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The Concept of Evolution in Early Twentieth-Century Americanist Archaeology

R. Lee Lyman & Michael J. O'Brien
University of Missouri

ABSTRACT

American archaeologists who founded the culture-history paradigm early in the 20th century held a view of the evolution of cultures commensurate with Darwin's notions of continuous, gradual change. Initially, in their view, artifacts varied continuously across space and through time. This materialist metaphysic was, however, short-lived. Its fall from favor began in the 1920s, when it was noted that cultural evolution was reticulate whereas biological evolution was only branching. When it was argued in the 1940s that cultures were, after all, not organisms and that inanimate objects did not interbreed, any hope of adopting a Darwinian version of evolution in archaeology was abandoned. Rather, archaeologists adopted cultural evolutionism founded by Herbert Spencer in the 19th century and popularized by Leslie White in the 20th century. Franz Boas and his students flatly rejected this version in part because of its essentialist metaphysic. A. L. Kroeber had some significant insights into how to construct historical lineages, but he and his contemporaries did not know how to implement Darwin's version of evolutionism in anthropology and archaeology. Archaeologists of the 1950s thus came to view culture change largely in terms of discontinuous stages. Such a view gained legitimacy because biologists in the 1950s claimed the evolution of cultures was unlike the evolution of organisms. After some forty years, it is time to try a Darwinian version of evolution.

When cultural evolution is considered a natural extension of organic evolution, our thinking about the total process of evolution will be greatly clarified (W. G. Haag 1961: 440).

INTRODUCTION

Over the past decade or so, an increasing number of archaeologists have begun to show interest in employing Darwinian evolutionary theory to explain variation in the material record. Although Darwinian evolutionary archaeology has not enjoyed the same degree of popularity as some other approaches have over the years, a now-sizable literature attests to the fact that archaeologists are serious about incorporating the tenets of Darwinism into their work (see O'Brien 1996b, Schiffer 1996 and references therein). Given our personal perspectives, we are pleased with the careful attention archaeologists are giving Darwinian archaeology, but we also realize that, regardless of the attention it receives, its success can only be measured in terms of its own performance in successfully explaining the archaeological record.

Clearly, Darwinian evolution is a different kind of theory from the ones with which it competes, being rooted as it is in materialism as opposed to typological strategies. Because of this, most of the available data that routinely are used in archaeological analysis are inappropriate for the kinds of investigation that derive from a Darwinian perspective–an issue that is as pertinent in archaeology (Dunnell 1980, 1982, 1985, 1986, 1988; O'Brien & Holland 1990, 1992; O'Brien et al. 1994) as it is in biology (Dobzhansky 1951; Ghiselin 1966, 1974, 1981; Mayr 1959, 1963, 1973, 1977, 1987; Sober 1980, 1984). This is one issue with which scientific evolutionism must contend, but at this point it might not be the most critical issue. One of the risks involved in persuading a discipline that a particular approach has much to offer is that practitioners will view it as simply another in a long string of approaches being sold on the street corner–something

seemingly exciting and appealing that quickly disappears after its original advocates find a new product to hype. In addition, there is a danger in the increase in visibility attached to a new approach. As one of us pointed out recently (O'Brien 1996a), history shows that archaeologists have been quick to jump on bandwagons without the slightest notion of why they are doing so and certainly without the background necessary to understand the nuances of the approaches they begin advocating. The lessons learned from Americanist archaeology of the 1960s and '70s should teach us that seemingly new approaches are not necessarily all they are purported to be, nor are they necessarily improvements on what came before.

One of the things missing in recent attempts to persuade the discipline that Darwinian evolutionism offers the best means of explaining the archaeological record is an examination of the roots of that approach in Americanist archaeology. Historical reviews by Dunnell (1980) and O'Brien and Holland (1990), for example, make it sound as if archaeologists all but ignored Darwinian evolution until an enlightened few made it accessible sometime after 1970. None of those involved in the renaissance of Darwinian evolution in archaeology actually believes such is the case, though it is clear in retrospect that their treatments of the historical roots of such evolutionism in archaeology are lacking in detail. Most such treatments spend considerable time on the differences between Darwinian evolution and cultural evolution, the latter of which made its way into archaeology during the 1950s and soon became a cornerstone of the so-called new movement in the discipline a decade later. These two kinds of evolutionism most assuredly have little in common, and hence discussion of the differences are warranted, but lost in the shuffle is the fact that Americanist archaeologists earlier this century made serious efforts to incorporate elements of Darwinian evolutionism in their work. Those committed today to such an evolutionary archaeology need to understand not only the arguments put forward by these earlier investigators but also why, when it appeared there was a growing consensus toward making archaeology a materialistic discipline, Darwinian evolutionism fell from favor. It is toward these two key issues that we address this paper.

CULTURE HISTORY: THE BACKBONE OF THE DISCIPLINE

Interpreting culture change—alternately referred to as *cultural development* by archaeologists early in the 20th century—was the central theme of Americanist archaeology beginning with the birth of the culture-history paradigm in the second decade of the 20th century (Lyman et al. 1997a, 1997b). As Clark Wissler (1917b: 100) remarked in what might be thought of as the birth announcement, Americanist archaeology not only now had some important questions to answer, but it also, for the first time, had a way to produce answers:

> [H]ow long has man been in America, whence did he come, and what has been his history since his arrival? . . . [T]he archaeologist finds in the ground the story of man and his achievements. The new, or the real archaeology is the study of these traces and the formulation of the story they tell. . . . [The archaeologist] must actually dissect section after section of our old Mother Earth for the empirical data upon which to base his answers. It is not merely the findings of things that counts; it is the conditions and interassociations that really tell the story.

This had been the goal of archaeology since the beginning of the 20th century. As Franz Boas (1904: 521-522) 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).

The techniques for addressing the issues raised by Wissler and Boas—the latter having recognized that "In the study of American archaeology we are compelled to apply methods somewhat different from those used in the archaeology of the Old World" (Boas 1902: 1)—involved studying the fluctuation of frequencies of artifact *types* through time, with the passage of time initially confirmed and later established by the superposed positions of the actual artifacts themselves (Lyman et al. 1997b).

That the goals of culture history never varied for the next several decades is clear from the remarks of one of the parents of the paradigm, A. V. Kidder:

> Archaeologists, noting that modern biology has mounted above the plane of pure taxonomy [that is, classification], have attempted to follow that science into the more alluring fields of philosophic interpretation, forgetting that the conclusions of the biologist are based on the sound foundation of scientifically marshalled facts gathered during the past century by an army of painstaking observers. This groundwork

> we utterly fail to possess. Nor will it be easy for us to lay, because the products of human hands, being unregulated by the more rigid genetic laws which control the development of animals and plants, are infinitely variable. But that is no reason for evading the attempt. It has got eventually to be done, and the sooner we roll up our sleeves and begin comparative studies of axes and arrowheads and bone tools, make classifications, prepare accurate descriptions, draw distribution maps and, in general, persuade ourselves to do a vast deal of painstaking, unspectacular work, the sooner shall we be in position to approach the problems of cultural evolution, the solving of which is, I take it, our ultimate goal (Kidder 1932: 8).

Kidder's remarks are important not only because they outline the goals of the culture-history paradigm but because they also indicate how culture historians conceived of the phenomena they were studying. Cultures *evolved;* a historically documented culture had a developmental heritage or lineage, and it was the job of the culture historian to describe that lineage and to determine why it had the form that it did. But Kidder correctly indicated that archaeology lacked both the basic data and a *theory* consisting of cultural processes parallel to the biological ones of genetic inheritance and natural selection to help explain a culture's lineage in evolutionary terms. To those interested in incorporating Darwinian evolutionary theory in archaeology, Kidder's words couldn't have been better put because he focused on the issue that underlies any evolutionary study—namely, the documentation of variation. As we point out later, without the means to document variation and then to link that variation to applicable theory (which should dictate *how* we measure the variation), so-called explanations of how and why things change—the epitome of evolutionism—will be nothing more than a series of stories.

Kidder knew this, but most of his contemporaries either didn't appreciate the point or failed to pay it much heed. Compiling the data and building the theory evidently would take some work, Kidder suspected, and in the long run, only the former has been met with any sort of empirically verifiable success. Chronologies of artifact types and larger units variously termed cultures, phases, complexes, and the like have been constructed, tested, refined, empirically verified, and are now available for many areas of the Americas. Explanations of those sequences, particularly in the sort of evolutionary terms Kidder and many of his contemporaries envisioned, are, however, notably lacking.

Some Americanist archaeologists would, we suspect, argue long and loudly that explanations of an evolutionary sort *are* available and are made every year in professional journals and books and at professional meetings (see the various chapters and references in Maschner 1996). In one sense they would be correct, but the sort of evolution that usually is presented and discussed has nothing to do with the sort that Kidder had in mind. To fully understand the significance of what Kidder envisioned and why his vision has not yet been attained, we begin with a consideration of why many so-called evolutionary explanations are non-Darwinian in nature.

WHAT *IS* EVOLUTION?

Anyone familiar with the history of biology will know that Charles Darwin's version of evolution was not the first or the only version available in the theoretical marketplace of the 19th century. For example, Jean Baptiste de Lamarck had a version, as did Georges Cuvier, and both versions were, at one time or another, as popular with segments of the scientific community as Darwin's came to be. But in the end, it was Darwin's version that took hold, in the process relegating its competitors to obscurity. Relative to biological evolution, our concern here is only with Darwinian evolution. There also was a theory of evolution that concerned nonbiological phenomena, including such things as human society and technology. This was the version of evolution that Herbert Spencer was selling in the mid-19th century (O'Dowd 1982). Throughout the following discussion, we use the term *cultural,* or *Spencerian, evolution,* to denote the kind of evolution espoused by Spencer and his intellectual descendants, including such luminaries as Lewis Henry Morgan and Edward Burnett Tylor during the 19th century and Leslie White, Julian Steward, Marshall Sahlins, Elman Service, Morton Fried, and Robert Carneiro during the 20th century.

To denote the kind of evolution introduced by Darwin, we use the terms *biological evolution,* or *Darwinism.* In this respect, it is important to note, following biologist Ernst Mayr (1972, 1982), that Darwinism was not some monolithic theory that swept through biological science in the 1860s and set it on an unwavering course. Just as with its biological subjects, Darwinian theory evolved. Darwinian evolution of the 1980s was not the same theory that it was in the 1880s or that it was in the 1930s. Understanding both the evolutionary history and the metaphysical underpinnings of Darwinian evolutionary theory is key to understanding why Americanist archaeology, after enjoying some success with that theory, rejected it and adopted cultural evolution in the 1950s.

Two Metaphysics

We use the unmodified term *evolution* to denote change, regardless of the mechanism, process, or form. As the quotations from Wissler and Kidder indicate, culture historians since the second decade of the 20th century were interested in documenting, studying, and explaining cultural change. Theirs was a historical science, in many ways similar to that of paleobiologists, whose business it is to document and explain the phylogenetic histories of organisms. But whereas paleobiologists were able to erect an explanatory theory of biological change after the synthesis of genetics and natural selection took place in the late 1930s and early '40s (Mayr 1972, 1982), archaeologists followed a different path and adopted a different method of explaining their subject matter. The most significant difference between the two fields of inquiry–paleobiology and Americanist archaeology–resides in the fact that by adopting different theories of evolution, the two fields adopted decidedly different metaphysics. Biologists in the early 19th century subscribed to an essentialist metaphysic, and it wasn't until the mid-1950s that they began to replace it with Darwin's materialist metaphysic (Mayr 1982; see also Gould 1986). Anthropologists and archaeologists of the early 20th century tended to subscribe to a notion of cultural evolution that was in many respects materialistic, but they replaced that notion with an essentialist one in the 1950s. What, then, are these two metaphysics?

Essentialism presumes the existence of discoverable, discrete kinds of things. Things are of the same kind because they share essential properties–their "essences"–and these essential properties dictate whether a specimen is of kind A or kind B. Essential properties define an ideal, or archetype, "to which actual objects [are] imperfect approximations" (Lewontin 1974: 5). This view renders nonessential variation between specimens as simply "annoying distraction" (Lewontin 1974: 5). An advantage of the essentialist metaphysic is that prediction is possible because the kinds are real and thus are always and everywhere of the same sort; they will therefore always interact in the same manner, and the same result will be produced by their interaction. Laws, in a philosophical sense, thus can be written because the interactions of things will always be the same. The things, as well as the interactions between things, will always, regardless of their positions in time and space, be the same because the essential properties of the things themselves never change. Specimens grouped within natural, essentialist kinds always, by definition, share essential properties. Not surprisingly then, ahistorical sciences such as chemistry employ an essentialist metaphysic, since what they are measuring is *difference* as opposed to *change* (Hull 1965, Mayr 1959, Sober 1980).

