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The Direct Historical Approach, Analogical Reasoning, and Theory in Americanist Archaeology

R. Lee Lyman[1,2] **and Michael J. O'Brien**[1]

Prior to the early 1950s, Americanist archaeologists, given their interest in chronology, routinely searched for direct historical connections between ethnographically documented cultures and archaeological cultures. In those instances where clear evolutionary connections existed between ethnographic and prehistoric cultures, the ethnic affinities of the latter could be assessed, chronologies of prehistoric cultures could be built, and ethnographic descendant cultures could be used as analogs of prehistoric ancestral cultures. The latter became known as specific historical analogy, and it stands in contrast to general comparative analogy, in which no detectable evolutionary connection exists between archaeological subjects and ethnographic sources. The theory underpinning the use of specific historical analogs is Darwinian evolutionism, or descent with modification; thus similarities between ethnographic sources and archaeological subjects are homologous. By midcentury, with the problem of chronology behind them, archaeologists began to address anthropological concerns. Darwinian evolutionism was replaced by the theory of orthogenesis as an explanation of culture change, and concomitantly specific historical analogy was replaced by general comparative analogy, in which similarities between ethnographic sources and archaeological subjects are the result of convergence. For over a century anthropologists and archaeologists have mixed elements of the two theories.

KEY WORDS: analogy; Darwinian evolution; direct historical approach; orthogenesis.

INTRODUCTION

An *analogy* is a form of reasoning that produces an inference about an unknown and invisible property of a subject phenomenon. The unknown property is

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inferred based on the fact that it is observable among source phenomena that are visibly similar in at least some respects to the subject. The *source* is the known side of the analogy and comprises the *analog*; the *subject* is the side of the analogy that includes the unknown property. Analogical reasoning has been commonplace in Americanist archaeology since at least the early nineteenth century (Baerreis, 1961; Charlton, 1981; Trigger, 1989). Five decades ago, Willey (1953a) identified two distinct kinds of archaeological analogy—what he termed *specific historical analogy* and *general comparative analogy*. Although analogical reasoning has been discussed extensively in the archaeological literature (e.g., Anderson, 1969; L. R. Binford, 1967, 1968a, 1972; S. R. Binford, 1968; Chang, 1967; Charlton, 1981; Clark, 1968; Crawford, 1982; Freeman, 1968; Gould and Watson, 1982; Green, 1973; Lange, 1980; Munson, 1969; Murray and Walker, 1988; Simms, 1992; Stahl, 1993; Wobst, 1978; Wylie, 1982, 1985, 1988, 1989), no one to our knowledge has explored the historical development of the two kinds, especially in terms of the theories that underpin them. That is our objective here.

We make two points. First, the two kinds of analogy identified by Willey rest on different theories of change, one being *Darwinian evolutionism* and the other *orthogenesis*. Both theories have been called "evolutionism," but they are decidedly different epistemologically and metaphysically (Alland, 1972, 1974; Carneiro, 1972, 1973; Dunnell, 1980; Freeman, 1974; Lyman and O'Brien, 1997; Mayr, 1982; Rindos, 1985). Second, a shift in the goals of Americanist archaeology in the 1950s away from culture history and toward cultural reconstruction (L. R. Binford, 1968b; Deetz, 1970) followed the abandonment of Darwinian evolutionism. These two developments resulted in the adoption of orthogenesis, which traces its direct anthropological roots to the late eighteenth and nineteenth centuries (Stocking, 1987). Accompanying the replacement in theory was a replacement of one kind of analogical reasoning, specific historical analogy, by its alternative, general comparative analogy. In the former, the ethnographic source analog is a direct evolutionary descendant of the ancestral archaeological subject; in the latter, the source analog is not a direct evolutionary descendant of the archaeological subject. Central to the history of these two kinds of analogy is the *direct historical approach*—a method that saw considerable use during the late nineteenth century and the first half of the twentieth century. Understanding the ways in which archaeologists used that method and the theories that underpin it reveals much about the history of analogical reasoning in Americanist archaeology.

Although our discussion suggests "direct historical analogy" would be more accurate historically and methodologically, we use the term "specific historical analogy" throughout because Willey (1953a) used this term. We begin with brief descriptions of Darwinian evolutionism and of orthogenesis. We then discuss each of the several distinct uses to which the direct historical approach was put. That discussion reveals why general comparative analogy came to be favored over specific historical analogy during the middle of the twentieth century. We then

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turn to a description of general comparative analogy and elaborate on why it rests on orthogenesis. How the change in goals of Americanist archaeology during the 1950s articulates with general comparative analogy and orthogenesis is then discussed. We conclude with a consideration of the claim made in the 1890s and again in the 1960s and 1970s that specific historical analogs are more valid than general comparative analogs.

THEORIES OF CHANGE

We use the term "descent with modification" to refer to Darwin's version of evolutionary theory (Darwin, 1859), which is based on the principle that heritable continuity exists between ancestor and descendant such that fidelity of replication of parental traits in offspring is ensured. Requisite mechanisms include transmission and natural selection, both of which involve sets of filters that influence fidelity and frequency of trait replication. Ontologically, sets or populations of things are more or less constantly in the process of becoming something else (changing) as a result of transmission and the action of the filters. Direction of change within a lineage, or line of heritable continuity, is unpredictable because it is historically contingent in three ways: (1) in terms of what is available for transmission, (2) in terms of what is transmitted and thus might be replicated, and (3) in terms of what actually is replicated (Ereshefsky, 1992; Gould, 1986). What is *available* for transmission depends on the random—with respect to what is or might be needed among descendants—generation of innovative variants; what *actually* is transmitted depends on the transmission mechanisms and their operation; what is *replicated* depends on the size of the transmitting population and the particular sorting filters in operation at the time of transmission. This is why the direction of change is largely unpredictable.

The result of Darwinian evolution has been characterized as "sorting" of available variants from one generation to the next (Vrba and Gould, 1986) such that replication of variants over time is differential (Leonard and Jones, 1987; Teltser, 1995). When the sorting, or filtering, mechanism is natural selection, descent with modification is affected by differential replication of available variants from one generation to the next. Descent with modification can also be affected simply by the vagaries of transmission such that replication is differential from one generation to the next.

The second theory, "orthogenesis," is a multifaceted and complex theory of evolution. The origin of the term itself is controversial; it is variously said to have been coined by biologist G. H. T. Eimer in 1898 (Bowler, 1979) and by biologist W. Haacke in 1893 (Grehan and Ainsworth, 1985). Historically, various proponents had different views of orthogenesis, and our discussion is necessarily simplified. Orthogenesis rests on the notion that change within a lineage involves "nothing accidental or creative, for evolution 'proceeds in accordance with laws,' through

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a predetermined sequence of stages or phases" (Dobzhansky, 1957, p. 382). Heritability and transmission comprise part of this theory, but mechanisms resulting in sorting are unnecessary because only variations that fulfill an immediate, long-term, or eventual need are produced, transmitted, and replicated. Therefore, natural selection and other "external" forces play no role in causing a lineage to evolve along a particular line (Cronquist, 1951). In orthogenesis, natural selection plays "a different role from that ascribed by Darwin" (Grehan and Ainsworth, 1985, p. 178). That role is termed "channelling," which results from mechanical constraints on the evolving entity's parts under descent with modification (e.g., Gould, 1982).

As documented by Bowler (1979), orthogenesis was felt by some biologists to account for the fact that organisms exhibited traits that appeared to have no obvious function. Some orthogenecists held that such traits could not be accounted for by natural selection because only those traits that fulfilled an obvious need were products of selection. Therefore, some other mechanism was necessary to account for what in the late nineteenth and early twentieth centuries seemed to be nonadaptive trends evidenced by the fossil record. Many who subscribed to this version of orthogenesis held that there was a mysterious internal drive resulting in the appearance of new variants; others held that the force was external (environmental); still others held that new variants were produced by a combination of internal and external forces. Whatever the source of the cause, the result was the same—directed variation precluding the operation of natural selection. Thus, orthogenesis suggests that the appearance of innovations follows "well-defined pathways of change. Such evolutionary trends [are] ascribed to [a] direction-giving force.... The evolution of phyletic lineages [occurs] along a predetermined linear pathway" (Mayr, 1991, pp. 61, 183). The theory of orthogenic evolution has been referred to as "the doctrine of 'straight-line' evolution" (Romer, 1949, p. 107).

Under either of the two theories of evolution, each lineage within a category of phenomena, despite its independence from every other lineage, can evolve parallel to, or converge with, all others. The modern concepts of parallel evolution and convergent evolution can be defined as follows: Parallelism results from the development of similar adaptive designs among closely related phenomena; convergence results from the development of similar adaptive designs among remotely related phenomena (Eldredge, 1989, p. 51). Evolutionists subscribing to descent with modification argue that the "very widespread occurrence of parallelism and convergence is the strongest sort of evidence for the efficacy of selection and for its adaptive orientation of evolution.... Yet... these processes do not produce *identity* even in the limited parts of structures most strongly affected by parallelism and convergence" (Simpson, 1953, pp. 170–171). The last observation is critical because it underscores the importance of systematics and classification to analytical efforts aimed at deciphering evolutionary history (see O'Brien and Lyman (2000) for elaboration).

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Orthogenic evolution[3] can entail convergence or parallelism because, for example, it begins with homogeneity and ends with heterogeneity (see discussion of Spencer's work by Carneiro (1972, 1973) and Freeman (1974)), or what could today be characterized as, say, every biological lineage beginning with generalists and ending with specialists (Gould, 1970). Orthogenecists may quibble over the precise nature of the stages that organisms (or cultures) go through, how the stages are defined, and how particular empirical phenomena are classified, but they typically agree on the result—that is, that a descendant is somehow more complex (heterogeneous) or more fully developed and less primitive than its ancestor (Carneiro, 1972; Jepsen, 1949). Orthogenesis in this sense is progressive. The mechanism prompting change may be some "unknown and unknowable force" (Spencer, as quoted by Freeman, 1974, p. 215), but the result is the same. This is not to say that orthogenic evolution is inevitably teleological. Many evolutionists in the nineteenth century argued that it was not, preferring instead to call on elements of Lamarckism—believing that the environment in which a population of organisms was located somehow stimulated the internal drive and directed the appearance of new variants (Bowler, 1979).

