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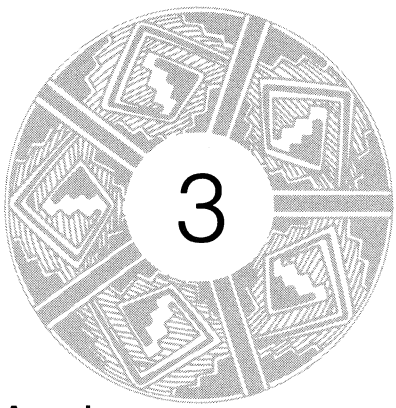

The Reaction against Analogy

ALISON WYLIE

> To confine our studies to mere antiquities is like reading by candle-light at noonday.
> (Daniel Wilson 1861, as quoted by Orme, 1974:200).

INTRODUCTION

However much analogical inference has broadened interpretive horizons and however indispensible it has seemed in the struggle to make archaeological data serve as evidence of the cultural past, inference by analogy has long been an object of uneasy mistrust among archaeologists. In fact, this mistrust seems to have grown steadily during the past few hundred years despite the essential role that Orme (1973, 1974, 1981) shows analogical appeals to an expanding repertoire of ethnographic sources to have played in shaping our contemporary conceptions of prehistory. It became a persistent, even dominant, theme in the methodological literature of contemporary archaeology, originally, it would seem, because the use of analogical argument by nineteenth-century evolutionism threw its potential unreliability into particularly sharp relief. Various attempts were made to salvage analogical inference as a respectable methodological tool and a parallel series of criticisms were produced stressing its inherent and insuperable insecurity. The culmination of this exchange is Ascher's seminal paper on analogy (1961) in which he optimistically counters the

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existing challenges with a series of proposals for "placing analogy on a firmer foundation" (1961:323). With the advent of a self-consciously "scientific" archaeology, however, these constructive suggestions were rejected out of hand on the ground that no amount of reformulation or restriction of analogical inference could establish its conclusions with the security appropriate to properly scientific research.

What has ensued is a reaction against analogy in which historical mistrust of its inconclusiveness has taken on entirely new proportions. It is insisted that, at the very least, the use of analogical inference in archaeological research should be strictly limited; analogy should serve only as a means of generating hypotheses whose credibility must be established on independent, nonanalogical grounds. The more extreme critics, like Freeman (1968) and, now, Gould (1980; Gould and Watson 1982), argue that analogical inference should be denied even this restricted role because, in their view, it inevitably distorts and limits what archaeologists can understand of the past; it is, by definition, an assimilation of past to present that obscures the possible uniqueness and diversity of past cultural forms. Given this radical mistrust of analogy, Gould declares unequivocally that "analogy is an idea whose time is gone" (1980:x). It can and should be entirely replaced by nonanalogical methods of formulating and evaluating interpretive inferences in archaeology.

In the first sections of this essay I provide an overview of the developments in thinking about analogy leading to this newly radical reaction against it. A critical analysis of this reaction follows, the point of which is to show that the extreme critics have failed to identify or formulate viable alternatives to analogical reasoning. In fact, I argue that the alternatives they propose are, themselves, unavoidably analogical in form and foundation, thus, most archaeological inference remains analogical. This does not mean, however, that archaeological interpretation is reduced to mere speculation. These critics have also failed, I contend, to establish that analogical inference is radically faulty—that is, categorically unreliable and misleading—as they claim. In the final sections of the essay I argue, first, that the recent, most outspoken critics inadvertently provide valuable insights as to how to improve the standing of analogical inference; second, that these insights complement the suggestions made by earlier methodologists (i.e., Ascher and his immediate predecessors); and, finally, that standard logical analyses of analogy provide a framework for characterizing the content of these convergent proposals in general terms. My thesis, then, is that though a candid appreciation of limitations is appropriate where analogical inference is concerned, its use in archaeological contexts is neither dispensible nor radically faulty. It can play a legitimate, constructive role in archaeological injury if used subject to the methodological constraints that have been emerging, under pressure of increasingly sharp criticism, since the inception of a methodologically self-critical archaeology.

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HISTORICAL AMBIVALENCE ABOUT ANALOGY: OBJECTIONS AND PROPOSALS

Early Uses and Abuses of Analogy

The very early uses of analogical reasoning are characterized by an expansive unmitigated enthusiasm for its potential as a source of insights about prehistory. They have been discussed by Charlton (1981), who traces them back to classical Athenian historiography, and by Orme (1974, 1981) in connection with her analyses of the impact that expanding ethnographic knowledge has had on archaeologically based conceptions of prehistory since the sixteenth century. With regard to these developments, Orme argues that contact with contemporary "savages" made it possible to conceive of British and, more generally, of European prehistory in entirely different terms than when it had been understood exclusively in terms of the life world of sixteenth-century Europe and its historically documented antecedents. At what Orme calls the "practical" level of "recognition and interpretation of artefacts" (1981:2), it became possible to reinterpret whole classes of enigmatic material (previously ascribed mythic or magical significance) as artifacts of human, prehistoric origin. This led, slowly, to the broader realization that the ancestors of the modern Britons very likely included "men as savage as the Indians who lived long before the start of recorded history," that is, "before the Roman Conquest" (1981:31). On the face of it, this constitutes the sort of broadening of interpretive perspectives that has traditionally seemed to vindicate enthusiasm about analogy, leading its proponents to see it as an antidote to narrow ethnocentrism and as a rich source of insights about "varied and heterogeneous reasons or causes" that may account for otherwise enigmatic archaeological materials (Ucko 1969:262).