In fact, under essentialism, it is impossible to measure change. Only the difference between different kinds, or types, can be measured. For this reason, essentialism often is referred to as *typological thinking* (Mayr 1959, 1988). Mayr (1959: 2) notes that "Since there is no gradation between types, gradual evolution is basically a logical impossibility for the typologist. Evolution [change], if it occurs at all, has to proceed in steps or jumps." How could change be anything *but* transformational? If things have essences, the only way that things could evolve is by dropping one essence and adopting another. Thus a specimen of kind A is kind A in this time and place, but in another time and place it somehow has transformed into kind B. As Mayr (1982: 38–39) stated in regard to biological speciation, "Genuine change, according to essentialism, is possible only through the saltational origin of new essences [species]." Change is neither gradual nor continuous; it is jerky and punctuated.

In opposition to essentialism, materialism holds that phenomena cannot exist as bounded, discrete entities because they are always becoming something else. With specific reference to organisms, Mayr (1959: 2) pointed out that "All [things] are composed of unique features and can be described collectively only in statistical terms. Individuals . . . form populations of which we can determine an arithmetic mean and the statistics of variation. Averages are merely statistical abstractions, only the individuals of which the populations are composed have reality." As a direct result of its materialist metaphysic, a historical science can monitor *change* in phenomena: "For the [essentialist-thinking] typologist, the type is real and the variation an illusion, while for the [materialist-thinking] populationist the type (average) is an abstraction and only the variation is real" (Mayr 1959: 2). It is this variation between and among specimens that "is the cornerstone of [Darwinian evolutionary] theory" (Lewontin 1974: 5).

Classification Units

If only *variation* is real, how do we study it? The answer is, by constructing a set of units that allows *properties,* or attributes (*character states* to biologists; e.g., Szalay & Bock 1991), of phenomena to be measured. Archaeology is nothing if not a spawning ground for units, many of which–type, group, class, period, phase, and so on–are rarely defined explicitly. Here we use the term *measurement* to denote the assignment of a symbol–letter, number, word–to an observation made on a phenomenon according to a set of rules. The rule set includes specification of a scale (e.g., Stevens 1946) and the relation be-

tween the symbols and the scale. Observation and recording of attributes on specimens comprise measurement. But which attributes should we observe and record?

The analyst selects attributes relevant to some problem, and it is those attributes and their combinations that result in the sorting of specimens into internally homogeneous, externally heterogeneous piles. Importantly, specimens that share attributes or properties—those that end up together in one of the analyst's piles—do not have an essence (Mayr 1987: 155). They have been grouped together not because of some inherent, shared quality but rather because they hold in common some number of attributes *selected by the analyst.* Theory is the final arbiter of which attributes out of the almost infinite number that could be selected are actually chosen by the analyst. Theory also specifies the kinds of units to be measured. We might decide, based on theory, that the color of a stone tool is not related to its function, whereas the angle of the working edge is; thus, if we are interested in functional variation in stone tools, we choose as our attributes edge angles, traces of use wear (edge damage), and other attributes that *theoretically* are causally related to the property of analytical interest.

Because *we* decide on the attributes to be recorded, the resulting units—variously termed in archaeology types or classes—are *ideational;* they are not real in the sense that they can be seen or picked up and held. An edge angle—itself an ideational unit with different empirical manifestations—is measured, using a goniometer, in other ideational units known as degrees. In the simplest of terms, ideational units are tools used to measure or characterize real objects. An inch and a centimeter are used to measure length, just as a gram and an ounce are used to measure weight. Inches, centimeters, grams, and ounces do not exist empirically; they are units used as analytical tools to measure properties *of* empirical units. A writing pen is an empirical unit that can be placed in a set of things that are all six inches long, but only if the attribute distinguishing the set specifies that the things within it must be six inches long to be included; our theory, of course, tells us what length is and how to measure it, as well as how it differs from width, color, or any other dimension.

An *empirical unit* is a thing that has a real existence; it is phenomenological. We use the term *type* for units of unspecified kind; they may be either ideational or phenomenological. We note this is not always the meaning given to "type" by culture historians. We use the term *classification* in a general sense to denote an arrangement of units of whatever kind (see Lyman et al. 1997b for additional discussion).

What is Change?

Under the materialist view, things are continually in the process of becoming something else—not in a saltational, or jumpy, sense but in the sense of slow, gradual change (Figure 2.1). Depending on the scale at which we are operating, that change may be difficult or relatively easy to measure. Regardless, change must be measured as alterations in the frequencies of analytical kinds, or what we have termed ideational units. The classic model of the evolution of the modern horse (*Equus caballus*) from *Eohippus* to *Miohippus* to *Merychippus* and finally to *Equus* (Simpson 1967: 135) is merely a heuristic device that simplifies the phylogeny of equids into terms that allow discussion and description. In such a phylogeny, different forms, or types, of equid are given a (taxonomic) name and arranged against a time scale. Variation within the populations of horses that existed at different times and places is masked by such a procedure, but paleobiologists recognize this and use the names of various populations—each of a different ideational form—merely to discuss equid evolution. A realistic phylogeny explicitly constructed within the materialist metaphysic would include frequencies of those different forms plotted against time and space, with—and this is crucial—indications of the particular variant forms and specimens that represented particular taxa (e.g., Simpson 1951, Vrba 1980; see Gingerich 1979: 457 for a real example).

There is a critical question here that must be addressed: If two things are similar but also somewhat different in form and also different in age, do they indicate that change has somehow taken place? From a modern Darwinian viewpoint, they represent change only if they are phyletically (genetically) related, in which case the similarity of form and difference in age signifies inheritance and thus continuity—an ancestor–descendant lineage. How does one demonstrate a phyletic relation—that two phenomena are parts of a lineage? Paleobiologists accomplish this task by identifying homologous traits, or attributes, of the two phenomena. If the two phenomena share one or more such traits, they are by definition phyletically related. The analytical challenge, then, is to identify such traits.

Identifying homologous traits is a significant analytical hurdle (e.g., Bock 1977, Fisher 1994, Forey 1990, Simpson 1975, Smith 1994, Szalay & Bock 1991) because a trait that is shared by two phenomena may be analogous—the result of convergence. How are analogous and homologous traits to be distinguished? A. L. Kroeber (1931:151) suggested that "Where similarities are specific and structural and not merely superficial . . . has long been the accepted method in evolutionary and systematic biology." He was correct, for this was, and is, pre-

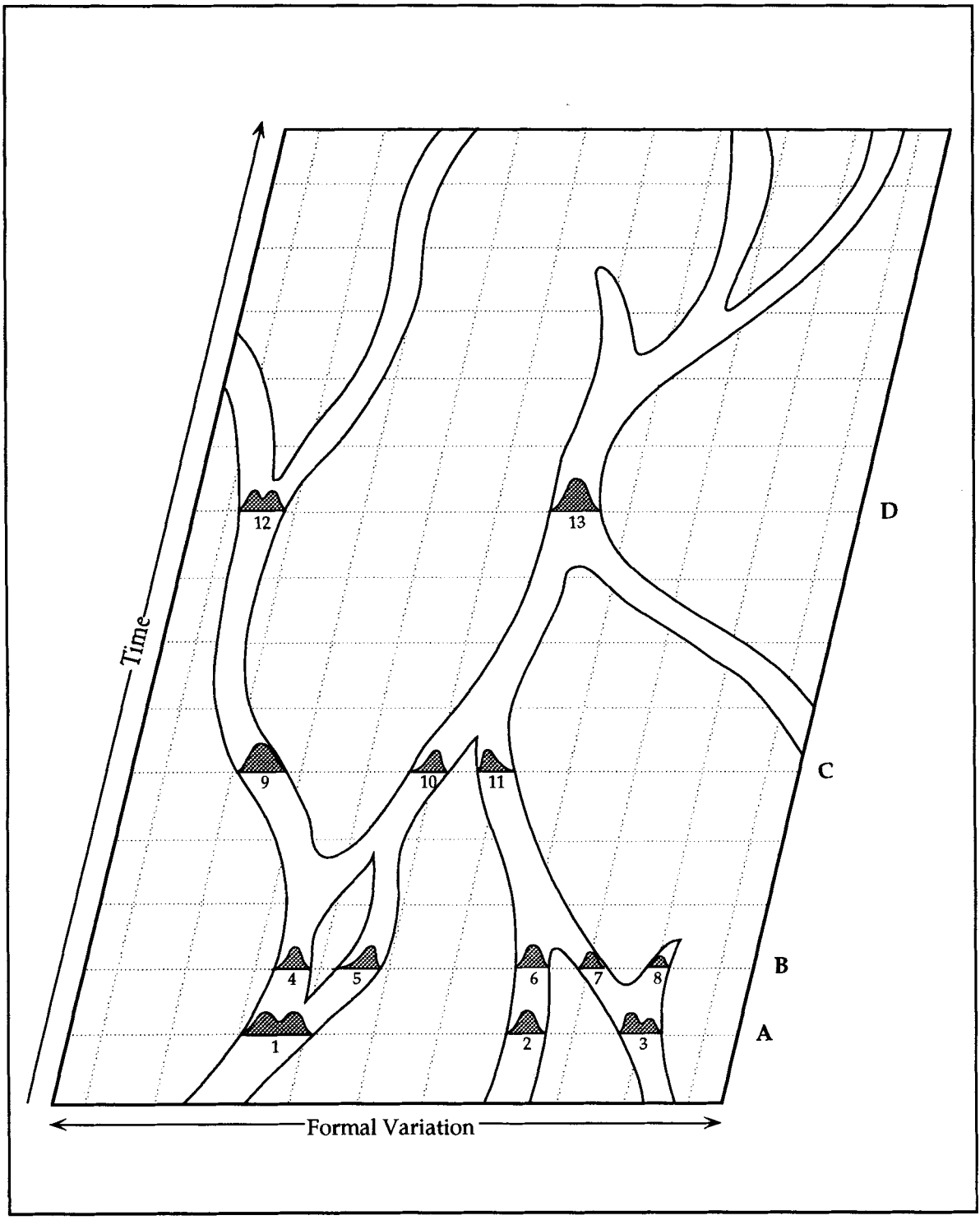

Figure 2.1. The braided (reticulate) continuum of Darwinian/materialist evolutionary change. Each shaded curve represents the frequency distribution of formal variants at a particular point in time. At time A, lineages 1 and 3 are diverging. At time B, lineage 1 has diverged into two (4 and 5), lineages 6 and 7 are converging, and lineage 8 is nearing extinction. At time C, lineages 10 and 11 are converging, hence the skewed frequency distributions of formal variants. At time D, lineage 12 is diverging. If each plotted frequency distribution represents an artifact assemblage or component (or set of fossils), and these distributions comprise the total known archaeological (fossil) record for an area, the analytical problem of culture history (paleobiology) is to determine their evolutionary relations, such as are indicated by the variously flowing, diverging, and converging lineages.

cisely how biologists distinguish between homologs and analogs (e.g., Rieger & Tyler 1979, Wagner 1989). The wings of eagles and those of crows are structurally as well as superficially similar; this is homologous similarity. The wings of eagles and those of bats are superficially, but not structurally, similar; this is analogous similarity. The important issue of distinguishing between analogous and homologous traits was only rarely mentioned and virtually never addressed in detail by culture historians, which was part of the reason the paradigm fell from favor in the second half of the twentieth century (Lyman et al. 1997b). But what is of most interest here is why culture historians abandoned a materialist conception of evolution in favor of an essentialist conception. By adopting the latter, the ability to distinguish between the two kinds of similarity became, in the minds of some, unnecessary.