Boas (1920, p. 312) referred to evolutionary theories of nineteenth-century anthropologists as comprising a notion of "orthogenetic development." Radin (1929, p. 12) characterized those theories as involving "straight-line evolution," and Lowie (1937, pp. 27, 28) characterized them as involving evolution that was "predestined" because they followed "a fixed law of development." Importantly, Radin (1929, p. 12) observed that the nineteenth-century subscription to an orthogenic theory of cultural evolution "deflected attention from the examination . . . of the mechanism of cultural transmission." In short, orthogenesis underpinned the work of Spencer, Tylor, Morgan, and numerous others in the nineteenth century, although Carneiro (1973, p. 80, fn 5) argues that Spencer found no value in Eimer's concept of orthogenesis. Alland (1974, p. 273) points out, however, that Spencer's views changed over time and, more importantly, that one can find contradictory statements in Spencer's many writings, "which may be used to support diametrically opposed positions." Further, given the different views of what orthogenesis actually comprises, we are not surprised that some historians find elements of orthogenesis in Spencer's writings whereas other authors do not.

Nineteenth-century anthropologists conceived of the development of cultural lineages as involving the more or less inevitable passage of cultures through a set series of stages (Simpson, 1961). It still underpins much Americanist archaeology (Dunnell, 1980, 1989; Lyman and O'Brien, 1997, 1998; see especially Spencer (1997) and selected references therein), paralleling the trend in modern anthropology (Rambo, 1991). During the period 1875–1960, in both Americanist

[3] We prefer the adjective "orthogenic" to "orthogenetic" because of the implication carried by the latter that all change is genetically based. This is not necessarily true with culture change.

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archaeology and anthropology elements of both descent with modification and orthogenesis were mixed together to produce evolutionary explanations of cultural phenomena. The issue of whether cultural evolution is orthogenic or comprises descent with modification sometimes took the form of a debate, such as that between White (1945, 1947, 1959a) and Kroeber (1946, 1960) on the differences between history and evolution (Lyman and O'Brien, 1997), but other times it was more implicit. Because the two theories were indistinct to most Americanist anthropologists and archaeologists working in the early and mid-twentieth century, elements of both underpinned various analytical procedures used at the time. Even in 1960 the two theories were not distinguished by anthropologists and archaeologists. Orthogenesis is mentioned and distinguished from descent with modification only by biologists who contributed chapters to Tax (1960); anthropologists and archaeologists who contributed chapters conflate the two theories and describe a form of cultural evolution that is a hybrid of the two theories. This mixing has deep roots, one clear example of which is found in the direct historical approach.

THE DIRECT HISTORICAL APPROACH

The archaeological record of the Americas was viewed by many nineteenth-century archaeologists as a short extension into the past of the ethnographically documented record (Lyman *et al.*, 1997; Trigger, 1989; Willey and Sabloff, 1993). Cultures and human behaviors of the archaeological past were seen as basically identical to those observed and described ethnographically; thus they could be studied by those with minimal training in archaeology (see discussion and references in Meltzer, 1983). One result of this view was the regular use of what came to be formally known in the 1930s as the direct historical approach. Wedel (1938), in the second of two works on the Pawnee, apparently was the first person to use the term, although the method was decades old at that point.

Formal recognition of the method is found in Dixon's presidential address to the American Anthropological Association, in which he stated, without elaboration, that "one would logically proceed to investigate a number of [sites of known ethnic affiliation], and work back from these," because it "is only through the known that we can comprehend the unknown, only from a study of the present that we can understand the past" (Dixon, 1913, p. 565). Strong (1933, p. 275) stated that Dixon "emphatically set forth" the procedure of the direct historical approach. Of Dixon's statements, de Laguna (1960, p. 220) remarked that "[a]lthough not the first, this is one of the earliest and clearest statements of what we now call the 'direct historical approach' in archaeology." We disagree with these evaluations because the exact protocol of the approach is not described by Dixon.

Two decades later, Strong (1935, p. 55) was no more explicit than Dixon (1913) when he stated that "once the archeological criteria of [a historically

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documented] culture had been determined, it [is] then possible to begin the advance from the known and historic into the unknown and prehistoric." Strong (1953, p. 393) later described the approach as "proceed[ing] from the known (documentary–ethnological) to the unknown (prehistoric–archaeological)," adding that such a procedure "is a clear application of [E. B.] Tylor's advice." Tylor (1881, p. 10), however, had simply remarked that "it is a good old rule to work from the known to the unknown." Perhaps Strong either was not intimately familiar with what Tylor said or was selective in what he quoted, because Tylor was *not* referring to specific historical analogy when he discussed his "rule." Rather, he was following his intellectual predecessors (Hodgen, 1964; Stocking, 1987) and referring to the use of ethnographic source cultures that were believed to be at the same stage of development as the archaeological subject cultures. Thus if one wanted to know something about an archaeological people who made stone tools but not pottery, the place to look for an analog was among modern or ethnographically known peoples who made stone tools but not pottery. Tylor, then, was advocating the use of general comparative analogy, not specific historical analogy.

Fenton (1940, pp. 243, 165), writing a few years after Strong and Wedel, noted that the direct historical approach involved proceeding "from the known groups to the unknown cultures and peoples that precede them" and indicated that "archeological studies should proceed from the [ethnically] known historic sites back through the protohistoric sites to the prehistoric period." Heizer (1941, p. 101) used the term in the title of a work on California but not in his text, noting only that the method comprised "the correlated historical–ethnographical–archaeological approach." Similarly, Steward (1960) used the term in a paper title, but it is unclear from the article what the direct historical approach involves. de Laguna (1960, p. 218) suggested that students use "the 'direct historical approach' [when reading a collection of turn-of-the-century articles on Americanist archaeology] by working backwards from some such easily reached vantage point as (Martin *et al.*, 1947)." She was referring to tracing an intellectual tradition or lineage, but she did not elaborate on how students were to accomplish this.

In his synopsis of anthropological methods of chronometry, Sapir (1916, p. 5) noted that there were two kinds of chronometric evidence, what he termed "direct and inferential." The former "meant such evidence as directly suggests temporal relations" and the latter comprised "such evidence as is inferred from data that do not in themselves present the form of a time sequence" (Sapir, 1916, p. 5). There were, in Sapir's view, three kinds of direct evidence: that which was "yielded by historical documents" (p. 5), statements of tribal history by tribal members, and "the stratified monuments studied by archaeology" (p. 9). Given that throughout his discussion Sapir (1916) repeatedly referred to these as "direct historical evidence," we suspect his discussion was the inspirational source for the term "direct historical approach" coined by Wedel (1938).

Curiously, the direct historical approach rarely appears, by its formal name or otherwise, in histories of Americanist anthropology (Lowie, 1937; Panchanan,

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1933). No one ever pointed out that the direct historical approach was actually used for three distinct, although interrelated, purposes. In Americanist archaeology these were: (1) to identify the ethnic affiliation of an archaeological manifestation; (2) to build relative chronologies of archaeological materials; and (3) to gain insight into the human behaviors that were thought to have produced particular portions of the archaeological record. The last use was derived directly from the first two uses and was, simply, a kind of analogical reasoning built around both time and the notion of heritable continuity between ancestor and descendant. We think the reason no one ever pointed out that there were three distinct uses of the direct historical approach was because by about 1915 they *weren't* distinct. Rather, the constituent parts were so intertwined that they were inseparable. In addition, by that time the basic method was viewed as being so commonsensical that no one felt the need to explore its underpinnings in any great detail. This was not damning, but in considering the method to be commonsensical the underpinning theories were viewed in similar light, and that resulted both in mixing elements of the two theories and in making ill-conceived statements about kinds of ethnographic analogy.

The Direct Historical Approach as an Ethnic Identifier

Writing after the heyday of the direct historical approach, Willey (1953b, p. 372) produced one of the clearest statements concerning its use as a means of assigning ethnic identity to archaeological phenomena:

> Through a series of successive periods prehistoric cultures were linked to proto-historic, historic, and modern descendants. This type of study, sometimes called the "direct historical approach," has a theoretical basis in cultural continuity. Starting with known, documented habitation sites, certain cultural assemblages were identified and associated with particular tribal groups. Earlier archeological assemblages were then sought which were not too sharply divergent from the known historic ones, and the procedure was followed backward in time....
>
> The establishment of prehistoric-to-historic continuity is of utmost importance as a springboard for further archeological interpretation, and, along with general chronological and distributional studies, it is one of the primary historical problems for the American archeologist.

Willey was correct: The theoretical basis of the direct historical approach lies in cultural continuity. But this begs the question of *why* there is continuity in the first place. The answer is, because it was produced through the process of cultural transmission—a process explicitly named and discussed by Sapir (1916), who argued that cultural transmission ensured a high degree of fidelity in trait replication between ancestor and descendant. Willey's theory of "cultural continuity" rests squarely on the notion of cultural transmission, which finds empirical expression in the overlapping of cultural traits across the trait lists of the historically and ethnographically documented cultures and the archaeological manifestation(s) in question. Willey, like many other culture historians (e.g., Ford, 1938a; Phillips *et al.*, 1951), knew this, but as evidenced by the statements of Dixon, Strong,

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Fenton, and Heizer quoted above, they did not feel the need to spell it out in explicit terms (Lyman, 2001; Lyman and O'Brien, 2000).

This is more or less the line of reasoning used by Cyrus Thomas (1894) in his demonstration that the numerous earthworks scattered across the eastern and midwestern portions of North America were produced by the direct genetic and cultural ancestors of historically documented ethnic groups. In many respects Thomas simply was verifying what many others, including Bureau of American Ethnology director John Wesley Powell, suspected. It was the vast amount of empirical evidence marshaled by Thomas, together with his analytical methods, that mark his work as the formal genesis of the direct historical approach as an ethnic identifier—regardless of the scale at which distinct ethnicity was conceived (Dunnell, 1991)—in Americanist archaeology. Thomas referred continually to historical records of the sixteenth through eighteenth centuries, where it was documented that the post-Columbian Indians were sufficiently "culturally advanced" (being sedentary agriculturists) to have built the mounds and in some cases had actually been observed building mounds. Documenting typological similarity of artifacts from the historical and prehistoric periods established evolutionary linkages and demonstrated the utility of the direct historical approach as an ethnic identifier (O'Brien and Lyman, 1999a).

