencer, 1997, and selected references therein). Not everyone agrees with this assessment, but not because of any perceived difference between history and science. Rather, now the difference of opinion has its roots in the different perceptions of what, exactly, comprises a materialist (configurational) ontology and whether or not such an ontology is necessary for archaeology to be a historical science (O’Brien & Lyman, 2000).

**Idiographic and Nomothetic Archaeology Examples**

According to processual archaeologists, culture historians believed they had arrived at an explanation when they could build a chronology of prehistoric events. Binford (1968, p. 268), for example, described an explanation offered by culture historians as involving an “argument which proposes a cause and effect relationship between events when the only justification for such a proposal is the sequential nature of the events cited.” He argued that this failure of “historical explanation” resulted from the absence of general laws regarding the “relationships between two or more variables” (Binford, 1968, p. 270). Laws in turn were to be built by “observation, hypothesis formulation and testing [because this] is what constitutes the scientific method” (p. 270). To illustrate his point, Binford (1968) critiqued a culture-historical explanation for the collapse of the 1,100-year old Mayan civilization that involved invasion of Mayan territory by some unidentified people. Binford’s points were several, including that invasions may not precipitate a collapse of indigenous society, and that such collapse may occur for some reason other than invasion. In his view and that of other processualists, general laws regarding cultural dynamics had to be written in order for archaeology to explain such events scientifically.

Examples of possible cultural laws were discussed by proponents of processual archaeology. Based on analyses of archaeological materials performed by others, Watson et al.
(1971, p. 39) suggested the following “hypothetical general law”: “If the formal characteristics of [prehistoric pueblo] rooms are the same or closely similar to those in [modern] pueblos, the functional characteristics are also the same or closely similar.” Watson et al. originally named a particular prehistoric pueblo and stated that the law when so worded “is of limited scope because of the specificity of the circumstances” (p. 39). Commentators expressed skepticism that this statement was a law (Renfrew, 1973; Schuyler, 1973). We have reworded the original from a particular prehistoric pueblo to include all prehistoric pueblos to underscore the fact that this is merely a statement of analogy of a sort typically used in Americanist archaeology (Wolverton & Lyman, 2000). Strangely, Watson et al. (1971) seem to implicitly recognize that their hypothetical law is a statement of analogy that they would not accord the status of a universal cultural law.

The supposed law of pueblo-room function does not literally concern culture processes, the alleged subject of interest of all processual archaeologists. Culture processes were originally defined by the chief architect of processual archaeology as “the dynamic relationships (causes and effects) operative among the components of a [cultural] system or between systematic components [of a culture] and the environment [in which the culture resides]” (Binford, 1968, p. 269). What Watson et al.’s law seems to suggest is that the spatial distribution of human behaviors and activities (causes) results in artifacts with different functions being differentially distributed across space (effects). To us, this seems to exemplify what processual archaeologist Kent Flannery (1973, p. 51) claimed had been labeled “by some [unnamed] critics [as] ‘Mickey Mouse laws.’” Further, the pueblo-room law does not concern another major subject of interest to processual archaeologists, namely, the causes (and processes) and effects of cultural change.

Processualists proposed and employed a possible law concerning cultural change in their seminal substantive studies (various chapters in Binford & Binford, 1968). This law can be stated basically as follows: As cultural-transmission networks within a society are disrupted and infiltrated by individuals originally external to the society, then nonfunctional (stylistic) phenomena will become more heterogeneous, change more rapidly, or both (Binford, 1963). Of course, whether this is truly a cultural law or not is questionable, as noted by one commentator on a particular version of this alleged law (Schuyler, 1973). The point to make here, however, is that many processualists believed that they could test such proposed laws of cultural processes and dynamics using archaeological data (e.g., Watson et al., 1971, p. 53). As noted by several commentators (e.g., Morgan, 1973, p. 270), this was incorrect because the test concerned the matching of effects created when the causes were invisible (prehistoric) with effects created when the causes were visible (modern). Equifinality, which “simply stated ... means that any final state may be reached from different initial conditions and in different ways” (Lamberg-Karlovsky, 1970, p. 112) was a serious problem. It is beyond our scope here to delve into this topic deeply, but we note that an eventual result was that processual archaeologists turned from the writing of laws of cultural dynamics as the hallmark of the scientific status of their discipline to the hallmark that their analytical and interpretive results be testable. That they might have originally adopted the wrong model of science as a guide to redesigning archaeology was pointed out almost from the get-go (Clarke, 1972). It should be clear that we think they would have been better advised to rewrite anthropological theory in archaeological terms, just as paleontology rewrote neo-Darwinian evolutionary theory in fossiliferous terms. Instead, as we have argued here, in their effort to make their discipline “explicitly scientific” (Watson et al., 1971), processual archaeologists attempted to write laws of culture process in terms of existing cultures.
The nature of anthropology as a scholarly discipline is such that individual anthropologists regularly felt compelled in the twentieth century to argue that the discipline is a science when it focuses on historical questions. Few of these discussions provide explicit definitions of science, history, or historical science, although authors might occasionally list some of the characteristics of each. More frequently, they simply apply the descriptors nomothetic (generalizing) to science and idiographic (particularistic) to history. One noteworthy exception to these general patterns is provided by David Aberle (1987, p. 356):