EVOLUTION AT THE TURN OF THE CENTURY

Many archaeologists believe that "cultural evolution was generally anathema as late as the 1950s" but that it was revived by the end of that decade (Willey & Sabloff 1993: 220; see also Ehrich 1963: 21 [for an alternative view, see Trigger 1989: 295]). To be sure, cultural evolution—in the sense we have defined it here—did enjoy only minimal status within Americanist archaeology between about 1900 and the middle 1950s, faring only slightly better in anthropology as a whole. Willey and Sabloff's (1993: 306) contention that cultural evolution was dead during the first half of the century apparently was based in part on the fact that William Haag's (1959) review article "indicates only a limited number of evolutionary efforts in the field." Haag's article, however, published in the Anthropological Society of Washington's volume commemorating the centennial of Darwin's *On the Origin of Species* (Meggers 1959), was not intended to be an extensive review of the evolutionary-archaeological literature, nor was it written to adequately sample that literature. Gordon Willey's (1960) review, which was published at about the same time and in another centennial volume (Tax 1960), contained many more references, but most of them were to works that were published in the 1950s and represented recent syntheses that Willey was using to discuss the overall evolutionary trajectory of American cultural lineages.

We believe that there should be no disagreement at all over the alleged death and resurrection of cultural evolution because it is easy to demonstrate that the general notion of evolution—cultural or otherwise—played a *major* role in the growth and development of the culture-history paradigm in Americanist archaeology. Given the goal of that paradigm, how could it not have? As South (1955) indicated, its use was often covert and seldom explicit, but it was there nonetheless. Archaeology, after all, had one thing that cultural anthropology did not—access to an extensive temporal record. Everyone knew that evolution takes place over time. Once it was clear that measurable temporal differences existed in the archaeological record of the Americas, evolution of some kind was an obvious if not always explicit or well-developed explanation *of* that difference. More importantly, the *kind* of evolution used by archaeologists in the first several decades of the 20th century was at least conceptually of the Darwinian, or materialist, sort. However, this brand of evolution has been reborn only in the last twenty years (O'Brien 1996b). What seems to have happened is that in the 1940s and 1950s, an essentialist *cultural* evolution usurped the role previously played in Americanist archaeology by Darwinian materialist evolution. Understanding why cultural evolution replaced Darwinian evolution is important for those of us who wish to see the latter now replace the former. Knowing this history, various objections can be anticipated and defenses prepared. More importantly, we can learn from the mistakes of our Darwinian-leaning predecessors and avoid those errors in the future.

Evolution in Cultural Anthropology

Received wisdom holds that Boas killed any notion of cultural evolution at the end of the 19th century—an erroneous assumption attributable in large part to the grumblings of White (1943, 1945a, 1945b, 1947, 1959a, 1959b) but also to the reading of the death notice by others (e.g., Lesser 1952). White seemed to relish any opportunity to point out just how wrong-headed and closed-minded Boas and his students were. However, Boas and some other leading figures in anthropology at the time did not reject outright the basic notion of *evolution,* a point White (e.g., 1947) only occasionally acknowledged. Even today, Boas is often attributed with such a stance (e.g., Willey & Sabloff 1993). But Boas (1896: 904) merely was concerned with the fact that the theory of Spencerian *cultural evolution* resulted in a comparative anthropology founded on the interpretive "assumption that the same [cultural] features must always have developed from the same causes, [leading] to the conclusion that there is one grand system according to which mankind has developed everywhere; that all the occurring *variations* are no more than minor details in this grand uniform evolution" (emphasis added; see also Boas 1904: 519). Given that the key assumption—essentialist change was the only kind that had occurred—was demonstrably wrong, Boas (1896: 907) preferred the "historical method"

and what he believed was its ability to discover the processes that resulted in the *particular* development of different cultures. The historical method was more in line with the materialist metaphysic and would ultimately reveal–inductively, in Boas's (1896, 1904) view–laws concerning how cultures evolved rather than assume them, as did the comparative method.

It was clear to Boas that each culture's heritage–its developmental lineage–was unique. Perhaps most importantly, Boas (1904: 516) noted that in the second half of the 19th century, the "regularity in the processes of [Spencerian] evolution became the center of attraction [to Morgan and Tylor and others] even before the processes of [cultural] evolution had been observed and understood." Thus, in Boas's view, as more data accumulated, Spencerian evolutionary theory came to be less and less tenable. As a result, Boas (1904: 522) indicated that instead of "a simple [universally applicable] line of evolution there appear a multiplicity of converging and diverging lines [read *cultural lineages*] which it is difficult to bring under one system." The important point here is that Boas did not reject evolution *per se.* As Bidney (1946: 297) pointed out, "Boas never rejected the concept of cultural evolution but merely the notion of uniform laws of cultural development and the *a priori* assumption that cultural development was always from a hypothetical simplicity to one of complexity." Similarly, some years later Naroll (1961:391) observed that "the reason Boas rejected 19th century [Spencerian] evolutionism was neither theoretical [n]or conceptual, but methodological. . . . Boas always maintained that the study of cultural evolution was a major task of cultural anthropology, but Boas was bitterly opposed to confusing speculation with scientific generalization." Thus, Boas rejected the interpretive assumption of Spencerian cultural evolution, and in so doing he rejected its essentialist metaphysic. Importantly, he retained the materialist notions of evolution as history and of variation as critical to any study of evolution.

Some of Boas's intellectual progeny followed his lead and made some rather astute observations that would greatly influence archaeological thinking. In the first edition of his classic *Anthropology*, Kroeber (1923: 3) used the term "tradition" to label the processes of cultural transmission and heredity, thus distinguishing them from the biological "force" of genetic heredity. Both biological evolution and cultural development involved transmission (as Boas [1904] had indicated), but because transmission within the latter could be between individuals who were not "blood descendants," the concept of biological evolution–that is, genetic transmission and change–was "ambiguous" in a cultural setting (Kroeber 1923: 7). Thus Kroeber's view paralleled that of Darwin. The latter avoided the term evolution–it was used by Spencer to label his social philosophy–and instead used "descent with modification." Morgan and Tylor had used the term evolution precisely in the Spencerian sense, and it was that sense of the term that Kroeber (and Boas) rejected, not the basic notion of evolution as change through time or descent with modification.

Kroeber failed to distinguish between what we have termed cultural evolution and Darwinian evolution. This was not critical to his discussion because not only did his comment that the term evolution was ambiguous signify his rejection of cultural evolution, it signified his rejection of Darwinism as well. Regarding the latter, cultural lineages were, after all, not the result of genetic transmission. As a result, in Kroeber's (1923: 8) view, "the designation of anthropology as 'the child of Darwin' is most misleading. Darwin's essential achievement was that he imagined . . . a mechanism [natural selection] through which organic evolution appeared to be taking place . . . [As a result, a] pure Darwinian anthropology would be largely misapplied biology." What had "greatly influenced anthropology," according to Kroeber (1923: 8), "has not been Darwinism, but the vague idea of evolution . . . It has been common practice in social anthropology to 'explain' any part of human civilization by arranging its several forms in an evolutionary sequence from lowest to highest and allowing each successive stage to flow spontaneously from the preceding–in other words, *without specific cause*" (emphasis added). This had been Boas's (1896, 1904) point too. Kroeber saw this Spencerian, essentialist notion as nonsense. As Bidney (1946: 295) argued a few years later, "*the concept of logical stages of cultural development presupposes the notion of the actual history of the cultural process* . . . [T]he stages of cultural development are but abstractions, useful to the student of culture, but not ultimately intelligible or explanatory of the dynamics of culture." To Kroeber (1923: 5), the development of a cultural lineage was historical, and historians "deal with the concrete, with the unique; for in a degree every historical event has something unparalleled about it . . . [Historians] do not lay down exact laws." This didn't mean that cultures do not evolve; it only meant that the Spencer-Morgan-Tylor model of essentialist cultural evolution was incorrect. Was there an alternative?

For Kroeber (1916c, 1917), culture was like a stream–it had a flow. It was heritable not in any genetic sense but rather was transmitted via learning (see also Boas 1904). Kroeber's notion of culture change was well expressed by one of his students, Nels Nelson (1932: 103), who wrote of "implements or mechanical inventions, i.e., material culture phenomena, as parts of a unique unfolding *process* which has much in common with that other process observed in the world of nature and generally called organic evolution" (emphasis in original).

Importantly, the evolution of culture was not only continuous, it also was gradual: "[A] study of the history of mechanization reveals few if any absolutely original contrivances that were not essentially the results of gradual transformation or combination of older inventions; that in reality spurts, mutants or leaps are as rare among artificial (intellectual) phenomena as among natural phenomena" (Nelson 1932: 122). Thus, there was a "mechanical culture stream" (Nelson 1932: 109).

As another student of his, Julian Steward, wrote, Kroeber "added time depth to the essentially synchronic ethnology of Boas" (Steward 1962: 203) and believed that the evolution of a cultural lineage involved a braided stream—what a Darwinist today would label a *reticulate* pattern of intersecting and diverging lineages—created by such processes as diffusion, trade, and migration. That this idea came from Boas is clear:

> There is one fundamental difference between biological and cultural data which makes it impossible to transfer the methods of the one science to the other. Animal forms develop in divergent directions, and an intermingling of species that have once become distinct is negligible in the whole developmental history. It is otherwise in the domain of culture. Human thoughts, institutions, activities spread from one social unit to another. As soon as two groups come into close contact their cultural traits will be disseminated from the one to the other (Boas 1932: 609).

Kroeber "constantly saw changes in styles as flows and continua, pulses, culminations and diminutions, convergences and divergences, divisions, blends and cross-currents by which cultures develop and mutually influence one another" (Steward 1962: 206). He did not, however, attempt to determine causes of culture change, no doubt because his mentor (Boas) had indicated that the reticulate evolution of cultures "puts the most serious obstacles in the way of discovering the inner dynamic conditions of change. Before morphological comparison [read *evolutionary synthesis;* see Boas 1896] can be attempted the extraneous elements due to cultural diffusion must be eliminated" (Boas 1932: 609). Probably as a result, Nelson (1932: 122) wrote that "Final explanations of [culture change], as well as of the driving force and the ultimate goal of culture, may be left to the philosophers." White later would have something to say about this, as well as about the explanatory power of cultural evolution.

Genetic transmission was omitted from consideration of cultural development by Kroeber and his students for two reasons: (1) cultural ideas were not genetically transmitted, and (2) the transmission and inheritance of ideas could be up as well as down between generations and also within a generation. This prompted Kroeber (1923) to avoid adopting the biological version of evolution then current—a version that, after 1900, was dominated by genetics and its attendant mechanisms such as transmission, mutation, inheritance, and speciation. Natural selection, along with phenotypic variation, was relegated to the back room; these two critical concepts would reassume their proper place in biological theory only with the synthesis of the late 1930s and early 1940s (Mayr 1982). There was another reason not to transfer Darwinian evolution as an explanatory theory to culture—the evolution of culture was reticulate, whereas the evolution of biological organisms was simply and only branching. This was made clear in the second edition of Kroeber's (1948) *Anthropology,* where he illustrated the differences between the evolution of organisms and the evolution of cultures (Figure 2.2).

The reticulate form of the evolution of cultures was recognized prior to the publication of Kroeber's textbook. Kroeber himself, in an earlier, short article titled "Historical Reconstruction of Culture Growths and Organic Evolution," had written that while a "culture complex is 'polyphyletic' [and] a [biological] genus is, almost by definition, monophyletic. . . . the analogy does at least refer to the fact that culture elements [traits] like species represent the smallest units of material which the historical anthropologist and biologist respectively have to deal with" (Kroeber 1931: 149). The stumbling block was the implicit equation of a culture with an essentialist biological concept. Today, a biological species is a population that is reproductively isolated from other such populations; in 1930, the concept was more strongly founded in the essentialist metaphysic than in the materialist metaphysic (Mayr 1982: 272). Therefore, the problem was that Kroeber chose the wrong biological unit—species—to equate with cultures, no doubt because of the lack of theory. This problem of unit choice and construction would continue to plague the adoption of Darwinism by anthropologists through the 1950s (Birdsell 1957) and into the 1990s (Dunnell 1995).

In 1930, the conceived difference between the branching evolution of organisms and the reticulate evolution of cultures had considerable impact on Americanist archaeology's use of Darwinian evolution. But so too did the notion that evolution consisted only of genetic change. To illustrate this, we now turn attention to how archaeologists of the early 20th century used the concept of evolution.