Thomas claimed the evidence was there if anyone examined it critically, especially the fact that earthworks with European items in them were similar to those from presumed earlier periods. There was no logical reason to suspect that the mound builders were of Mexican origin or that Indian groups had pushed the mound builders out of the eastern United States. In other words, the archaeological record demonstrated to Thomas's satisfaction that a high degree of cultural continuity had existed for an untold age and that such threads of continuity showed no major disruptions. Change, of course, had occurred—this much was indicated in the myriad forms of earthworks recorded and the different kinds of artifacts found within them—but this type of change was an orderly, continuous progression as opposed to a punctuated, disruptive progression of cultural epochs such as was evident in the European Paleolithic–Neolithic sequence.

After Thomas's work, the ultimate goal of anthropology, especially as practiced by those connected with or trained through the Bureau of American Ethnology, was to write a full description of each ethnic group's culture before it disappeared under the onslaught of Euroamerican expansion. Such "salvage ethnography" was an attempt to determine the pristine, pre-Columbian nature of Native American cultures (Dunnell, 1991). As the National Research Council's Committee on State Archaeological Surveys noted, the "reconstruction of the original culture of tribes at the time of their first meeting with the settlers is a most important problem" (Wissler *et al.*, 1923, p. 2). Archaeological data, because they were incomplete, played a secondary role to ethnographic data, being used primarily to help fill in gaps which, given the then-prevalent notion that the American archaeological record had a shallow time depth, were not viewed as being

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particularly large. To ensure that the correct archaeological data were used to fill the gaps, one had to identify the ethnic affiliations of those data. Precisely how such an identification was made was seldom spelled out in great detail.

The literature of the late nineteenth and early twentieth centuries suggests the procedure could take one of two forms. Both forms find their roots in Sapir's suggestion that if a prehistoric artifact was recovered from the same geographic area as a formally identical ethnographic artifact, then it could be concluded that the former was produced by the same ethnic group as the latter (Sapir, 1916, p. 6). These two criteria—identical geographic origins, and typological identity or similarity—were separated and used independently in some applications. Mott (1938), for example, compared the geographic distributions of archaeologically defined culture units with the historically documented distributions of various ethnic groups. She concluded that archaeological units had been created by the ethnic groups with the same geographic distributions as the archaeological units. Griffin (1937) used a similar procedure.

More commonly, however, the type of procedure followed involved assessing the similarity of sets of artifacts rendered as sets of culture traits sometimes referred to as "complexes." This procedure was spelled out in a pamphlet published by the Committee on State Archaeological Surveys of the National Research Council: "In all the states there are known sites of what were Indian villages during the period of colonization and in many of the States there still remain remnants of Indian tribes once living and flourishing there. It is thus possible to connect the immediate prehistoric with the historic.... When articles identical with those found on the historic sites occur on those of prehistoric origin, careful comparison with other sites in the locality will leave little doubt as to the identity of the people inhabiting the locality" (Wissler *et al.*, 1923, pp. 2–3). This procedure entailed three basic steps. First, compile a list of cultural traits for the extant ethnic group under consideration. Second, convert that portion of the archaeological record under scrutiny into a list of cultural traits. Third, compare the two lists, and when an ethnographic list corresponded (to some unspecified but relatively high degree) with an archaeological list, conclude that a match had been made. All that was demanded by that conclusion was heritable continuity between the two groups; the underlying theory could be either descent with modification or orthogenesis, as both entail cultural transmission. Thus no discussion of theoretical nuances was necessary.

Wedel's work on the Pawnee is an excellent example of the procedure involving the comparison of trait lists (Wedel, 1938). Wedel compared three cultures in terms of traits they exhibited. Two cultures—the Lower Loup Focus and the Oneota Aspect—were archaeological, and one—Historic Pawnee—was ethnographic. The list contained 120 traits, of which the Historic Pawnee had 80, the Lower Loup Focus 82, and the Oneota Aspect 74. Wedel noted that all three cultural units shared 39 traits, the Historic Pawnee and Lower Loup Focus shared 55 traits, the Historic Pawnee and Oneota Aspect shared 42 traits, and the Lower Loup Focus and Oneota Aspect shared 48 traits. Based on these observations, Wedel (1938, p. 11) stated

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that the "conclusion seems inescapable that the Lower Loup Focus stands in very much closer and more direct relationship *genetically* to the later historic Pawnee than to the contemporaneous Oneota peoples" (emphasis added). Such "genetic" connections were typical inferences (e.g., Boas, 1904; Ford, 1938a; Griffin, 1943; Lowie, 1912; Wissler, 1916a), although the term "genetic" was being used in a metaphorical sense to refer to relationships between cultural traits rather than to those between biological phenomena.

The Direct Historical Approach as a Chronometer

With minor methodological and theoretical extension, the same procedure used to demonstrate ancestor–descendant relationships was used to measure the passage of time. The methodological extension comprised building temporal sequences of artifacts by beginning with a list of cultural traits rendered as artifact types of various scales possessed by a historically documented culture and then working back ever deeper into the past by determining which traits (artifact types) were held by archaeologically represented cultures (Fig. 1). The analyst could go beyond mere ethnic identification to *temporal sequences* by including the theoretical notion of sorting—that is, that traits would come and go through the sequence, but they would each appear only once or temporally continuously across multiple

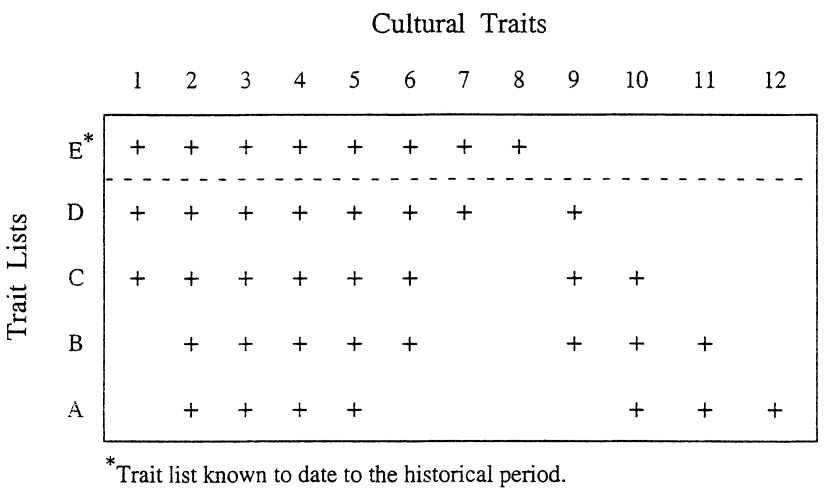


Fig. 1. A model of the direct historical approach used as a chronometer. Each column designated by a number represents a cultural trait; each row comprises a trait list. Plus marks indicate a trait is present. Row E, comprising traits known to date to the historical period, anchors one end of the sequence; thus time passes from the bottom to the top. Each lower row designates an archaeological manifestation that is earlier in time than row E. The trait lists have been ordered using the notion of sorting (each trait occurs during only one part of the sequence). The overlapping traits across multiple lists signifies transmission and heritable continuity between contiguous lists.

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trait lists during the sequence. As Wissler (1917, p. 278) put it, "by working backward from the historic period or . . . from a fixed date, it is possible by these methods to separate the older elements of culture from those of relatively recent origin. . . . In the main, [the method] first analyzes the culture trait-complexes and then by comparative reasoning arranges them in time sequences." Perhaps not surprisingly, some archaeologists referred to this use of the direct historical approach as "historic archeology" (e.g., Strong, 1935, p. 55; 1940, p. 353).

Assumptions underpinning use of the direct historical approach as a chronometer are that (1) the traits shared by prehistoric cultures with their historically recent descendant culture(s) become progressively fewer in number as the traits in successively older cultures in a lineage are polled; (2) the overlapping of traits across several temporally distinct yet contiguous cultures in a temporal sequence links them together in time; and (3) the linkage represented by overlapping traits denotes a line of heritable continuity comprising an evolutionary lineage (Lyman and O'Brien, 2000). Archaeologists and anthropologists referred to the last as "historical continuity" (Ascher, 1961) or "historical relatedness" (Parsons, 1940), but they seldom acknowledged explicitly that cultural transmission had resulted in heritable continuity between an ancestral and a descendant culture (Lyman, 2001). This critical process was further obscured because the direct historical approach *as a chronometer* was not used as a means of determining cultural lineages or of writing the evolutionary history of an ethnographically documented culture. The theoretical reason *why* the direct historical approach worked as a chronometer—it monitored heritable continuity (Lyman and O'Brien, 2000; O'Brien and Lyman, 1999b, 2000)—was unstated.

That sorting and overlapping of traits comprised the epistemological basis of the use of the direct historical approach as a chronometer escaped comment; Sapir (1916) was virtually the only one to note that cultural transmission comprised the theoretical basis. This may have been of no great moment because overlapping could be affected by either orthogenesis or descent with modification, and although only the latter explicitly implicated sorting, no major mental gymnastics were required to conceive of sorting as a result of orthogenesis (Sapir, 1916). The general failure to explicitly consider the theoretical reason *why* the approach worked as a chronometer, however, resulted in archaeologists viewing their chronologies as "largely a matter of inference from the data" (Wissler, 1916b, p. 487). Like his contemporaries (e.g., Gamio, 1917; Kidder and Kidder, 1917; Kroeber, 1916; Nelson, 1916; Sapir, 1916; Spier, 1917), Wissler believed that stratigraphic excavation provided a more direct approach to cultural chronology, but this belief was often based on a naivete regarding the principle of superposition—that vertical position implied the relative age of artifacts as opposed to the relative age of their deposition (Lyman and O'Brien, 1999; Lyman *et al.*, 1997, 1998; O'Brien and Lyman, 1999b). Kidder (1924, pp. 45–46) did not describe any technique similar to the direct historical approach when discussing chronometers available in the

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1920s, instead expressing preference for stratigraphic observation. Similarly, although Ford (1936a, p. 103) wrote that "identifying the sites of villages mentioned in the chronicles of the first explorers [allows one to connect] these sites with a definite time period, and gives a starting point for the projection of the chronology back into the prehistoric," he went on to note that "vertical stratigraphy [constituted] the best basis for the relating of time changes." Finally, Lowie (1944, p. 323) noted that in "the Plains Wm. D. Strong has demonstrated the value of combining a stratigraphic technique with a historical approach, applying the sound principle of working from the known backward to the unknown."