The historical sciences are historical because they deal with entities and groups of entities that have traceable continuity over time, but that also change. They are scientific because they have methods for choosing among competing hypotheses about the nature and order of changes in the phenomena under study and methods as well for choosing among competing hypotheses that attempt to explain those changes. The first of these analyses is the process of historical reconstruction. In this approach there is no antithesis between history and science, or between history and evolution. The methods and goals of historical science are scientific; its products range from particular histories to tests of general propositions.

In his synopsis of the methods of culture history, linguist Joseph Greenberg (1968, p. 449) outlined the methods used by Darwin and subsequent evolutionists interested in history: “[T]he general strategy of the historical enterprise is based on the circumstance that the same event may leave multiple traces, each of which provides independent evidence for the fact.” Further, “a process is ... a class of similar changes” (Greenberg, 1968, p. 449). In Greenberg’s (1968, p. 451) view, archaeology is a historical science because it reasons “from trace as effect to historical cause,” where traces are artifacts. Cultural processes—the subject of most interest to nomothetic archaeologists—were characterized as the historical processes of cultural change by Murdock (1956), who listed the processes of innovation (including invention, cultural borrowing, and what he called “tentation”), social acceptance, selective elimination, and integration (see also Barnett, 1940; Herskovits, 1945; Steward, 1953). These are only proximate causes because one can readily ask, even after identifying the specific process that applies in a particular case, why a cultural trait was invented, why it did or did not diffuse, why it was or was not accepted, eliminated, or integrated (Lyman & O’Brien, 2003b). Theory provides hypothetical reasons, in the form of ultimate causes, and these have testable implications. From this perspective, the issue becomes not whether anthropology is science or history, but rather what the explanatory theory is.

Robert Carneiro (2000, p. 216) recently observed that “Nothing inherent in the totality of events constituting history renders them unsuitable to serve as raw material for a science of culture. After all, the phenomena of every science come to us not only as particular and unique, but also as intricately bound up with each other. Yet all the factors at work are carefully sorted out and categorized by the scientist, and laws are derived for their individual behaviors.” Carneiro (2000, p. 218) identified a critical stumbling block to scientific history when he noted that to a “traditional historian, an event is always concrete and particular; it happened once and will never recur. As such, it is to be described and explained in terms of the unique constellation of circumstances that preceded and surrounded it, that gave it its distinctiveness and individuality.” The key, he pointed out, is to dissolve individual, singular events “in the universal solvent of general types and classes” (Carneiro, 2000, p. 219). We agree but add the qualification that unit construction, testing, and revision must take place simultaneously with the building, testing, and revision of explanatory theory. Carneiro (2000)
prefers the essentialist evolutionary models of Leslie White and Julian Steward. We prefer the materialist evolutionary theory of Darwin (O’Brien & Lyman, 2000, 2002b).

We find the distinction between history and science when both are cast in terms of evolution (irrespective of the version) to be false unless history is defined narrowly as simple chronology—what came before what. Although there are epistemological differences between the two, science is not inherently superior to history (Cleland, 2002). In Americanist anthropology, the distinction between history and science can be traced back to Boas’s (1896, 1904) discarding of the universal cultural evolutionism of Tylor and Morgan. Although Boas perceived history to be scientific, various of his students and critics worried about the distinction because of the view that science is nomothetic and law-based whereas human history is contingent on free will and accident. The distinction carried over into the archaeology of the 1960s and 1970s. The processualists assured themselves that what they were doing was scientific as opposed to simply historical, never realizing that one did not preclude the other. It was not until the 1990s that the discipline began to take seriously the integration of history into science—a development that we chronicle elsewhere (O’Brien & Lyman, 2000).

REFERENCES