Evolution in Archaeology

A Darwinian sort of evolution played a major role within culture history virtually since its birth as a viable

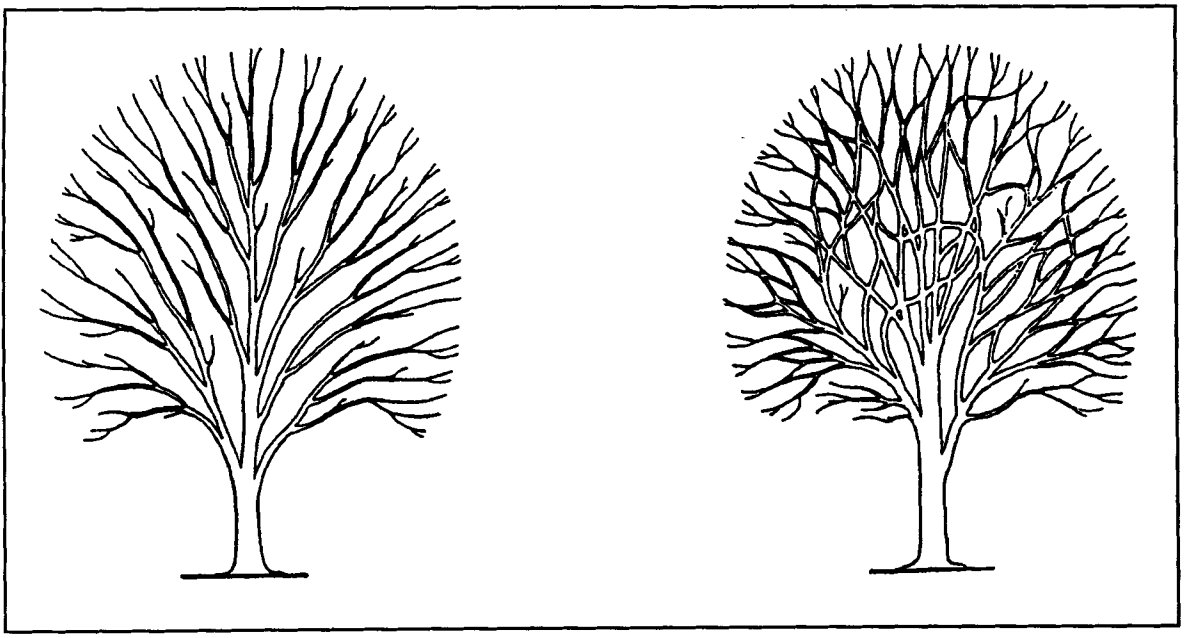

Figure 2.2. A. L. Kroeber's (1948:260, figure 18) tree of biological evolution (left) and tree of cultural evolution (right). Note the simple branching structure of the former and the reticulate (branching and intersecting) nature of the latter.

paradigm in Americanist archaeology. For example, when commenting on Max Uhle's (1907) stratigraphic excavations at the Emeryville Shellmound in San Francisco Bay, Kroeber (1909: 16) noted that most artifact types were found throughout the stratigraphic sequence and that although there was "some gradual elaboration and refinement of technical process . . . it was change of *degree* only" (emphasis added). Historians have tended to focus on the last phrase and have argued that Kroeber's vision of culture change was of the essentialist kind (Lyman et al. 1997b, Rowe 1962). Although this is correct, Kroeber clearly was aware of the fact that what he was looking at involved change of the materialist sort, though he did not recognize that his *conception* of culture change involved a particular metaphysic. He in fact measured just such materialist change a few years later when he invented frequency seriation (Kroeber 1916a, 1916b; Lyman et al. 1997b).

Two of Kroeber's students mimicked this materialist method of measuring the passage of time. Nelson's (1916) excavations in the Southwest and his use of percentage stratigraphy to show the waxing and waning of a pottery style's popularity was decidedly materialistic. Nelson's data consisted of the frequencies of ideational units termed types and showed continuous and gradual change through time. He had excavated precisely because he wanted to establish the chronological *continuity*–not just the relative temporal *positions*–of his pottery types (Lyman et al. 1997b). And he spoke, no doubt metaphorically, of types becoming extinct. Leslie Spier (1917a, 1917b) also used percentage stratigraphy and frequency seriation of ideational units to measure culture change in the Southwest. Although Nelson and Spier tended to avoid using the terms of Darwinian evolutionary theory in discussing what they documented, others were not so shy.

Kidder (1915: 453; 1917), for example, discussed different styles of pottery in terms of ancestral–descendent relationships. One pottery type might "father" another (Kidder 1915: 453), and, in wording similar to that of Nelson, a pottery type might become "extinct" (Kidder & Kidder 1917: 348). Kidder's types were of a kind that, hopefully, reflected the passage of time *as well as* evolutionary–that is, phyletic–relations, as is clearly implied in his seldom-mentioned 1917 paper on sequences, or "series," (Kidder 1917) of ceramic designs (Figure 2.3). Kidder (Kidder & Kidder 1917: 349) stated that "One's general impression is that [the types] are all successive phases of [particular pottery traits], and that each one of them developed [read *evolved*] from its predecessor." Kidder's word "phase" is a common-sense English designation for a portion of a continuum. To measure time, Kidder erected types that were both "more or less arbitrarily delimited chronological subdivisions of material" (Kidder 1936b: xxix) and "chronologically seriable" (Kidder 1936a: 625). Kidder's types were ideational units constructed to measure time: Conceptually, (1) variation was continuous–things were in the continuous process of becoming, and thus types had to be of the ideational sort to allow the measurement of change; and (2) change in

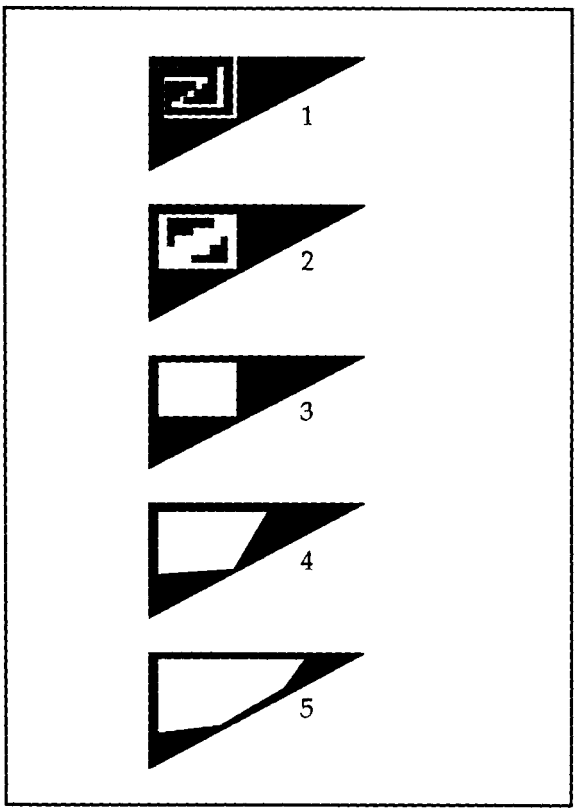

Figure 2.3. A. V. Kidder's (1917) example of a phyletic evolutionary "series" of Southwestern ceramic design elements.

the frequencies of types reflected the passage of time. This was, simply, the materialist metaphysic, but Kidder would abandon it in his Pecos volumes and elsewhere.

Numerous individuals (e.g., Dutton 1938, Ford 1935, Hawley 1934, Kniffen 1938, Martin 1936, Olson 1930, Reiter 1938b, Schmidt 1928, Strong 1925, Vaillant 1935) over the next two decades mimicked the methods and techniques of Kroeber, Nelson, Spier, and Kidder. Without explicitly recognizing it, they were using the materialist metaphysic and ideational units to document culture lineages. But they all also talked about *sections* of the materialistically measured cultural continuum—sections that were variously labeled cultures, complexes, periods, phases, and the like—as if they were real, phenomenological units (e.g., Kidder 1924, 1927). Such discussions represented what has been termed the materialist paradox (Dunnell 1995: 34). If cultures evolved, how could such units be real? Even Kroeber (1916a, 1916b), who surely must have recognized that his periods were arbitrary chunks of the continuum, treated his cultural units as if they were real, as did everyone else. What in reality were a few randomly selected frames of a reel of film—or arbitrarily chosen (they were accidents of sampling) one-foot sections of a mile-long cultural stream—conceptually came to have essentialist realities. The film had definite acts, and the stream had placid pools punctuated by brief but violent rapids.

Conflation of the materialist and essentialist metaphysics began with, among others, Kroeber (1916a, 1916b), who spoke of the sections of the continuum he distinguished in the Southwest as epochs, periods, eras, and the like. A few years later, Kroeber encountered the materialist paradox head-on. In his *Handbook of the Indians of California,* Kroeber (1925: 926) indicated that he was interested in the documentation that archaeology could provide on "the development of culture" in the state. By that time, it was clear to Kroeber (1925: 927) and other culture historians that "the correct [excavation] procedure [was] to follow lines of deposition in instituting comparisons; but this is not practical, stratification being confined to limited areas and often wholly imperceptible." After examining the shifting frequencies of various artifact types across selected vertical columns of sediment, Kroeber (1925: 926, 930) concluded that geographic subdivisions, or provinces, of the California culture area "were determined a long time ago and have ever since maintained themselves with relatively little change"; that "the basis of culture remained identical during the whole of the shell-mound period"; and that in California, "civilization, such as it was, remained immutable in all fundamentals." That his artifact types were variously functional or simply descriptive and not historical went unnoticed.

Most importantly, Kroeber (1925: 931) indicated, first, that types "must, of course, be interpreted as periods"; second, differences in types represented differences in culture; and third, cultures differed because the human groups that bore the cultures and made the artifacts differed. In short, Kroeber's types of artifacts and types of cultures were in some sense real, essentialist, units. Spier (1917a, 1917b, 1918, 1919) had interpreted his pottery data from the Southwest in just such terms, as had others (e.g., Kidder 1924, 1927; Schmidt 1928, Vaillant 1935). Kidder, for one, recognized the problem almost immediately, as revealed in the following statement:

> The division of the Glaze ware of Pecos into six chronologically sequent types is a very convenient and, superficially, satisfactory arrangement. For some time I was very proud of it, so much so, in fact, that I came to think and write about the types as if they were definite and describable entities. They are, of course, nothing of the sort, being merely useful cross-sections of a constantly changing cultural trait. Most types, in reality, grew one from the other by stages well-nigh imperceptible. My groupings therefore amount to a selection of six recognizable nodes of individuality; and a forcing into

> association with the most strongly marked or "peak" material of many actually older and younger transitional specimens. . . . This pottery did not stand still; through some three centuries it underwent a slow, usually subtle, but never ceasing metamorphosis (Kidder 1936b: xx).

Kidder's comment concerned the materialist paradox–that is, the conceived materialistic "slow, usually subtle, but never ceasing metamorphosis" of artifact forms through time–was monitored using typological (in the sense of Mayr 1959), essentialist, "*recognizable* nodes of individuality." The important point is that despite this early recognition of the paradox, the equation of types of artifacts with particular *cultures* became the interpretive algorithm for culture history. Such an equation represented the first step toward adoption of the essentialist metaphysic and would eventually lead to the rejection of any form of materialist evolution. The conceived reality of the sections of the temporal continuum was reinforced by the perceived discontinuity of the stratigraphic column represented by discrete strata because the set of artifacts within each stratum was conceived as representative of a discrete cultural occupation (e.g., Fowke 1922, Thompson 1956, Wissler 1917a). As a result, the use of strata (or arbitrary levels) as artifact-collection units not only was beneficial to Americanist archaeology (because it allowed time to be measured) but came to have a serious cost as well.

Darwinism's Last Hurrah

The basic notion of Darwinian evolution affected the interpretations of early culture historians, but it was unclear to them how to transfer such a purely genetic process–as biological evolution was then thought to be–to a nongenetic phenomenon such as culture. Nor was it clear how the merely branching pattern of biological evolution could be applied to the clearly reticulate evolution of culture. Kroeber continued to grapple with these problems and managed early in the 1930s to identify one of the significant aspects of biological evolution that should have been employed by culture historians. Kroeber (1931: 151) pointed out that the "fundamentally different evidential value of homologous and analogous similarities for determination of historical relationship, that is, genuine systematic or genetic relationship, has long been an axiom in biological science. The distinction has been much less clearly made in anthropology, and rarely explicitly, but holds with equal force." Kroeber (1931: 151) went on to imply that a "true homology" denoted "genetic unity," and he argued that

> . . . there are cases in which it is not a simple matter to decide whether the totality of traits points to a true [homologous] relationship or to secondary [analogous, functional] convergence . . . Yet few biologists would doubt that sufficiently intensive analysis of structure will ultimately solve such problems of descent . . . There seems no reason why on the whole the same cautious optimism should not prevail in the field of culture; why homologies should not be positively distinguishable from analogies when analysis of the whole of the phenomena in question has become truly intensive . . . [S]uch analysis has often been lacking but judgments have nevertheless been rendered (Kroeber 1931: 152–153).