Perhaps lineages did not need to be constructed because, at least initially, it was believed that they would show minimal cultural development and change, given the perceived lack of time depth in the American archaeological record. As Kroeber (1909, pp. 3–5) remarked early on, the "civilization revealed by [archaeology] is in its essentials the same as that found in the same region by the more recent explorer and settler. . . . Archaeology at no point gives any evidence of significant changes in culture [; thus] differences between the past and present are only differences in detail." Similarly, Boas (1902, p. 1) had stated that it "seems probable that the remains found in most of the archaeological sites of America were left by a people similar in culture to the present Indians." Thus, the perceived shallow time depth of the American archaeological record lessened the chances of convergence or parallelism resulting in shared traits; it appeared much more likely that such overlapping traits were the result of shared ancestry (Steward, 1929; Swanton and Dixon, 1914). Even after the Folsom discovery in 1927, however, Swanton (1932, p. 74) could state that "it is important beyond all else for you archaeologists to tie your discoveries into known tribes, after having done which you may trace them back into the mysterious past as far as you will, and your work will have more interest and more meaning for you and for us all."

Of greatest yet largely unremarked theoretical importance was the fact that examination of temporal change in the list of cultural traits assumed no inevitable direction to the change. Wissler (1916b, p. 487) noted that "we must have a point of departure in the historic period, from which to work backward," but he did not say why. Others were explicit about why the chronological anchor of the present was mandatory. Kidder (1917, p. 369), for example, cautioned that the "only safe method for working out developments in decorative art is to build up one's sequences from chronologically sequent material, and so let one's theories form themselves from the sequences." Kroeber (1916) used the present as the historical anchor for his seminal frequency seriation so that he could determine in which direction change had occurred. Ford (1938b, p. 263) was perhaps most explicit about the necessity of using the present as a chronological anchor: "Without this tie-up it would be as logical for one end of the chronology to be recent as for the other." In other words, cultural change was not orthogenic *because it had no inevitable direction.*

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Advocation of stratigraphy as the best way to build a chronology is further evidence that few archaeologists working in the early twentieth century believed cultural evolutionary change had an inevitable direction. *Time* had an inevitable direction, but cultural evolution did not. That there was no assumption of straight-line evolution involved in the use of the direct historical approach as a chronometer was made abundantly clear when it was used in the Great Plains to ascertain the history of local cultural lineages (Strong, 1935, 1936, 1940; Wedel, 1936, 1940). The sequence of cultural change evident there proceeded from sedentary horticulturalists to nomadic equestrian hunters, rather different from and opposite to the sequence predicted on the basis of nineteenth-century orthogenic cultural evolution (e.g., Morgan, 1877; Tylor, 1881). Indeed, Kidder (1931, p. 4) noted that one must not assume "the crudest and most widely diffused remains [of ceramics were] the oldest" but instead test such hypotheses with independent chronological evidence such as stratigraphy—a point that Holmes (1892, 1897) had made earlier with respect to stone artifacts that were assumed to constitute evidence of an American Paleolithic (Meltzer, 1983, 1985). Had orthogenesis underpinned the chronometric properties of the direct historical approach, Kidder's caution would have been unnecessary because orthogenesis assumes that all stages and their order of expression within any given lineage are inevitable and sequential.

On the one hand, the fact that the theoretical underpinnings of the direct historical approach when used as an ethnic identifier were not explicitly recognized was not a fatal oversight by itself. Cultural transmission was all that was required, and everyone knew this. Hesitancy to accept the chronometric results of the direct historical approach, on the other hand, resided squarely in the failure to make explicit the theory underpinning this use of the approach. Hesitancy of another form occurred for exactly the same reason when specific historical analogs were contrasted with general comparative analogs.

The Direct Historical Approach as Analogy

Once it had been established that the direct historical approach could be used both to place ethnographic labels on archaeological cultures and to measure the passage of time, archaeologists took the logical next step and used the approach to create analogies. We say this was the logical next step, but perhaps it would be more precise to view the analogical use as a natural outgrowth of the first two steps, given that ethnicity and time are the two main ingredients of specific historical analogy. After placing a source and subject in proper chronological order and establishing a line of descent between them, all that was left to do was mine the ethnographic-source record for information and then use it to reconstitute the archaeological-subject phenomena into a dynamic cultural system. Fewkes (1896, p. 158) was exceptionally clear when he noted that a demonstration of evolutionary connection between an ethnographic source culture and an archaeological subject culture not only "implied that the former culture had been transmitted, [but that

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this] renders it safe to apply the principle of interpreting archeology by ethnology." Why it was "safe" apparently resided in the fact of evolutionary continuity, a notion that would reappear in the 1960s.

To archaeologists working in the first half of the twentieth century, the ethnographic record of the United States provided a rich source of information on dynamic, operating cultures. Information gleaned from that record with respect to material culture allowed artifact function to be determined—function not only in terms of how a tool was used but in terms of how it was made, recycled, and so on—and it was only a short step from how a tool was used to the behaviors of the people who manufactured and used the tool. As Mott (1938, p. 227–228) noted, "if an identification is established between a historic tribe and an archaeological manifestation, then the whole inter-related pattern of pre-recorded cultures receives a new orientation and is linked to 'history.' . . . Once such a link is made, a mass of knowledge concerning the economy, the techniques, the ceremonial life, and the location of the people so identified merges with the written facts concerning them." Thus, "rounded conical stones . . . tipped over on their sides" on the floor of an eleventh-century kiva provided "pretty good evidence for the existence of a whole religious system" (Parsons, 1940, p. 215). It is not surprising that this line of reasoning came to be known as the "direct ethnological approach" (Kehoe, 1958).

As neat a package as specific historical analogy produces, there were at least three problems with it. Eggan (1952, p. 38) identified the first problem, if only by implication: For "certain recent archaeological cultures the direct historical method, once valid connections [of heritable continuity] are established, offers an avenue by which late [archaeological] manifestations may be enlarged through inferences from ethnological horizons." The key word in Eggan's remarks is *late*, the implication being that the direct historical approach when used as analogy works progressively less well as the subject archaeological manifestation increases in age. This worrisome aspect of ethnographic analogy was noted by Slotkin (1952), and it continued to plague the use of ethnographic analogy into the 1960s, when it was pointed out that "although this approach may be an especially fruitful one when applied to recently extinct cultural systems, it is likely to yield misleading results when applied to the study of cultural materials produced by more ancient societies, especially societies more than 40,000 years extinct" (Freeman, 1968, p. 263). Here Freeman (1968, p. 263) was speaking about specific historical analogy, as he noted that one should derive, "from the study of a sample of modern societies, elements of sociocultural structure which are *homologous* with those of the prehistoric period" (emphasis added).

There was a related issue, and that was the fact that groups of people do not remain in one place on the landscape. They move around, and the greater the temporal distance between source and subject, the greater the possibility that one or the other moved. Willey (1953a, p. 373) commonsensically pointed out that "in general, the most successful continuities . . . have been determined for those regions

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where there has been relatively little ethnic shifting in aboriginal or protohistoric times and where there still remain native populations with predominantly native cultures." He also pointed out that "a general, but not absolute, assumption which Americanists have followed in these reconstructions is that gradual and unbroken continuity of culture also implies continuity of population and that a sudden change or break in continuity is a reasonable indicator of population change" (Willey, 1953a, p. 373). This line of reasoning might have held for late-prehistoric cultures found in the same geographic area as the historically documented culture (Ford, 1936b; Steward, 1929; Stirling, 1932), but what about older archaeological cultures? This question dogged culture historians well into the 1950s (Lyman *et al.*, 1997): How can one escape the problem of the increasing temporal distance between the source and the subject? The answer was, one *cannot* escape the problems imposed by the remote past on keeping an ancestor–descendant line intact; rather, one has to create a type of analogy independent of that relationship.

The second problem with specific historical analogy had to do with the kinds of traits that were being used. The use of specific historical analogs by culture historians grew from an interest in chronological sequences of cultural manifestations, in much the same way that post-Darwinian anatomists turned to homologous anatomical structures as indicators of evolutionary descent with modification (Lyman (2001) and references therein). Users of the direct historical approach as a chronometer for studying the history of an ethnographically defined culture tried to focus specifically on homologous traits—those attributable to shared ancestry—because the underpinning, yet covert, theory was descent with modification. They knew, if only in rudimentary fashion, that nonhomologous traits could not be used to trace ancestry, and they were ever mindful of the difficulties in distinguishing between homologous traits and nonhomologous traits (e.g., Boas, 1924; Kroeber, 1931a; Lowie, 1912; Sapir, 1916; Steward, 1929).

Identifying homologous traits and separating them from instances of convergence is as significant an analytical hurdle in biology and paleobiology as it is in archaeology (Lyman, 2001). 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 nonhomologous similarity. There obviously are differences between the two kinds of traits, but *how* can they be distinguished? Sapir (1916, p. 37) suggested culture traits that display merely "superficial" similarity are probably not evolutionarily related and are likely the result of convergence, whereas those that display "fundamental" similarity are "historically related." Kroeber (1931a, p. 151) was more explicit, although he used some of Sapir's words and 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, precisely how biologists distinguish between homologs and analogs (Lyman, 2001). But despite Kroeber's insightful comment, few culture historians

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went the next step and explored the matter in theoretical or methodological fashion (Lyman, 2001).