There was, however, another side to these early, horizon-expanding uses of ethnographic analogy. A secondary outgrowth of the "change of attitude" about prehistory documented by Orme was the development of general interpretive schemes at a theoretical level, that is, schemes for "discerning and explaining the processes of human cultural development" (Orme 1981:2). These ultimately served to underwrite the notoriously intemperate uses that classical evolutionists made of ethnographic analogy. With this, the liabilities of dependence on ethnographic analogy were made clear, giving rise to the ambivalence and mistrust that has since characterized its reception in archaeological contexts.

Two features of the emergence of these schemes are relevant here. First, by the eighteenth century, Orme finds the interpretive comparisons of prehistoric and primitive peoples so thoroughly absorbed by antiquaries that they simply and unquestioningly equate the prehistoric with the (modern) primitive (1981:11, 1973:489). Second, as ethnographic contacts and reports proliferated, a great variety of "primitive" forms of life were recognized to exist in the present,

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suggesting, on the basis of the prehistoric–primitive equation, that human prehistory was vastly more complex and diverse than originally thought. This variability was given structure and made intelligible by nineteenth-century evolutionary schemes according to which contemporary cultures were understood to embody differing degrees of cultural achievement and were, in turn, projected onto the past as stages in a determinate, historical course of development. The most primitive contemporary cultures were thus presumed comparable to the earliest prehistoric forms of "savagery"; they comprise the evolutionary starting point in a sequence of technological, economic, and political stages of development that culminate, naturally, in the industrialized civilizations of Great Britain and Western Europe. Once formulated, this speculative scheme functioned, in turn, as a template for the interpretive reconstruction of particular prehistoric cultures wherever a consideration of archaeological materials entered into their interpretation. Ascher cites this as the first systematic use of analogy in archaeological interpretation (1961:317).

The classic example of analogical interpretation conceived in the tradition of nineteenth-century evolutionary thought is Sollas's much-cited series of lectures, *Ancient Hunters,* in which four ethnographically documented hunting cultures are identified as the contemporary counterparts of four archaeologically known prehistoric "ages." (The lectures were originally delivered in 1906, and the third published edition of the text, cited here, appeared in 1924.) In selecting these interpretive analogs, Sollas was directly influenced by Tylorian evolutionism, which is to say, his interpretation proceeds on the unquesioned assumption that the modern ethnographic "primitives" he cites "represent" their prehistoric counterparts in the strong sense of being, quite literally, their descendants. He argues that the populations who originally developed the prehistoric hunting adaptations comprising his four prehistoric ages would each have occupied "what is now the focus of civilization" (1924:599) during the period when they represented the highest level of human cultural achievement. As successively more "intelligent" races (i.e., "races" that had developed more sophisticated adaptive strategies) emerged to displace them, each would have been "expelled and driven to the uttermost parts of the earth" where, on Sollas's account, their descendants live to this day in an arrested, "primitive" state of development. It is given this literal construal of the descriptive metaphors used to characterize modern primitives as "survivals" or "representatives" of past forms of life that Sollas is able to conclude, as follows, in interpretation of the archaeological record of specific ages of prehistory:

> The Mousterians have vanished altogether and are represented by their industries alone at the antipodes; the Aurignacians are represented in part by the bushmen of the southern extremity of Africa; the Magdalenians, also in part, by the Eskimo on the frozen margin of the North

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> American continent and, as well, perhaps, by the Red Indians, on the one hand, and, on the other, by the Gaunches and sporadic representatives in France. (1924:599)

With Sollas, then, the formal relations of comparison set up in the sixteenth century between prehistoric cultures and modern "primitives" are supplanted by the presumption that actual, historical, or "genetic" connections exist between the prehistoric subject cultures and their contemporary analogs (Clark [1951] introduces the term *genetic* in this connection). To put it another way, in Sollas' hands these formal relations of comparison are reified and the evolutionary theory, itself an interpretive postulate loosely based on analogy, is treated as a factual account of prehistory. This effectively predetermines the answers to virtually all specific interpretive questions that might be raised about prehistoric cultures. Rather than functioning as a source of guidelines for selecting analogs, it dictates that prehistoric subjects will be literally assimilated to the contemporary cultures that are assumed, on the encompassing theoretical scheme, to "represent" the same stage of evolutionary development.