Kroeber's (1931) remarks were largely ignored, leading him to lament over a decade later that anthropology was still "backward" with regard to distinguishing between analogous and homologous similarities (Kroeber 1943: 108). Instead of implementing Kroeber's suggestions, archaeologists adopted the rather more easily conceived and implemented dictum that "typological similarity is [an] indicator of cultural relatedness" (Willey 1953: 363). A not-so-liberal interpretation of this notion is found in the morphological species concept of early 20th-century biologists who still adhered to the essentialist metaphysic: Morphologically similar species were members of the same taxon or type and thus were phylogenetically related (Mayr 1982: 270). This notion was axiomatic in culture history (Willey 1953: 363), and in both contexts it put the cart before the horse. As George Gaylord Simpson (1961: 69) pointed out some years later, "individuals do not belong in the same taxon because they are similar, but they are similar *because they belong to the same taxon*" (emphasis added). In other words, archaeologists noted similarities between artifact types, assemblages of artifacts, and the like, but those similarities might not be of the homologous sort requisite to determining phylogenetic relations and writing phylogenetic histories (e.g., Szalay & Bock 1991). That some archaeologists were misled by Willey's axiom is clear. Not everyone was misled, but, as we discuss later, the objections of this minority ultimately led to the rejection of any potential archaeological utility of Darwinian evolution.

Ceramic and Cultural Phylogenetics

Shortly after Kroeber's (1931) paper was published, the Darwinian notion of evolution took on a form within Americanist archaeology that prompted several commentators to reiterate Kroeber's reasons for rejecting Darwinism as a viable theoretical model for the explanation of culture change. Harold S. Gladwin, working in the Southwest, created not only a binomial system of pot-

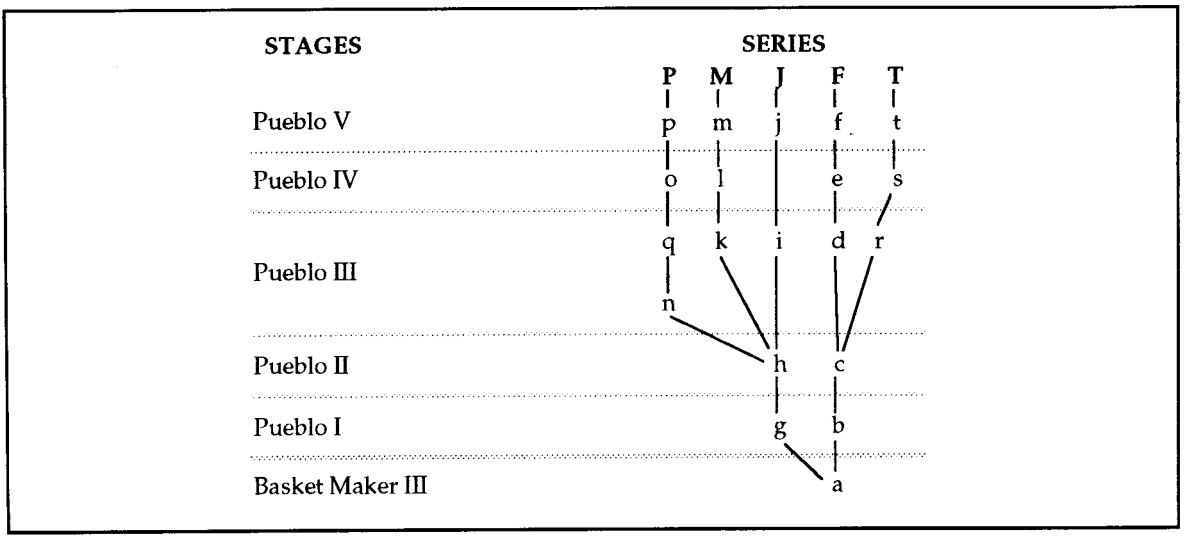

Figure 2.4. Colton and Hargraves's (1937:4 figure 1) model of the phylogenetic evolution of pottery. Each upper-case letter represents a ceramic "series" in the sense of Kidder (1917; see Figure 2.3); each lower-case letter represents a type; phylogenetic relations of types are indicated by the vertical lines connecting lower-case letters. Colton and Hargraves's figure caption reads, in part, "type c is collateral to types d and r, derivative [descended] from type b, and ancestral to types f and t . . . types q and n are both ancestral to type o, but collateral to each other, and derivative from types h, g, and a."

tery classification explicitly modeled on the biological genus-and-species concept (Gladwin & Gladwin 1930), but he also created a hierarchical structure for organizing archaeological units variously termed roots, stems, branches, and phases (Gladwin & Gladwin 1934). The latter were meant specifically to assist in the working out of "a comprehensive scheme by which relationships and relative chronology could be expressed" (Gladwin & Gladwin 1934: 8-9). In a few short years, Gladwin (1936) abandoned the scheme. His statement to that effect is revealing:

> My original suggestion . . . of using a generic [read *genus*] and a specific [read *species*] name for pottery types implied a biological analogy which I now think was a mistake. The idea is being carried too far along biological or zoological lines, and men do not realize the profound differences which exist between zoological species and the things which have been made by men and women.
>
> Zoological species do not cross and intergrade; evolution is so slow as to be hardly distinguishable. The evolution of culture . . . was stepped up to almost incredible speed, and on every side we find evidence of merging and cross-influences (Gladwin 1936: 158).

The individuals who were carrying the idea too far no doubt included Harold S. Colton, a professional biologist by training who left a professorship of biology to pursue archaeological interests. Along with Lyndon L. Hargrave, Colton published a major statement on the phylogenetic implications of pottery types. A *type* was "a group of pottery vessels which are alike in every important characteristic except (possibly) form," and a *series* was "a group of pottery types within a single ware in which each type bears a genetic relation to each other" (Colton & Hargrave 1937: 2-3). Thus, Colton and Hargrave's series was identical to Kidder's (1915, 1917). But Colton and Hargrave carried the biological analogy further than their predecessors had and distinguished among derived, collateral, and ancestral types and graphed the relations among them (Figure 2.4). Although Colton's knowledge of Darwinian evolution no doubt underpinned their scheme, the key to it was in the supposition that related forms were related because they were similar. As we pointed out above, it should be the other way around—similar forms are similar because they are related. The problem was that Colton and Hargrave offered no argument—nor did anyone else at the time—for the belief that the similarities described were of the homologous sort.

Kidder's (1915, 1917) simpler phyletic scheme (Figure 2.3) denoted some of the same sorts of relations between types, but his contemporaries did not heap criticism on him the way they did on Colton and Hargrave. In his review of their work, James Ford (1940: 265) noted that Colton and Hargrave had ignored the problem of selecting "a class of features [attributes] which will best reflect cultural influences [e.g., transmission via contact or heredity], and which in their various forms will be mutually exclusive, to serve as guides in the process" of determining ancestral–descendent relations. Ford was,

without using the term, speaking of homologous similarity, but he failed to make this clear. In another review, Paul Reiter (1938a: 490) noted that he "was unable to find a single instance of proof of [the genetic relations of pottery types]." Colton and Hargrave (1937: 2–3, 5) indicated that genetic relations among types were "obvious" and "clearly revealed" and that "definite evolutionary characters were recognized," but they provided no list of them, nor did they provide guidance as to how such characters were to be analytically identified. They apparently were unaware of Kroeber's (1931) guidance in this regard.

Reiter (1938a: 490) also noted that Colton and Hargrave's pottery typology tended to ignore variation, and he insisted that "variation tendencies cannot be overlooked if genetic or chronologic emphasis is strong." Reiter's comments were on the mark–they underscored that a materialist conception of change was requisite to what Colton and Hargrave were attempting. But Colton and Hargrave's types were not ideational units that could be used to measure variation. The types were empirical, as evidenced by Colton and Hargrave's (1937: 30–31) suggestions that the members of a type "will not [always] fit the [type] description perfectly" and that "there are sherds that are intermediate between types."

Colton and Hargrave's classification scheme was hierarchical, but that structure was void of explanatory content. In another monograph, Colton (1939) employed the terms then becoming popular within the Midwestern Taxonomic Method, an organizational method explicitly built to be void of explanatory content (McKern 1934, 1937, 1939). As the name of the method implied, the structure of the system was hierarchical. In combination with the use of the Midwestern Taxonomic Method's terminology, then, Colton's (1939) phylogenetic interpretations, not surprisingly, were criticized. Reviewers focused on Colton's interpretations rather than on his methods. Erik Reed (1940:190) thought the scheme was a reasonable one and remarked that Colton's "genetic and temporal approach seems more desirable in a region such as this where chronology is relatively well-known." Chronological control is surely a requisite of determining a phylogenetic history, but it is only one of several requirements. Reed's failure to note that Colton had not established that the typological similarities he discussed were of the homologous sort was, however, typical rather than anomalous.

Not everyone overlooked the point, however. Julian Steward rejected Colton's analytical procedure and the results for the same reason that Kroeber had earlier rejected Darwinian evolution–it was nonapplicable to cultural phenomena:

> It is apparent from the cultural relationships shown in this scheme that strict adherence to a method drawn from biology inevitably fails to take into account the distinctively cultural and unbiological fact of blends and crosses between essentially unlike types . . . It is true that cultural streams often tend to be distinct, but they are never entirely unmixed and often approach a complete blend. A taxonomic scheme cannot indicate this fact without becoming mainly a list of exceptions. It must pigeon-hole . . . [T]he method employed inevitably distorts true cultural relationships (Steward 1941: 367).

Steward simply couldn't accept a taxonomic structure for any classification of cultural phenomena because such a structure implied the same thing to him that the Linnaean taxonomy tended to imply to some biologists–phylogenetic or branching evolution. This is clear in Steward's (1942, 1944) discussion of the Midwestern Taxonomic Method. Steward (1942: 339) offered the typical criticism of that method when he noted that it produced a "set of timeless and spaceless categories." In response, McKern (1942) protested that he had merely set those dimensions aside for the moment in favor of the formal dimension and had explicitly *not* discarded the time-space dimensions.

Steward's (1944: 99) rebuttal entailed two elements and is where the heart of the matter resides. He simply could not conceive of how taxonomic or hierarchical classification, which for him denoted branching evolution, could be forced onto cultural phenomena that not only branched or diverged through time but that also converged to create a reticulate form of evolutionary descent. Steward's implicit equation of species and cultures–both conceived as essentialist units–is clear. Second, Steward could not find any utility in the method because it seemed to lack any reference to theory. The latter was one reason numerical phenetics, also termed numerical taxonomy, fell from favor in biology several decades later (Mayr 1969), but Steward's observation had minimal impact on archaeological thought. Interestingly, the most damaging criticism of what had begun as Kidder's (1917) phyletic series of ceramic designs and what eventually grew into Colton and Hargrave's (1937) collateral and descendent types resided elsewhere.

The Paradox

J. O. Brew (1946: 46) argued that classifications are arbitrary constructs of the analyst–"no typological system is actually inherent in the material"–and that a classification should fit the purpose of the investigation. He clearly recognized the interplay of theory and unit con-

struction and argued that we should change our classifications "as our needs change and as our knowledge develops" (Brew 1946: 64). His conception of cultural change was the same as Kroeber's: "We are dealing with a constant stream of cultural development, not evolutionary in the genetic sense, but still a continuum of human activity" (Brew 1946: 63). In holding to such a materialist conception of culture change, Brew (1946: 65) argued, we "must ever be on guard against that peculiar paradox of anthropology which permits men to 'trace' a 'complex' of, let us say, physical type, pottery type, and religion over 10,000 miles of terrain and down through 10,000 years of history while in the same breath, or in the next lecture, the same men vigorously defend the theory of continuous change." The paradox emanates "from the belief that the manufactured groups [types] are realistic entities and the lack of realization that they are completely artificial. . . Implicit in [the belief] is a faith . . . in the existence of a 'true' or 'correct' classification for all object, cultures, etc., which completely ignores the fact that they are all part of a continuous stream of cultural events" (Brew 1946: 48).