The third problem with the use of specific historical analogs was that evolutionary trends can be independent of one another as well as of no predictable direction. By this we mean that although a direct historical link might exist between source and subject, it is unreasonable to think that the source side is an image identical to that of the subject side—that there has been no cultural change over the hundreds or thousands of years that passed in the interim. It is also unreasonable to assume that all traits of the subject culture evolved at equal and consistent rates. In paleobiology this is referred to as mosaic evolution, which holds that different characters (traits) within a lineage can evolve independently of one another and that each lineage can evolve independently of every other lineage (Eldredge, 1989). This important concept can be extended to culture, where we can talk about the independence of lineages of automobiles from lineages of computers or the independence of lineages of lithic technologies from lineages of ceramic technologies. The QWERTY keyboard first developed in the 1870s has remained with us despite remarkable changes in how our finger strokes are mechanically and (increasingly) electronically transferred to paper.

Fewkes (1896, p. 164) recognized the problem of mosaic evolution early on when he noted that the "vein of similarity of old and new can be used in an interpretation of ancient [myth and ritual], but we overstep natural limitations if by so doing we ascribe to prehistoric culture every conception which we find current among modern survivors." Parsons (1940) and others were more optimistic, but many recognized Fewkes's central point (e.g., Kroeber, 1919, 1923; Sapir, 1916). No better example of the problems involved with mosaic evolution can be found than in the wrangling between archaeologists and ethnographers at the National Research Council-sponsored conference on midwestern prehistory held in Indianapolis in 1935 (National Research Council, 1937). One of the basic issues at stake was the same one that had held center stage at a previous NRC-sponsored meeting, held in Birmingham, Alabama, in 1932: how to make connections between historically known groups and prehistoric cultures and then use the information to create analogies. The proceedings from both meetings make it clear that archaeologists were desperate to make such connections and thus were willing to use any shred of evidence at their disposal, including ethnohistoric data and linguistics. They played fast and loose with descriptors such as Algonquin, Algonkian, and Algonquian, using the terms interchangeably regardless of whether they were talking about kinship systems, material items, social organization, or language. In their minds, the terms all meant the same thing: ethnic identity.

One voice of caution at Indianapolis was that of John R. Swanton, a Harvard-trained archaeologist-turned-ethnologist who spent his career with the Bureau of American Ethnology. Swanton's early work was on North American Indian languages, and although he continued to produce linguistical texts throughout his

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career (e.g., Dorsey and Swanton, 1912; Gatschet and Swanton, 1932; Swanton, 1919, 1940), he became better known for his ethnohistorical work. Swanton was an archaeologist's dream—someone who both spoke the language and was sympathetic to the goals of prehistory. More importantly, Swanton was someone who could place individual Indian groups in particular places at particular times. At the Indianapolis conference Swanton issued mild warnings against subsuming various cultural components within linguistic units without taking into consideration that language was only one kind of identifier—and a broad one at that—and that it had little or nothing to do with things such as customs, social organization, and material culture.

Swanton was not the only person at the Indianapolis conference to see the problems inherent in trying to stretch the use of direct historical analogy. Even a casual reading of the conference proceedings indicates that most participants were aware of the shortcomings, but it also is apparent that in their desire to convert the archaeological record into an ethnographic record they were willing to accept that mistakes might be made. We see the conference as more or less a last-ditch effort to revive the direct historical approach as a useful tool for constructing analogies. As the meeting wore on, however, it became painfully obvious that there were just too many problems with specific historical analogy: too much time had elapsed between source and subject; it was too difficult to identify ethnographic groups except in terms of general linguistic affinity; and not all cultural traits evolved at the same rate.

Although the direct historical approach would be used for several more years, particularly on the Plains (e.g., Mott, 1938; Wedel, 1938), it began to have competition from methods that did not depend on documenting ancestor–descendant relationships. In fact, despite the fact that considerable time at the Indianapolis conference was devoted to trying to document those relationships for the Midwest, considerable effort was also expended on perfecting a system of classification that by its very nature would by-pass some of the problems associated with the direct historical approach. That system of classification became known as the midwestern taxonomic method (McKern, 1939). That so many culture historians working in the Midwest immediately embraced it tells us either that they were ready to abandon the direct historical approach or that they were looking for a method that was complementary. Our take (O'Brien and Lyman, 2001) is that the majority of culture historians viewed the midwestern taxonomic method as a logical and complementary partner to the direct historical approach (see below).

The protocol for building the classification was worked out in the years immediately preceding the Indianapolis conference, primarily at the hands of W. C. McKern. A colleague of McKern's at the Milwaukee Public Museum later recalled why the method was needed:

> After the close of the 1929 field season our noon-time discussions began to concentrate on how a cultural classification system could be designed to serve the archaeological needs of the Wisconsin area. It was recognized at the outset that temporal considerations would

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> have to be ignored because no means was available for the relative dating of what had been found. Certain assumptions could have been made about how the prehistoric culture traits had evolved and then one could have arranged the collected data to fit these assumptions. A hypothetical culture sequence could have been created by that approach but that was rejected by both of us as interestingly speculative but not worth the time that would have been required to develop it. What was wanted was a cultural classification system the criteria for which could be agreed to as valid by all who chose to become familiar with it and to use it. When it became unavoidably clear to both of us that temporal and developmental or evolutionary considerations could not be incorporated in the system, it was finally admitted that the system that was needed so urgently would have to be based on morphological or typological considerations alone. (Fisher, 1997, p. 119)

That was exactly what the method *was* based on: "morphological or typological considerations alone." Time and space were to be ignored (at least initially; see below). In principle the method was simple—a branching taxonomy with successively higher levels of inclusiveness. The building blocks of the method were called *components*, defined as assemblages of associated artifacts that represented the occupation of a place by a people. Thus a component was not viewed as being equivalent to a site unless a place had experienced *only* a single occupation (McKern, 1939, p. 308). Artifact trait lists were used to create higher-level groups. An archaeologist polled available components and identified those traits that linked—were shared by—various components; linked components then were placed together in a group termed a *focus*. Simultaneously, one used those same trait lists to identify traits that could be used to isolate one group of components from another group. Five levels of groups were specified. From least to most inclusive these were *focus*, *aspect*, *phase*, *pattern*, and *base*. Three kinds of traits were distinguished: *linked traits* were common to more than one unit; *diagnostic traits* were limited to a single unit; and *determinants* were traits that occurred in all members of a unit but in no other unit (Fig. 2).

Some researchers (e.g., Kehoe, 1990) have equated the midwestern taxonomic method with systems of biological classification that either are based on descent, such as cladistics, or from which implications about descent can be drawn, such as the Linnean taxonomic system. Fisher (1997), in fact, stated that the basic ideas for the method were derived from that system. The Linnaean taxonomy was built in the eighteenth century purely on the basis of the morphometric similarity of organisms; it held no evolutionary implications until after the publication of Darwin's (1859) *Origin* (Ereshefsky, 1994). If the midwestern taxonomic method shows anything in common with biological systematics, it is with phenetics, sometimes referred to as numerical taxonomy (Lyman *et al.*, 1997; O'Brien and Lyman, 2001). Given that time was jettisoned from the midwestern taxonomic method, it cannot be aligned with any system that uses history (ancestry) as a criterion for membership in a taxonomic unit. Geographic space was similarly omitted, no doubt at least in part because Wissler's (1917) age–area theory (Kroeber, 1931b) was no longer perceived as a valid chronometer (Hodgen, 1942; Wallis, 1925, 1945; Woods, 1934).

In the midwestern taxonomic method, form-related units were the building blocks of the classification. No longer did archaeologists have to argue about

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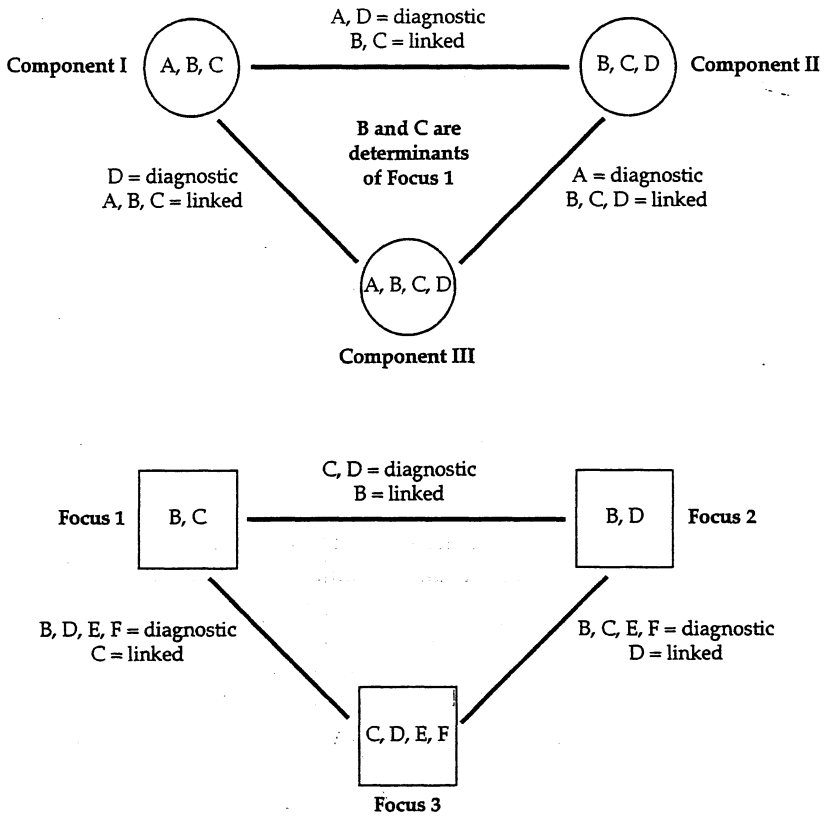


Fig. 2. The analytical relations among traits (capital letters), components (circles), and foci (squares) in the midwestern taxonomic method (after Lyman *et al.*, 1997).

whether shell-tempered pottery was Siouan in origin or grit-tempered pottery Algonkian in origin. Fisher (1997, p. 119) noted that McKern and others freely admitted that *at some future point* "patterns of arrangement" *might* emerge that would suggest not only "cultural relationships but perhaps evolutionary sequences as well"—a point underscored by Steward (1942, p. 337) when he noted that the midwestern taxonomic method was "not necessarily in conflict with the direct historical approach to archaeology." McKern (1937, 1942) indicated that time and space would be considered only after archaeological complexes of traits had been established, at which time analytical efforts could turn to establishing historical linkages with ethnographically documented groups. Thus, archaeologists who used the midwestern taxonomic method regularly opted to include time and space in order to establish such linkages and to write evolutionary history (e.g., Cole and Deuel, 1937; Griffin, 1943; Ritchie, 1937).