Although the term was not in general use, no one noticed that Brew had described the materialist paradox. What *was* taken notice of was Brew's (1946: 53) observation that the evolutionary implications of the Gladwin–Colton scheme were unacceptable for the simple reason that "phylogenetic relationships do not exist between inanimate objects" such as pot sherds. This statement was repeated by Beals et al. (1945: 87), who saw Brew's manuscript before it was published. Ironically, they mimicked Kidder's (Figure 2.3) technique of constructing ceramic series, as did others (e.g., Wheat et al. 1958). Brew's discussion nonetheless had one apparent impact on Americanist archaeology–to drive the final nail into the coffin containing the Gladwin-Colton scheme; no one after that time discussed ceramic series in such flagrantly Darwinian terms. That this was the singular impact of his arguments probably resulted because Brew could offer no clearly articulated alternative. For example, he stated that the "only defense there can be for a classification of [artifacts] based upon phylogenetic theory is that the individual objects were made and used by man" (Brew 1946: 55), but he failed to make the conceptual leap to "replicative success" (Leonard & Jones 1987)–the cornerstone of a Darwinian archaeology–for two reasons.

First, to Brew, evolution involved only the processes of genetic transmission and genetic change. There was, then, only a weak correlation between an organism and the "artifacts" that that organism might produce, such as birds and egg shells or molluscs and mollusc shells, and no connection at all between people and their artifacts (Brew 1946: 55–56). Since artifacts were not genotypic phenomena, they were not subject to evolutionary forces. That it is the phenotype–of which artifacts are an expression–that undergoes selection escaped Brew's–and contemporary biologists'–attention for a number of years (Leonard & Jones 1987, Mayr 1982). Second, Brew quoted a single biologist–a geneticist–who argued that a phylogenetic (or phyletic) history did not *explain* organisms; hence, to Brew (1946: 56), it could hardly explain artifacts: "This is a most important point, and I wish to emphasize it here." Of course a phylogenetic history is *not* an explanation, but such a construct *is* a requirement of using Darwinian evolutionary theory to explain the diversity of forms of organisms and their distributions in time and space (Szalay & Bock 1991).

THE REBIRTH OF CULTURAL EVOLUTION

Brew's (1946) and Steward's (1941, 1942, 1944) devastating criticisms of any suggestion that the evolution of cultures might have some similarities to Darwinian evolution and be discussed using Darwinian terms essentially killed any such notions from being stated explicitly. Archaeologists still used Kidder's (1917) phyletic-seriation technique (e.g., Beals et al. 1945, Wheat et al. 1958), but they were not explicit about the Darwinian implications of the results. Cultures still came in contact–the evolution of cultures was reticulate–but biologists would not come to recognize how often Darwinian evolution took that form until the 1980s, when studies of plant evolution became more rigorous and studies of animal evolution became more detailed (e.g., Davis 1996, Szalay & Bock 1991, Weiner 1994). Rather than retooling materialist evolution into something that was applicable to archaeological phenomena and that was aligned with their materialistic conceptions of culture change–just as paleobiologists had done to make it applicable to the fossil record (fossil bones, like pots, don't interbreed)–culture historians did something else. They adopted both Willey's (1953: 363) axiom that "typological similarity is [an] indicator of cultural relatedness" and a reborn version of Spencerian cultural evolution. There were problems with both, but the adoption was not a long and painful one because there was no effective competition; the only potential competitor had been eliminated, and the winner was therefore chosen by default.

Typological Similarity and Homologous Similarity

Willey (1953) disliked the Gladwin-Colton scheme, and when he and Philip Phillips published "Method and Theory in American Archaeology: An Operational Basis

for Culture-Historical Integration" in 1953, they explicitly stated that the archaeological "use of the organic evolutionary model is, we believe, specious" (Phillips & Willey 1953: 631). Their basic operative unit was a *phase*—an archaeological manifestation of a culture that had some ethnographic reality. They suggested that the use of *traditions* and *horizon styles* would reflect the braided stream of the evolution of cultures and allow one to correlate phases. As Irving Rouse (1954: 222) noted, a metaphor could be drawn between the use of horizons and traditions as integrative devices for archaeological materials distributed across an area and a rectangular piece of cloth, the side edges representing the geographical limits of the area and the top and bottom edges representing the temporal limits:

> The warp threads of the cloth consist of a series of regional traditions running from the bottom towards the top of the cloth, while the weft is composed of a number of horizon styles which extend from one side of the cloth towards the other. The cloth is decorated with a series of irregularly arranged rectangles, each representing a single culture [read *phase*], and these are so colored that they appear to form a series of horizontal bands (Rouse 1954: 222).

But Rouse (1955) was concerned that the use of Willey's dictum that typological similarity denoted cultural relatedness was too simplistic. Rouse wanted to determine the phylogenetic relations among phases rather than merely to track the distributions of a few artifact classes, as the horizon and tradition units did. To illustrate this, he distinguished three ways to correlate phases. First, one might use a Midwestern Taxonomic Method-like procedure to group phases that shared traits. Why the traits were shared was a separate issue. Second, one could note similarities in the time–space distributions of phases. Identical or adjacent distributions of two or more phases "establishes contemporaneity and contiguity, or lack thereof, and nothing else" (Rouse 1955: 717). To argue that contemporaneous phases were phylogenetically related "because they share a given horizon style . . . is on the genetic rather than the distributional level of interpretation, for it requires an assumption that the style has diffused from one phase to the others with little or no time lag" (Rouse 1955:718). Third, one might trace the "genetic" relations among phases by establishing that the phases had been in "contact" temporally and spatially by using horizons and traditions that comprised homologous types (Rouse 1955:719). One needed to distinguish between analogous and homologous similarity, the linchpin to this kind of comparison, to ensure the relations were "genetic." The modeled result of such analyses is shown in Figure 2.5.

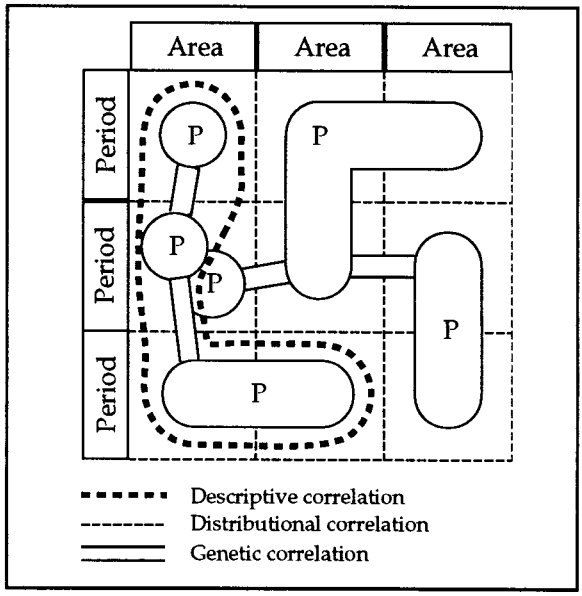

Figure 2.5. Irving Rouse's (1955:720, figure 4) model of the results of comparing phases (P) to show their relative contiguity in form, time, and space. Phases that were formally similar as well as contiguous in time and space had "genetic" relations.

Willey and Phillips (1958: 31) responded to Rouse (1955) by arguing that his "genetic" relations could only "be revealed and expressed by means of integrative concepts that are culturally determined." The phrase "culturally determined" was critical. Horizons and horizon styles by definition reflected cultural transmission or diffusion over space. A tradition was "a (primarily) temporal continuity represented by persistent configurations in single technologies or other systems of related forms" that operated at Rouse's (1955) "genetic level of interpretation" (Willey & Phillips 1958: 38) and reflected transmission or heredity across time. Thus, Willey and Phillips's conception of cultural development was well captured by the flowing braided-stream metaphor. Each trickle was a tradition that to varying degrees met and mixed with other trickles as denoted by horizons and horizon styles. Such a conception presumed that the typological similarities denoted by horizons and traditions were of the homologous sort.

For Willey and Phillips, horizons and traditions provided the empirical warrants for discussing the historical development of cultures. They were "integrative" units that denoted "some form of historical contact" *rather than* "implications of phylogeny" (Willey & Phillips 1958: 30). Culture history demanded "culturally determined" integrative concepts such as horizons and traditions, not phylogenetic ones (Willey & Phillips 1958: 30–31). Willey's (1953: 368) suggestion that "principles of continuity and change are expressed in the degrees of trait likeness and unlikeness which are the mechanisms of es-

tablishing the *genetic* lines binding the assemblages together" (emphasis added) thus was purely metaphorical. But cultural or historical "relatedness," when couched in a temporal framework aimed at studying the developmental lineages of cultures, such as that envisioned by Willey and Phillips, cannot fail to be phylogenetic in the sense of Darwinian evolution.

The generally discipline-wide abhorrence of anything Darwinian and/or genetic resulted in the contradiction internal to the Willey-Phillips scheme going unrecognized. Further, Kroeber's (1931, 1943) earlier critical point regarding the importance of the distinction between analogous and homologous similarity, even though repeated by Rouse (1955), was overlooked. It was, we suspect, overlooked at least in part because there was an alternative version of evolution that (1) did not require this critical distinction, rendering it pointless to consider; (2) did not entail genetic transmission, rendering further consideration of the mechanisms of heritability and thus consideration of continuity unnecessary; and (3) did not consider whether evolution was merely branching or reticulate, rendering this problem unworthy of further discussion. That version of evolution was Spencerian, or what we have termed *cultural,* and it had been lurking within anthropology since the early 1940s, precisely when Brew, Steward, and others were refuting the applicability of Darwinism to cultural phenomena.

LESLIE WHITE'S BRAND OF CULTURAL EVOLUTION

The 1940s witnessed the rebirth of cultural evolution within anthropology, initially at the hands of White (1943, 1945a, 1945b, 1947, 1949: 363–393) and later with contributions by Steward (1949, 1951, 1953). Not surprisingly, Kroeber was not impressed, and he and White exchanged broadsides into the late 1950s (Kroeber 1946, 1960; White 1945a, 1945b, 1947, 1959a, 1959b). A detailed analysis of their debate is beyond the scope of this paper, but several aspects of their exchange are significant in the present context. These do not entail the famous dictum of White's (later adopted by the processual archaeologists of the 1960s) that culture is humankind's extrasomatic means of adaptation, nor do they entail the statement by White (1959b: 30) that in his view history is ideographic and evolution nomothetic. Rather, the important points are that Kroeber's view of culture change was more in line with the materialist metaphysic, whereas White's was strongly within the essentialist camp. Detecting this critical difference is difficult because of the manner in which the two antagonists distinguished between history and evolutionism.

Although both conceived of cultural change as involving transmission and heritability and referred to it metaphorically as a flowing stream or continuum, White (1945b) and Kroeber (1946) viewed history and evolution very differently. White (1938) viewed specific events as varying formally—that is, structurally and/or functionally—with each event occupying a particular position in the time-space continuum. Kroeber (1946) apparently agreed. How these three dimensions—to borrow Spaulding's (1960) term—of form, space, and time were analytically interrelated is how White distinguished between history and evolution: "Events are related to each other spatially, and we may deal with [them] in terms of spatial, or formal, relationships, ignoring the aspect *time*" (White 1938: 375). White (1945b: 222) later termed this the "formal (functional) process, which presents phenomena in their non-temporal, structural, and functional aspects." Formal-functional aspects of events could be "repetitive," by which White (1945b: 229) meant different events as phenomena could have "generic likenesses." In our terms, events could be classified according to a set of ideational units, and thus while each event as a phenomenon occupied a unique time–space position, events as classes shared certain features in common. As classes or ideational units, events had distributions; that is, a class of event could occur in more than one time-space location.

In White's (1945b: 222) view, history concerned "non-repetitive" events: "History is that way of sciencing in which events are dealt with in terms of their temporal relationships alone. Each event is unique. The one thing that history never does is repeat itself" (White 1938: 374); the "temporal process [history] is a selective arrangement of events according to the principle of *time*" (White 1938: 376); the historic process is the one "in which specific and severally unique events take place in a purely temporal context" (White 1938: 380). White (1945b: 222) later referred to this as the "temporal process, being a chronological sequence of unique events, the study of which is history." In our terms, history concerned a set of empirical units—labeled events by White—arranged in a temporal sequence, each event in a particular spatial position. Because events were empirical, they had locations in the time-space continuum.

Evolution, in White's view, was distinct from history: "The temporal-spatial process is an evolutionary, or developmental process . . . Evolution is temporal-alteration-of-forms" (White 1938: 377). The "historic process [dimension] is merely temporal, the evolutionary process is formal as well: it is a *temporal-sequence-of-forms*" (White 1938: 379). The evolutionary process involves "new forms grow[ing] out of preceding forms" (White 1938: 380). White (1945b: 222) later described this as the "temporal–formal process, which presents phenomena

as a temporal sequence of forms, the interpretation of which is evolutionism." Evolution was not a "chronological sequence of particular and unique events [this was history], but [rather] a general process of chronological change, a temporal-sequence-of-forms, with the growth of one form out of an earlier, into a later, form" (White 1945b: 224). Thus, evolution was different than history: "[T]he historic process and the evolutionist process are alike in that both involve temporal sequences. They differ, however, in that the historic process deals with events determined by specific time and space coördinates, in short with *unique* events [empirical units], whereas the evolutionist process is concerned with *classes* of events [ideational units] independent of specific time and place" (White 1945b: 230).

Kroeber (1946) simply couldn't understand White's distinction between history and evolution. His confusion no doubt arose from several sources, one being that White (1945b: 222) indicated both that evolution was "non-repetitive," suggesting that evolutionary events were unique, empirical units rather than ideational classes, and that evolution concerned "*classes* of events" (our ideational units) (White 1945b: 230, 238). Perhaps White merely misspoke here, because it is relatively straightforward to see in the bulk of his discussions that in the formal dimension events were ideational units, in the historical (temporal) dimension events were empirical units, and in the evolutionist view events were ideational. For example, in more than one place White (1945b: 239) indicated that the interests of the historian were in "the unique event at a specific time and place," whereas those of the evolutionist were in "a class of events."

To Kroeber (1946), history and evolution were one and the same–a point accepted by most biologists today (e.g., Gould 1986, Szalay & Bock 1991). In fact, the difference between Kroeber and White was their respective metaphysics. Kroeber (1946: 9) observed that apparently "what White means by evolution is a fixed, necessary, inherent, and predetermined process . . . White's evolution thus seems to be an unfolding of immanences." By the last we suspect Kroeber had in mind what we have termed essentialist units. This is important, for it comprises a much more significant source of confusion for Kroeber and source of disagreement between him and White. What White was speaking of is known in biology as the theory of *orthogenesis*–a point that was made by at least one anthropologist, Joseph Birdsell (1957), and an evolutionary biologist, Theodosius Dobzhansky (1957), at the time White was writing. Birdsell (1957: 399) noted that White was a "modern advocate of the orthogenetic evolution of culture." In the hands of Spencer, Morgan, and Tylor, the orthogenetic evolution of cultures was a "single inflexible and limited theory of culture change [that had] left scars on 20th century anthropology . . . [An] unreasonable amount of time and energy had been spent [by White and Steward] on beating [this] dead and specialized theory of evolution." Why was Birdsell so concerned?

Dobzhansky (1957: 382–383), in a companion article to Birdsell's, noted that orthogenetic evolution consisted of

> unfolding or manifestation of pre-existing rudiments"; there is in it nothing accidental or creative [no mutation], for evolution "proceeds in accordance with laws," through a predetermined sequence of stages or phases . . . Theories of orthogenesis represent evolution as unfolding of pre-existing but latent forms [Kroeber's immanences] . . . [An] idea popular among believers in orthogenesis is that the evolution of most phyletic lines tends towards evolutionary senility and extinction. If we were to accept this idea then all we can hope to do for our descendants is to postpone the inevitable.

When discussing his view of evolution, White repeatedly said that (1) "new forms grow out of preceding forms" (1938: 380, 1945b: 224, 1947: 175); (2) the evolutionary process was law-like (1949, 1959b); and (3) the sequence of stages was inevitable in the sense that all societies would eventually represent civilizations, whether they all were at one time chiefdoms or not (1947, 1959b).

It is not difficult to perceive essentialism in White's (1943) seminal discussion of his view of orthogenetic cultural evolution. It is even easier to see in his discussion of evolutionary stages: "For those who recognize that one form grows out of another, the concept of stages will be found useful as a descriptive, interpretative, and evaluative device . . . [Stages] serve to mark off steps in development . . . Stages are merely the succession of significant forms in the developmental process" (White 1947: 179). What made a stage "significant" was never made explicit by White, but it no doubt was the fact that metaphysically it was a real, essentialist category. Steward's (1955: 89) "cultural core" and "cultural type" were also essentialist units. Every anthropologist recognized a hunter-gatherer economy or tribal-level social organization or the like; these sorts of anthropological phenomena must therefore be real (recall Morgan's tripartite scheme of savagery, barbarism, and civilization). When such cultural traits occurred in particular combinations–and they seemed to covary in nonrandom fashion–they comprised a certain evolutionary stage–a point later shown to be fallacious by Leonard and Jones (1987; see also Rambo 1991). In short, this was an essentialistic view. Kroeber and other Boasians couldn't fathom what White was talking about because they held a rather more mate-

rialistic view, even though they seldom recognized that the materialist paradox tended to consistently thwart their explanatory efforts.

The cultural-evolutionary process was another matter of concern. Despite White's (1943: 339) disclaimer that he was not saying that "man deliberately set about to improve his culture," close reading of what he said indicates that he strongly believed all organisms, including humans, had an "urge" to improve and that *this* was the "motive force as well as the means of cultural evolution." In what we view as a maximal overstatement, White (1943: 339) proclaimed that the "culturologist knows more about cultural evolution than the biologist, even today, knows about biological evolution." White (1947: 177) also regularly indicated that he and other cultural evolutionists "did not identify evolution with progress [and that they] did not believe progress was inevitable."

But by default, cultural evolution was synonymous with progress: "[B]y and large, in the history of human culture, progress and evolution *have gone hand in hand*" (White 1943: 339). The key evolutionary mechanism–urge or necessity as a motive force–demanded absolutely no reference to a source of variation, to natural selection, or to the shape of lineages–all of which are of critical interest in Darwinian evolution. In White's view, every human invented new tools as necessary, and they were always better than the preceding ones because they allowed the procurement or exploitation of additional energy:

> The best single index [of progress] by which all cultures can be measured, is *amount of energy harnessed per capita per year.* This is the common denominator of all cultures... Culture advances as the amount of energy harnessed per capita increases. The criterion for the evaluation of cultures is thus an objective one. The measurements can be expressed in mathematical terms. The goal–security and survival–is likewise objective; it is the one that all species, man included, live by. Thus we are able to speak of cultural progress objectively and in a manner which enriches our understanding of the culture history of mankind tremendously. And finally, we can evaluate cultures and arrange them in a series from lower to higher. This follows, of course, from the establishment of a scientifically valid criterion of value and means of measurement (White 1947: 187).

If you don't think this sounds like the new, or processual, archaeology of the 1960s, then read the first thirty pages of White (1959b).

What gave White's evolution its distinctive punctuated form was his belief that change could occur only two ways: Either humans improve the efficiency of old tools, or they invent new tools. Evolution via the former was limited, however, as exemplified in White's (1943: 343) statement that the "extent to which man may harness natural forces [energy] in animal husbandry is limited" and his later statement that "some progress can of course be made by increasing the efficiency of the technological means of putting energy to work, but there is a limit to the extent of cultural advance on this basis" (White 1959b: 369). This is merely another expression of orthogenetic evolution, as the quote from Dobzhansky (1957) cited earlier makes clear.

It was the technological breakthroughs or revolutions that were important and resulted in "tremendous" changes, "extremely rapid" progress, and "great cultural advances" (White 1945b: 342, 344). Such breakthroughs gave cultural evolution its jerky, discontinuous appearance. The other sort of change–mere improvement of the efficiency of existing tools–produced "no fundamental difference" (White 1945b:344). To Kroeber, the evolution of a cultural lineage was continuous and gradual, like a gradually ascending ramp (Figure 2.6); to White, cultural evolution was not a ramp but a staircase, perhaps with each step at a slight incline to reflect the fact that existing technology was constantly being improved, but the risers were most significant because they represented the technological breakthroughs and the fundamental differences between what came to be referred to as stages (Figure 2.6).

Archaeologists opted for White's version, with its cultural stasis punctuated by relatively brief and abrupt change, because it showed that cultural change was discontinuous. This fit the discontinuous nature of change evidenced not only by stratigraphically superposed artifact assemblages (Lyman et al. 1997b) but also by the fact that, as Raymond Thompson (1956: 36) pointed out, "artifacts tend strongly to occur in spatial clusters." Hence, "the site provides a unit of artifact association for analysis and synthesis.... [F]orm classes of artifacts and a unit of artifact association provide the necessary data for quantitative description and comparison" (Thompson 1956:36). This fit well with White's (1947:175) notion that as a cultural evolutionist, an archaeologist "would begin, naturally, with the present, with what we have before us. Then we would arrange other forms in the series in accordance with their likeness or dissimilarity to the present form [Kidder's ceramic series].... Stratigraphy is often involved here."

DISCUSSION

In 1953, Steward reiterated the typical objections to the application of Darwinian evolution to cultural phenomena:

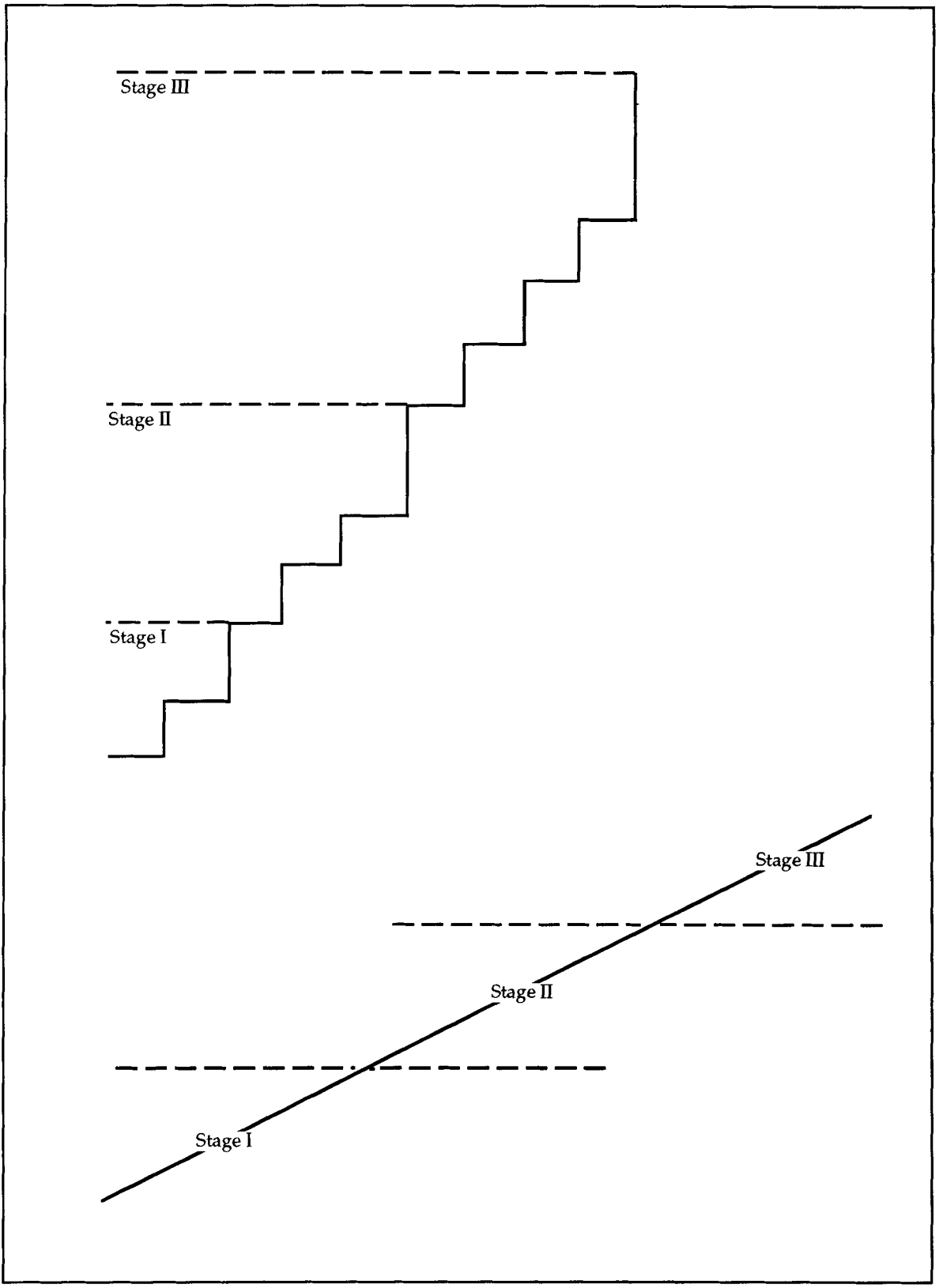

Figure 2.6. A model of essentialist cultural evolution (upper) and a model of materialist Darwinian cultural evolution (lower). In the latter, the term "stage" is metaphorical, and stage boundaries are arbitrarily assigned by the analyst.