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Several archaeologists attempted to integrate the results of classification with ethnographic results—Wedel's (1938) study of the Pawnee being a well known example—and in some cases it appeared that there *was* no conflict between the direct historical approach and the midwestern taxonomic method (e.g., Griffin, 1937; Mott, 1938). For example, Vickers (1948) compared traits in components, foci, and aspects—all units in the midwestern taxonomic method—in Saskatchewan and Manitoba with traits found in local ethnographically documented cultures. On the basis of the comparison Vickers (1948, p. 36) concluded that "similarity of geographical distribution, close correlation in time, known continuation of burial and other traits from the [prehistoric] aspect to the historic tribe . . . all buttress the [inferred ethnic affinity of a particular archaeological manifestation]." Such studies showed that the direct historical approach and the midwestern taxonomic method were compatible. Further, the results were predictable given the short time span between the source and subject sides of the analogy and the fact that they were linked through common ancestry.

The appearance of the midwestern taxonomic method was a clear sign that by the 1930s archaeological emphasis was shifting away from specific historical analogy, with its roots in phylogeny, and toward general comparative analogy. This replacement, however, was only half the story, and it actually was of lesser importance than the other half—the abandonment of Darwinian evolutionary theory in Americanist archaeology in favor of orthogenesis. *That* was the important change. The shift in the kind of preferred analogical reasoning in archaeology received much discussion in the literature (e.g., Anderson, 1969; Ascher, 1961; Binford, 1967; Chang, 1967; Gould, 1974; Lange, 1980; Thompson, 1958; Watson, 1979; Willey, 1953a). The theories that underpinned each kind of analogy were, however, neither identified nor discussed, just as they had not been during the preceding 60 years. Surprisingly, of the three problems attending the use of specific historical analogs—temporal limitations, mosaic evolution, and identifying homologous traits—only the last was mentioned (L. R. Binford, 1968a) after the shift from specific historical analogy to general comparative analogy had been made. Thus, not surprisingly that mention was in a conceptual context other than analogical reasoning. One result of the failure to explicitly consider the theoretical issues was repetition of Fewkes's earlier implication regarding the validity of specific historical analogs (Fewkes, 1896).

GENERAL COMPARATIVE ANALOGY AND ORTHOGENESIS

If our reading of the literature is correct (Lyman *et al.*, 1997), the 1940s and early 1950s constituted a period of considerable change in Americanist archaeology, away from culture history and toward cultural reconstruction. We are not suggesting that culture history was abandoned in favor of reconstructionism nor

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that the former was somehow transformed into the latter. Rather, as our discussion to this point should make clear, Americanists working prior to 1940 had always been aware of their alliance with anthropology. This is why Dixon (1913, p. 558) could claim early on that "archeology is but prehistoric ethnology and ethnography." The use of specific historical analogy by culture historians was geared in large part toward making prehistoric behavior accessible—that is, toward rewriting the archaeological record in ethnographic terms. After about 1945, archaeologists perceived themselves *explicitly* as "anthropologists who dig" (Deetz, 1970, p. 123). Similarly, it was said on more than one occasion that "New World archaeology is anthropology or it is nothing" (Phillips, 1955, pp. 246–247).

Contrary to the received wisdom of the 1960s, chronology and other nonanthropological issues addressed by culture historians were perceived as merely the first in a series of goals—not the final goal—of early twentieth-century Americanist archaeology. As Steward (1942, p. 339) noted, the ability "to describe archaeological materials in terms of time and space [was] the first elementary step toward understanding culture change"—a process he labeled "historical anthropology." Nevertheless, anthropologists (e.g., Bennett, 1943; Kluckhohn, 1949; Parsons, 1940; Steward and Setzler, 1938) on occasion felt compelled to remind archaeologists of what they took to be archaeology's larger anthropological goals and suggested ways that archaeologists might attain those goals, such as by determining the functions of particular artifacts, assessing their roles within a culture, and identifying behaviors signified by the artifacts. All such remarks simply underscored what archaeologists had long believed: that before they could begin to explain the archaeological record in anthropological terms and with anthropologically worded (ethnological) theory, that static record had to be reconstituted into one or more dynamic cultural systems complete with lists of human behaviors.

Analogy was the preferred tool for the job, but as we have seen, there were problems with using specific historical analogy. There was an alternative, however. In an early statement, Griffin (1943, pp. 336–337) referred to "the eventual reconstruction of the life and historic development of the peoples who inhabited the area." A few pages later he noted that the ethnographic record seldom provided the archaeologist with details of material culture. Most importantly, he suggested that "In case definite historical connections cannot be made, correlations between ethnology and archaeology should not be formulated on a tribal-focus basis, but should be established on more general bases, such as the matching of an aspect or phase trait complex against the element list of some such ethnological division as the culture area" (Griffin, 1943, p. 341). The implication of using a culture area as a criterion for selection of an appropriate source analog is unclear in Griffin's discussion, but we suspect it rested on the notion that similar cultures would be found in similar environments, and the latter in part dictated the boundaries of culture areas.

Willey (1953a, p. 229) referred to the alternative to specific historical analogy as "'general comparative analogy,' in which we are interested in cultures for

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comparisons, in cultures of the same general level of technological development, perhaps existing under similar environmental situations." This was precisely the kind of analogy for which the midwestern taxonomic method, with its emphasis on formal properties and deletion of time from consideration, was well suited. Thompson (1958, p. 5) discussed the rationale for using that kind of analogy:

> The archaeologist who formulates an indicated conclusion is suggesting that there is a correlation between a certain set of archaeological material percepta and a particular range of sociocultural behavior. He must test this conclusion by demonstrating that an artifact–behavior correlation similar to the suggested one is a common occurrence in ethnographic reality. What actually happens is that he compares an artifact type which is derived from archaeological data with a similar artifact type in a known life situation. If the resemblance in the form of the 2 artifact types is reasonably close, he can infer that the archaeological type shares the technique, behavior, or other cultural activity which is usually associated with the ethnographic type.
>
> Thus the archaeologist proceeds from basic or descriptive data to a contextual or interpretive inference by demonstrating the existence and validity of various degrees of relation of likeness. This similarity or parallelism of relations is called analogy. Archaeological inference, and particularly that related to the reconstruction of cultural (and ecological) contexts, is impossible without recourse to analogy.

The difference between specific historical analogy and general comparative analogy was striking: In "the general comparative analogy the artifact–behavior correlation derives from a pattern of repeated occurrences in a large number of cultures. In contrast, the specific historical type [of analogy] depends upon the existence of a direct continuity in a single culture or area" (Thompson, 1958, pp. 5–6). In a later and more detailed discussion of the two kinds of ethnographic analogy, Ascher (1961, p. 319) came down decidedly in favor of general comparative analogy because it required no demonstration of heritable continuity between source and subject. Instead, it specified "*boundary conditions* for the choice of suitable analogs" (Ascher, 1961, p. 319, emphasis added). As Ascher (1961, p. 319) put it, "the canon is: seek analogies in cultures *which manipulate similar environments in similar ways*" (emphasis added).

Heritable continuity was not required in general comparative analogy, as it had been in specific historical analogy. Yet several commentators (e.g., Anderson, 1969; Ascher, 1961; Binford, 1967; Chang, 1967; Lange, 1980; Watson, 1979) repeated Fewkes's (1896) earlier implication and indicated that such a connection would strengthen the validity of an ethnographically based analogy. Thus it was not unusual to read something like the following: The greater the degree of "cultural continuity" between the source and the subject, the greater "the likelihood of drawing a correct analogy" (Brumfiel, 1976, p. 398); or, "the tightest analogies are direct historical" (Peterson, 1971, p. 240); or, specific historical analogies "offer an inherently higher degree of probability of interpretation and should be sought [because such analogies offer a] greater probability that the [source analog] derived in the present is applicable to the past" in the geographic area containing the archaeological subject (Gould, 1974, p. 39). Simply put, specific historical analogies "were seen as having an inherently greater probability of being accurate

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approximations of behavioral realities than [general analogies]" (Gould, 1980, p. 35). No one, however, expressed in theoretical terms *why* a historical connection denoting heritable continuity made a specific historical analogy more valid than a general comparative analogy and its attendant "boundary conditions" (Ascher, 1961).

Using wording that echoed the sentiment of attendees at the Indianapolis conference of 1935, Green (1973, p. 140) pointed out that one employs a general comparative analogy when "demonstration of prehistoric–historic cultural continuity is not possible." When this is the case, certain restrictions apply: "General analogy allows that a prehistoric culture may be compared with a contemporary one even though the two are not within the same cultural tradition [or line of heritable continuity]. However, the two groups should be at the *same level of subsistence and live in comparable, although not necessarily identical, environments*" (Green, 1973, p. 140, emphasis added). The warrant for the application of general comparative analogy is that cultures thought to be historical descendants of the prehistoric culture are unknown. The reasons for the restrictions placed on appropriate source analogs are implicit; together, they comprise the notion of convergence, or as Stahl (1993, p. 244) aptly put it, "societies occupying comparable environments pursuing similar subsistence strategies [represent] comparable *stages* of evolutionary development" (emphasis added). This statement requires that we briefly consider the concept of stage.

Prior to the late 1940s, stages were not explicitly defined as analytical units and they were often equated with periods (see the review in Lyman and O'Brien, 2001). So far as we are aware, Krieger (1953, pp. 247–248) provided the first formal definition of a stage as an analytical unit when he contrasted it with a period as an analytical unit.