> [C]ultural evolution is an extension of biological evolution only in a chronological sense. The nature of evolutionary schemes and of the developmental processes differs profoundly in biology and culture. In biological evolution it is assumed that all forms are genetically related and that their development is essentially divergent [branching] . . . In cultural evolution, on the other hand, it is assumed that patterns are genetically unrelated and yet pass through parallel and historically independent sequences (Steward 1953: 313).

Steward (1953: 315) went on to suggest that the use of cultural evolution as an explanatory model demanded two "vitally important assumptions. First, it [assumes] that genuine parallels of form and function develop in historically independent sequences or cultural traditions. Second, it explains those parallels by independent operation of identical causality in each case." Cultural evolution was concerned with generalities of processes and change—that is, with cross-cultural regularities or laws—and was therefore scientific (Steward 1953: 315; White 1945b, 1959b). These aspects of cultural evolution no doubt are what made it so attractive to the processual archaeologists of the 1960s and '70s. Culture historians had adopted it a decade earlier (Krieger 1953, South 1955, Spaulding 1955, Willey & Phillips 1955, 1958) because it was the only alternative, with Darwinian evolution having been discarded as a result of its conceived inapplicability to cultural phenomena.

1950s Cultural Anthropology and Evolution

The second "assumption" of cultural evolution—that causality was identical in case after case—was part of the orthogenetic version of evolution and denied any role for natural selection. Such a mechanism was unnecessary, as White's discussions indicated. Steward (1953, 1955, 1956) advocated multilinear evolution as distinct from what he characterized as the unilinear evolution of White, but there really was no significant difference, as White repeatedly (1945b, 1959a, 1959b) indicated. We suspect that Steward never had White's distinction of history and evolution clear in his mind, just as Kroeber hadn't. What is important here is that Steward, like White, saw "order" (Steward 1956: 73) in cultural evolution; this was typically glossed as "progress"—a notion that several individuals (e.g., Dunnell 1988, Mayr 1988, O'Dowd 1982) have shown to be inappropriate in the biological and cultural realms. Given such a connotation of the apparent order of evolutionary change, Steward, again like White, perceived no role for natural selection in determining the trajectory of a culture's lineage.

Such notions often were reinforced by some biologists writing in the mid-twentieth century. For example, Julian Huxley (1956), in an address delivered to anthropologists in 1951, implied that because cultural evolution was superorganic and involved the psychosocial realm, it could be directed by human intent. Paleontologist George Gaylord Simpson agreed (1949: 344-345). Huxley also reinforced White's views that (1) "every biological improvement appears eventually to reach a limit," such that coincident with the time the first culture-bearing hominids appeared, "biological evolution on this planet had reached the limit of its advance" (Huxley 1956: 6)—a patently orthogenetic statement; (2) "evolution includes advance, or improvement in organization" (Huxley 1956: 5)—a progressionist statement; and (c) in cultural evolution, just as in biological evolution, "major advance proceeds by large steps, each marked by the spread of the successful new type of organization" (Huxley 1956: 10)—a baldly essentialist stance. Thus evolution was jerky and consisted of one dominant type or group being "replaced" by another "related but improved type" (Huxley 1956: 6).

Kroeber (1960: 15) approved of how Huxley (1956) distinguished between biological and cultural evolution. Huxley (1956: 3) indicated that he did "not believe that any purely biological concepts and principles can be immediately applied or directly transferred to anthropology"—a notion that, as we have seen, was strong in Kroeber's (and Boas's and Steward's) thinking from the start. Kroeber also appreciated that Huxley made several other observations that were in line with his own thinking:

> The evolutionary approach in anthropology has been bedeviled by false starts and false premises—notably the erroneous idea that biological evolution could be represented by a single straight line of inevitable progress, [and this created] an evolutionary strait-jacket for culture (Huxley 1956: 15).
>
> Culture was not only that which was transmitted, it was also the mechanism of transmission and thus of reproduction (Huxley 1956: 9).
>
> [E]volution still works in man, but overwhelmingly as a cultural, not a biological process. . . . [C]ultural (psycho-social) evolution shows the same main features as biological evolution. Cultures advance, progress, diverge, and stabilize, but the mechanisms of change are different, and cultures tend to converge, whereas biological evolution involves only divergence (Huxley 1956: 23–24).

Huxley's ideas were, of course, expressed before explicit recognition within biology of the distinction between the materialist and essentialist metaphysics. Rec-

ognition of the effects of the essentialist metaphysic on biology came initially in the 1950s and was discussed at length by biologists during that decade and following ones (e.g., Hull 1965; Mayr 1959, 1972, 1982; Simpson 1961, Sober 1980). By the time the distinction had taken a fairly strong hold in biology, it was too late to have any influence within anthropology and archaeology. The distinction was made explicit in the latter two disciplines only in 1982 (Dunnell 1982). Despite the increasing number of attempts to apply an evolutionary perspective to anthropological and archaeological research (e.g., Boyd & Richardson 1985, Cavalli-Sforza & Feldman 1981, Durham 1991, Marks & Staski 1988), the critical role one's metaphysic plays in research remains generally unrecognized (Mithen 1989, Schiffer 1996, Tschauner 1994; although see Clark 1991, 1993, 1994; Clark & Lindly 1991). (The same has been argued for biology [Szalay & Bock 1991].)

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Earlier, we suggested that culture historians had adopted cultural evolution as an explanatory framework in the 1950s. That suggestion is founded on papers such as Alex Krieger's "New World Culture History: Anglo-America" in which a set of four adaptational stages are described (1953). The abhorance of anything evolutionary, however, prompted Willey and Phillips (1958: 67) to label Krieger's (1953) set of stages and a similar one proposed a few years earlier by Steward (1949) as "historical-developmental schemes." They proposed their own developmental scheme in what would become–with the later substitution of PaleoIndian for Lithic–a well-known set of terms for five stages: Lithic, Archaic, Formative, Classic, and Postclassic. Noting that their scheme had been derived "from an inspection of archaeological sequences throughout the hemisphere," they concluded by noting that the stages were "abstractions which *describe* culture change through time in native America. The stages are not formulations which *explain* culture change" (Willey & Phillips 1958: 200).

With respect to Willey and Phillips's scheme, Swanson (1959: 121) observed that "no theory has been developed. What is *assumed* is an evolutionary theory about the nature of culture, though Willey and Phillips are shy about admitting this. Moreover, no history has been written." The parallels between Willey and Phillips's scheme and White's notions of cultural evolution are clear from both the assumed theory and the lack of history in White's sense of the term inherent in the former's scheme. Another parallel is found in the fact that Willey and Phillips characterize their developmental scheme of stages as descriptive rather than explanatory; recall that Bidney (1946: 295) characterized White's scheme as lacking explanatory power. Bidney may have been worried that White was unclear on whether energy capture was the cause or the effect of change; casting human intent as the catalyst–as White did–renders this point irrelevant. Alternatively, Bidney's concern may have been that change in energy capture might be construed as the *proximate cause* of cultural change; if so, this left the *ultimate cause* unidentified. As Mayr (1961: 1053) noted, the latter "are causes that have a history and that have been incorporated into the system through many thousands of generations of natural selection." White, as far as we know, never once mentioned *selection* as the ultimate cause of anything.

White's students took up his banner. The classic *Evolution and Culture,* edited by Marshall Sahlins and Elman Service (1960) sought to clarify, solidify, and expand various issues. Of most concern here is their distinction between general and specific evolution. The former was defined as "passage from less to greater energy transformation, lower to higher levels of integration, and less to greater all-around adaptability. Specific evolution is the phylogenetic, ramifying, historic passage of culture along its many lines, the adaptive modification of particular cultures" (Sahlins & Service 1960: 38). Ironically, given his earlier proposal of five developmental stages, Willey (1961: 442) thought the former kind of evolution was of little use because "its processes are obscure." Specific evolution was, however, useful because it combined "history plus explanation of process–the story of how a given culture, or culture continuum, changed through time by the processes of its adaptations to natural and superorganic environments" (Willey 1961: 442). Continuing, Willey (1961: 443) noted that "until the processes of this general evolution are better understood, particularly as these pertain to the way in which the many streams of specific evolution feed into the main one, I cannot appreciate the difference between a general universal evolution of culture and a general universal culture history."

Contrary to the processual archaeology that was to emerge a few years later, Willey (1961: 442) indicated that a central disagreement he had with Sahlins and Service (1960) resided in the latter's "insistence that the technico-economic realm must always be given priority in explaining the processes of cultural change." This notion, of course, resided in White's formulations: Culture is humankind's extrasomatic means of adaptation; culture is a system of interrelated variables; and energy capture is central to system stability and change. Despite the protests of Willey and others (e.g., Haag 1959), the cultural-evolutionary notions of White provided the basis for the emergence of what came to be known as processual

archaeology. Definitions of key concepts, conceptions of how science was supposed to work, and those essentialist units known as bands, tribes, chiefdoms, and the like were carried over wholesale into that new paradigm (e.g., Binford 1969, 1972) because Darwinian evolution had already been discarded by the discipline.

CONCLUSION

On the eve of the birth of processual archaeology in the early 1960s, Haag perceptively observed that

> The usual reason for the rejection [by anthropologists and archaeologists] of the biological model [of evolution] is either, (a) that genetic mutations create "new materials" whereas human mutations (inventions) do not; or (b) that man can control and direct his evolution whereas animal mutations are random... Once the understanding of the nature of culture is gained, there is no longer any confusion between the genetic process in [biology] and the culture process in man (1959: 104).

Haag hit the nail squarely on the head as far as most anthropologists and archaeologists were concerned; there really *were* two kinds of evolution–one biological and one cultural–and different theory was needed for each. Haag's views of what cultural evolution entailed were no different than those of his contemporaries: a decidedly Spencerian and Whitean approach, complete with orthogenetic notions of progress, human intent, essentialist stages, and the like, and lacking any appeal to natural selection or a materialistic conception of variation. Regarding natural selection, Haag (1959: 103) made the observation that "[a]lthough an entirely different mechanism is involved in the adaptation of man to his environment, and thus a different mechanism controls his cultural evolution, adaptation is still the keyword." The biological mechanism was natural selection; the cultural mechanism was *integration*, defined by Haag (1959: 103) as "adjustment." Although Haag was vague, "adjustment" apparently meant addition to the pool of variant cultural traits; if a trait couldn't be adjusted to fit the existing pool, it wasn't integrated into (added to) the cultural system. *Why* it could not be integrated or adjusted was not addressed because a process comparable to natural selection was not included in Haag's rendition of cultural evolution, just as it wasn't in any of his contemporaries' views.

That Spencerian and Darwinian evolution are dissimilar is clear. Americanist archaeologists and cultural anthropologists of the early 20th century appear to have recognized at least some of the differences between the two, but they were insufficiently knowledgeable about Darwinism to figure out how to use it. Further, that theory was not in a form that could be readily adapted to the metaphor of culture change as a gradually, continuously flowing braided stream. Biologists were focusing on genetic change and essentialist categories termed species; typological thinking was rampant. Darwinism simply had little to offer anthropology and archaeology because (1) cultural evolution was reticulate, whereas biological evolution was branching; (2) cultural evolution did not involve the transmission of genes, whereas biological evolution did; and (3) people were not subject to the forces of natural selection and intentionally directed the evolution of their cultures, whereas biological evolution depended on the natural selection of random mutations. The option of Spencerian cultural evolution was attractive because it was not faced with these difficulties, and it concerned culture, not organisms.

The insights provided by Kroeber and several of his students have a somewhat unique and decidedly materialist feel to them, but the version of biological evolution then popular was not conducive to adoption for use in explaining the archaeological or cultural records. The alternative adopted–Spencerian evolution–has now been in active use for some five decades. It has granted us some significant insights to the past, and it has suggested some of the important variables with which we must contend. But it is in no sense a complete theory–some would argue it is not a theory at all but rather a model or philosophy (Dunnell 1980)–and thus its ability to provide testable explanations is limited. Its use of essentialist categories is one of its weaknesses; its orthogenetic view of evolutionary progress as inevitable is another. We suggest that it is appropriate to take up Kroeber's banner and try again to borrow from Darwinian evolution, the 1990s version, and see where it takes us. We believe the results will be significant.

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