> I will consider a [cultural evolutionary] "stage" a segment of a historical sequence in a given area, characterized by a dominating pattern of economic existence. The general economic life and outlines of social structure of past peoples can often be inferred from archaeological remains and can be related to similar phenomena, whether the dates are known or not. The term "period," on the other hand, might be considered to depend upon chronology. Thus a stage may be recognized by content alone, and in the event that accurate dates can be obtained for it in a given area, it could be said that the *stage* here existed during such-and-such *period*. Further, the same stage may be said to appear at different times or periods in different areas and also to end at different times. A stage may also include several locally distinctive culture complexes and minor time divisions.

Stages were, as Rowe (1962, p. 43) noted, a "legacy" of eighteenth- and nineteenth-century cultural evolutionism. Most importantly in the present context, stages provide the warrant for the use of general comparative analogy, and that warrant resides squarely in orthogenesis:

> Each stage is supposed to be characterized by a certain pattern of institutions, so that a certain kind of technology is associated with a certain kind of social organization and certain economic, political, and religious ideas. It is this last aspect of [orthogenic] evolutionary theory which is particularly attractive to archaeologists looking for a short-cut to cultural

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> interpretation, for it provides a method of reconstructing those aspects of culture for which the archaeological record provides no direct evidence. (Rowe, 1962, p. 43)

In short, then, within the theory of orthogenic evolution and its attendant stages, similar adaptive problems faced by historically unrelated cultures in similar environments should result in similar adaptive solutions termed "stages" as a result of convergence and/or parallelism. Theoretical reasons as to why convergence should result were not specified, even though Rowe (1962) noted the necessity of such. Appropriate source analogs represent nonhomologous cultural traits that are in fact analogous in a Darwinian sense—that is, they are similar as a sole result of functional convergence. How to determine if similar traits are analogous (the result of convergence) rather than homologous (the result of shared ancestry) was not considered, despite recognition of the problem (L. R. Binford, 1968b). The places to search for appropriate source analogs, then, are among cultures found in environments and with technologies similar to those of the archaeological subject; ethnographic source cultures should be within the same *adaptive stage* as the archaeological subject culture. Adaptive—and by implication behavioral—similarity is the analytical focus, not ethnic identity, history, or heritable continuity, all of which demand an analytical focus on homologous traits.

Most importantly, the requirements of the "new analogy," to use Ascher's (1961) term, not only replaced that of phylogenetic continuity but omitted the nondirectional (nonpredictable) theory of descent with modification. The theory underpinning the use of general comparative analogy is implicit, but it clearly comprises orthogenesis—similar stages and convergence. With implicit reference to the general comparative application of analogy, Binford (1962, p. 219) observed that "the study and establishment of correlations between types of social structure classified on the basis of behavioral attributes and structural types of material elements [is] one of the major areas of anthropological research yet to be developed. Once such correlations are established, archaeologists can attack the problems of evolutionary change in social systems." For such correlations to exist, cultural evolution had to involve cultural stages rendered as sets of functionally interdependent culture traits. Processual archaeologists termed these stages *systems*.

Statements were made such as "The formal structure of artifact assemblages together with the between element [or culture trait] contextual relationships should and do present a systematic and understandable picture of *the total extinct* cultural system" (Binford, 1962, p. 219). Such presumed functional interdependence of culture traits fit well with White's (1959b, p. 8) conception of culture as both a system and "an extrasomatic mechanism employed by a particular animal species in order to make its life secure and continuous." This conception and its attendant definition were adopted by processualists, along with the orthogenic models of cultural evolution and their attendant stages as described by White (1959b), Steward (1955), Fried (1967), and others (e.g., Spencer (1997) and selected references therein).

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Cultural evolution *was*, then, orthogenic—an inevitable (as a result of universal convergence, perhaps, but not necessarily affected by diffusion) linear passage through a set of adaptive stages. Largely because of this, Binford's statement and similar ones made by others resulted in archaeologists devoting their time *not* to developing ways to distinguish between homologous and nonhomologous forms but rather to searching for correlations between human behaviors and artifacts (Lyman and O'Brien, 1997; O'Brien and Lyman, 2000). It was remarked in the 1970s that archaeologists "have probably spent more time in attempting to infer and describe lifeways associated with a given [cultural] stage than in accounting for changes from stage to stage" (Plog, 1973, p. 189; see also Binford, 1977).

Correlations between archaeological phenomena and human behaviors were sought because of the renewed emphasis beginning in the 1940s to make archaeology more anthropological (e.g., Binford, 1962; Caldwell, 1959; Strong, 1952; Woodbury, 1963, 1972). Correlations were believed to exist because cultures conceived of as systems representing evolutionary stages were rendered as sets of functionally integrated traits (Dunnell, 1991). During the rise in popularity of processual archaeology, this belief was questioned by Rowe (1962) and also by Trigger (1968, p. 16), the latter suggesting that "identity or close similarity in material remains [may *not* indicate] identity in all aspects of culture, including language, social structure, and ideology." Nevertheless, it was later suggested by processualists that functional and adaptational similarities, rather than more purely phylogenetic connections (e.g., Greenberg and Spielbauer, 1991), serve as the linkages between an archaeological subject and an ethnographic source. These were little more than general comparative analogies as construed by Willey (1953a), Thompson (1958), and Ascher (1961) couched in terms of cultural systems conceived of as adaptive stages.

By using general comparative ethnographic analogy to reconstitute the archaeological record into something an ethnographer would recognize, archaeologists believed they could contribute to the construction of anthropological theory rather than act merely as technicians in the service of cultural reconstruction. Further, there was an "embarrassment of choices among [anthropological] theories" (Shelley, 1999, p. 603) from which to draw. Given this plethora of riches, one might expect that anthropological theory would have been a source of sufficient explanations for the archaeological record, but this was not the case. Otherwise, archaeologists who themselves study source-side analogs (e.g., Binford, 1977; O'Connell, 1995; Schiffer, 1996; Simms, 1992; Watson, 1979) would not keep calling for the construction of general explanatory theory.

Although commentators in the 1960s did not mention many of the problems with specific historical analogs that their intellectual predecessors had identified, they did worry about one problem with general comparative analogy: the fact that the restrictions of similar environment and similar technology placed on general comparative analogs could still result in multiple source analogs. The problem was perceived to be so great that Howell (1968, p. 287) suggested that "reconstruction

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efforts [based on ethnographic analogies] be discouraged or very severely curtailed except for very recent time periods." Part of the reason for the repeated calls for general theory applicable to the archaeological record may well have resided in the fact that, as Shelley (1999) indicates, the use of multiple-source analogs can, and often does, result in a loss of resolution because of conflicting implications of the sources—a fact underscored by the use of multiple general comparative source analogs (e.g., Binford, 1967, 1972; Munson, 1969). The single-source analog demanded by specific historical analogy avoided this problem, but as was noted more than once, use of the single-source analog suggested by the direct historical approach "lessens the number of possible interpretations of an artifact, although it cannot guarantee completely valid interpretation" (Anderson, 1969, p. 134; see also Watson, 1979). This problem has never been solved, as evidenced by the diversity of suggestions regarding how the number of possible interpretations might be reduced (e.g., Wolverton and Lyman, 2000; Wylie, 1989).

DISCUSSION

A change in the analytical goal of Americanist archaeology—from one focused on historical questions to one focused on ahistorical anthropological questions—resulted generally in expanded use of ethnographic analogy and particularly in an increased use of general comparative analogs. Not only were specific historical analogs not always available, the direct historical approach was used less and less as a chronometer because first stratigraphy and then radiocarbon dating usurped that role (Nash, 2000; O'Brien and Lyman, 1999a). By the late 1940s, the archaeological record was sufficiently well known to allow syntheses of local and regional cultural chronologies (Martin *et al.*, 1947; Willey and Phillips, 1955, 1958), and the time was ripe to move on to new problems (Caldwell, 1959). One result was Ascher's belated birth announcement of the "new analogy," although as we have shown there was nothing particularly new about it (Ascher, 1961). It had always been there, eclipsed for a while by specific historical analogy, but still present in the archaeological tool kit.

In an early programmatic statement characterizing general comparative analogy, Hewett (1908, p. 591) stated that the "subject matter [of American archeology] lies mainly in the prehistoric period, but this must be studied in the light of auxiliary sciences which have for their field of investigation the living people." Interestingly, Hewett (1908, p. 593) went on to remark that study of "cultural process" was accomplished "in the light of facts which ethnology lends to the interpretation of [prehistoric] phenomena." And while he noted that the study of language history and evolution required direct historical connections, he implied that the study of the history and evolution of "cultural phenomena" did not (Hewett, 1908, p. 592). The implication is that what would later become known as general comparative analogy was sufficient in the latter case. An early example of using

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general comparative analogs was Harlan Smith's study entitled "The Prehistoric Ethnology of a Kentucky Cave" (Smith, 1910). Smith called on various ethnographically documented cultures in North America to sort prehistoric artifacts into those used by men and those used by women; to infer that a deer bone displaying a particular kind of artificial modification had been used as a hide scraper; to infer that deer toe bones with particular artificial modifications had served variously as gaming pieces or ornaments; and to derive other, similar analogy-based inferences of human behavior and artifact function. Heritable continuity played no role in Smith's study; it was purely general comparative analogy.

Because archaeologists had pride of place in having access to the full extent of humankind's past, they believed they could contribute to anthropological theory, particularly with respect to the processes of cultural evolution. They did not recognize that when they used general comparative analogs they implicitly adopted a different version of evolutionary theory than that underpinning specific historical analogy. White (1947, p. 175) did not help matters when he remarked 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." The implication that specific historical analogs could be applied under orthogenesis was unqualified—a point made all the more curious by the fact that research on the Plains (e.g., Strong, 1933, 1935, 1936; Wedel, 1936) had made it abundantly clear that such an application was ill-advised.

Kroeber (1923, pp. 7–8) did not confuse the two theories, but others did. Nelson (1932, p. 103) conflated the two when he "propose[d] to consider 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." The conflation was further compounded by Nelson's (1932, p. 111) remark that the mechanism of change was not natural selection working on undirected variation but rather "the old adage that necessity is the mother of invention"—in other words, variation directed by human intentions. This had a later parallel with White's (1943, p. 339) claim that the human "urge" to improve was the "motive force as well as the means of cultural evolution" (see Dunnell, 1989). Such conflations of the two theories contributed to Americanist archaeologists' failure to recognize the difference in underpinning theory that accompanied the differences between specific historical analogs and general comparative analogs. Even if they had recognized the difference (and it is clear they did not), by the mid1940s they had no choice but to change.

Discard of the theory of descent with modification by Americanist archaeologists had begun early in the twentieth century for various reasons; the two typically mentioned ones were that cultural evolution was faster than biological evolution and that cultural evolution was reticulate whereas biological evolution

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was diversifying (e.g., Gladwin, 1936). Thus there were reasons not to model cultural evolution on Darwin's theory. A few archaeologists, such as Colton and Hargrave (1937), attempted to use biological methods in archaeology, specifically taxonomic methods, but they were brought up short in their endeavors by overwhelming criticism (e.g., Brew, 1946). By the mid-1940s the disposal of Darwinian evolutionism was virtually complete (Lyman and O'Brien, 1997), leaving orthogenesis, a theory then being resurrected by White (1943, 1945, 1947) and Steward (1955), as the only game in town. Use of that theory of evolution was, we suspect, reinforced by the observation of evolutionary trends in the archaeological record (e.g., Willey and Phillips, 1955, 1958). Our suspicion is founded on the fact that paleontology was one of the last strongholds of orthogenesis within biology, in part because long spans of evolutionary trends were visible (Bowler, 1979; Gould, 1982). Straight lines of evolution were apparent among various sets of fossils to some paleontologists, and some but not all of them explained those lines in terms of orthogenesis, although by the mid-twentieth century most paleontologists had discarded orthogenesis in favor of descent with modification (Jepsen, 1949; Romer, 1949; Simpson, 1953; Watson, 1949).

Paleontologists who preferred descent with modification to orthogenesis noted that evolution was not a line or ladder but rather a bush (Simpson, 1953); that any appearance of a line was the result of the way in which paleontologists had classified and arranged fossils against time (Jepsen, 1949); and that an evolutionary line could be said to be "directional [only] when the direction is known" (Jepsen, 1949, p. 493). Today, apparent evolutionary lines rendered as character gradients or chronoclines (Koch, 1986) within a clade are labeled "trends" (Alroy (2000) and references therein) and are explained as resulting from various constraints and mechanisms "channeling" evolution in particular directions (e.g., Gould, 1982, 1988) rather than from some mysterious force directing the appearance of new variants. Once a trend is identified, the challenge is to explain it within the bounds of the theory of descent with modification (Alroy, 2000; McShea, 2000).

The appearance of trends in the cultural record was in large part a result of the classification of archaeological manifestations into stages (e.g., Willey and Phillips, 1955, 1958). As Rowe (1962, p. 51) indicated early on, "because of the close association of stages with the theory of cultural evolution, virtually every archaeologist who uses stages to organize his data thereby builds into them certain assumptions about cultural development without being aware that he is doing so. Later, in making his cultural interpretations, he discovers the pattern of cultural development which was assumed in his system of organization [the stages] and thinks that he is deriving it empirically from the data." Dunnell (1989, pp. 178–180) later expanded on this point, underscoring the fact that the cultural traits chosen as definitive of particular cultural evolutionary stages are typically derived from a sample of societies thought to differ in terms of the evolutionary stage each represents. Not only will those definitive traits vary with the societies polled, they

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comprise empirical generalizations. As a result, trends in orthogenically conceived cultural evolution were, and are, difficult to explain.

In our view, because the stages comprise poorly defined analytical units (Leonard and Jones, 1987; Rambo, 1991), meaning that the stages are really little more than empirical generalizations, the orthogenic theory of cultural evolution necessitates concepts such as "devolution" (Carneiro, 1972)—a concept that ignores the fact that evolution can be in either direction along the straight line of orthogenesis (Alland, 1974). Paleobiologists (e.g., Gregory, 1936; Simpson, 1960) grappled with this problem of apparent reversals, often referring to Dollo's law, which states that temporally remote character states do not recur in *exactly* the same form at a later time. Rather, the manner in which phenomena are classified makes it *appear* as if a reversal occurred. As Gould (1970, p. 208) makes clear, Dollo was careful in phrasing his law precisely because of his interest in "the historical nature of evolutionary events." Dollo's law of irreversibility "functions as a guarantee that convergences can be recognized by preservation of some ancestral structure (incomplete reversion). Convergence is the major impediment to phylogenetic interpretations" (Gould, 1970, pp. 206–207). In modern terms, Dollo's law allows evolutionists operating under the theory of descent with modification to distinguish between analogous (convergent) character states and homologous character states, one subset of the latter of which—shared derived character states—are used to reconstruct phylogenetic histories (e.g., O'Brien *et al.*, 2001).

An implicit notion of Dollo's law attended use of the direct historical approach as a chronometer; otherwise, it could not have been used as such. The focus on homologous traits, necessitated when the direct historical approach was used as a chronometer, reflects that notion. Some cultural traits displayed cycles and recurred at various times (Kroeber, 1919; Richardson and Kroeber, 1940), a fact recognized by some archaeologists (e.g., Kroeber, 1916; Rands, 1961), who insisted that chronologies of artifacts erected on the basis of the direct historical approach and frequency seriation be tested with stratigraphic data. This concern evaporated with the invention of radiometric chronometers in the 1950s. Simultaneously, there was a bit more explicit consideration of parallelism and convergence and of why these processes should occur in independent cultural lineages (e.g., Rands and Riley, 1958). Orthogenesis held the answer: Convergent and parallel evolution were to be expected because the process of orthogenic evolution not only "meets basic needs" but creates similar cultural patterns by its "normalizing powers" (Steward and Shimkin, 1961, pp. 486, 484). Spencer's "unknown force" was revealed to be human needs and urges.

Because orthogenic cultural evolution dictated that similar evolutionary results be produced, convergence was to be expected. Convergence was not used as evidence of the power of natural selection; it was instead used as evidence of orthogenesis. Without demonstrating that convergence had in fact occurred, however, conclusions were tautological, just as Rowe (1962) had indicated.

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CONCLUSIONS

Although many archaeologists prior to 1950 professed to know what the direct historical approach was and how to use it, no one described its analytical protocol in detail. We suspect this was not only a result but a cause of its being used for three distinct analytical purposes—as an ethnic identifier, as a chronometer, and as a warrant for specific historical analogy. The theory underpinning the approach has not previously been considered in detail, perhaps because two of the three uses to which it could be put are readily accommodated by either descent with modification or orthogenesis. That the use of the direct historical approach as a chronometer was underpinned by the theory of descent with modification has not been previously recognized for two reasons. First, that theory had been discarded by 1950, and second, the role of the approach as a chronometer was usurped by other chronometers at about the same time.

The ultimate goal of Americanist archaeologists to be contributors to anthropology rather than mere users of its products was thought to be attainable by the 1950s. It was also thought that that goal could be reached only by rendering the archaeological record in ethnographic terms (Lyman and O'Brien, 2001). Such renderings could then be subsumed under the emerging orthogenic evolutionism of White and Steward. The century-long failure to explicitly distinguish between the particular theory of evolution that informed the use of specific historical analogy and the theory that informed the use of general comparative analogy resulted in the 1960s and 1970s in claims that the former was more valid than the latter. Given the distinctive theories that underpin each kind of analogy, we suspect that the reasoning underpinning this belief was as follows: Cultural traits of a direct historical analog are similar because they share ancestry and are also functionally and behaviorally similar *precisely* because they have (metaphorically) genetic affinity. Thus, the similarity of an ethnographic source and an archaeological subject of a direct historical analogy has two causes—a historical affinity or genetic connection and also a functional–behavioral connection. For a general comparative analogy the cause of similarity is singular; the ethnographic source and the archaeological subject are similar as a result of a functional–behavioral connection rendered as convergence. The historical connection of a specific historical analogy is, it seems, the root of the belief that this sort of analogy is better than a general comparative analog. Either that, or those expressing the belief doubted that convergence was universal.

The cause of similarity between cultural traits in general comparative analogy (implicitly) comprises not only orthogenesis but also convergence, the warrant for which is provided by the boundary conditions of similar technologies and environments. Evolutionary convergence is, however, more often assumed than demonstrated. This notion harks back to anthropology of the seventeenth through nineteenth centuries and denies that existing primitive cultures used as analog

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sources have evolutionary histories (Hodgen, 1964; Stocking, 1987; Van Riper, 1993). Thus existing primitive cultures are treated conceptually and analytically as evolutionarily stable states that first appeared in the remote past. Living fossils such as the coelacanth (Gorr and Kleinschmidt, 1993) do exist, but paleontological evidence shows just how fallacious using such phenomena as behavioral–functional analogs can be (e.g., Crompton and Parker, 1978). Descent with modification and orthogenesis have regularly been confused by Americanist archaeologists. The history of the direct historical approach and its role in ethnographic analogy reveals some of the roots of this confusion.

On the one hand, use of the direct historical approach as a chronometer is hardly mentioned in recent historical overviews (Lyman *et al.*, 1997; Nash, 2000; O'Brien and Lyman, 1999a), perhaps because it seems to be seldom used in this way these days. Its uses as an ethnic identifier and as a source of analogs, on the other hand, seem assured for at least the immediate future. The Native American Graves Protection and Repatriation Act mandates the ethnic identification of human remains, and the direct historical approach as an ethnic identifier can be and is being used to serve that purpose. As the references cited above demonstrate, modern Americanist archaeologists continue today to seek source analogs that are evolutionarily linked with archaeological manifestations in the belief that such specific historical analogs are better than general comparative ones. All three uses are underpinned by the assumption that there is evolutionary continuity of some scale between the present and the past, an assumption that warrants more detailed study (see Dunnell (1991) for an initial effort). Understanding how and and why the direct historical approach works in each of the three uses to which it has been, and is being, applied is mandatory to its continued application.

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