Though Sollas was, to some degree, selective in this assimilation, often recognizing partial representation of past in present, the essentially arbitrary nature of his interpretive scheme is unavoidable. No matter how striking the factual anomalies and, as Ascher observes, these were ubiquitous (1961:318), the encompassing theoretical framework was never, itself, considered open to question. Thus, far from furthering the liberation of antiquarian interpretation from its original ethnocentric limitations, Sollas's scheme represents a reemergence of the earlier, precontact pattern of interpretation by projecting the familiar present (now much expanded ethnographically) directly onto the past. In this case, rather than postulating a past "peopled with characters from Caesar and Tacitus, living in a world curiously akin to the sixteenth century" (Orme 1981:3), prehistory is envisioned as having been peopled by "savages" not unlike those that had recently been subjugated by Europeans and exemplifying a course of development governed by a principle of "right . . . founded on might" (Sollas 1924:599) not unlike that that governed the politics of nineteenth-century imperialism.

The ideological nature of Sollas's creation of a past in the image of the present is explicit when he argues that "The facts are clear . . . justice belongs to the strong, and has been meted out to each race according to its strength; each has received as much justice as it deserved" (1924:599). Because he takes it that each instance of "dispossession" of a weaker race by a stronger one has marked a step in "the intellectual progress of mankind" (a striking instance of theoretical predetermination of how specific past cultures will be understood) he infers that it is "a duty which every race owes to itself and to the human family as well, to cultivate by every possible means its own strength" (1924:600).

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Reactions against the Excesses of Classical Evolutionism

First Criticisms

What the critics of analogy originally reacted against were analogical interpretations that, like Sollas's reconstructions, represent a "simple and direct reading of the past from the present" (to use Gould's phrase, 1982:446). This reaction was not restricted, however, to these worst-case examples of radically overextended analogical reasoning. As is shown in the next section, it was very quickly extended to analogical inference considered as a class; Sollas-type cases were feared to exemplify certain dangers, namely certain liabilities to error, inherent in any use of analogy. The source of this worry, conceptually, is the recognition that such inference inevitably presupposes some set of general reasons, some form of uniformitarian justification, for assuming that the similarities known to hold between particular classes of interpretive subjects and their potential analogs are indicative of the further specific similarities inferred in the interpretive conclusions of analogical arguments. Not only are the scope and reliability of such assumptions inevitably suspect, leaving the inferences based on them inconclusive (this is a worry that is discussed in more detail below), but, in making such assumptions there is always what Clark describes as "the real danger of setting up a vicious circle and of assuming what one is trying to discover" (1951:52). In Sollas's work this danger is realized; he assumes, in reifying the comparative relations that evolutionary theory suggest may hold between modern and prehistoric "primitives," precisely the similarities that his analogical arguments are meant to establish.

The larger worry inspired by the critical reaction against Sollas-type interpretations is that where it is always impossible to inspect the past and "prove" the truth or falsity of either general, inference-guiding assumptions of uniformity or of specific interpretive conclusions, there is nothing to preclude systematic error in our characterizations of the past in terms of the present. We lack any independent access to the past on the grounds of which we might decisively disprove analogical assumptions and conclusions. It is in consideration of this possibility that the more radical critics of analogy characterize all analogical argument as, in some degree, an arbitrary "reading of the past from the present" whose accuracy cannot be established or reliability even assessed. The possibility of error, as exemplified by cases like Sollas's interpretations, is taken to establish not just that analogical inference is always liable to error, but also that it inevitably distorts our understanding of the past because it requires that this understanding be constructed in the image of contemporary cultural forms or, in the image of the archaeologists' ethnocentric understanding of them.

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Constructive Responses: Restricted Principles for Selecting Analogs

Writing about the state of anthropological research in 1939 and, more specifically, about archaeology in 1940, Kluckhohn reports what he took to be the lingering effects of overreaction to the excesses of evolutionary speculation. These consist of a persistent and debilitating wariness of any of the "more abstract aspects of anthropological thought" (1939:328), including both general theorizing and the construction of particular interpretive inferences. It was assumed, his account suggests, that the only way to avoid systematic error was to eliminate any interpretive, theoretical extrapolation beyond "the facts." Hence, anthropology was characterized by a general preoccupation with "the accumulation of facts" and archaeology by "a good deal of obsessive wallowing in detail of and for itself" (1940:42). In addition, Kluckhohn found that this paralyzing cautiousness was self-perpetuating because it was accompanied by conspicuous avoidance of any reflective, methodological discussion such as might expose the futility of this "narrow empiricism."

By calling attention to these features of postevolutionist anthropology, Kluckhohn's concern was precisely that of reopening methodological discussion. In this, he wanted to demonstrate not only that it is impossible to eliminate all interpretive, theoretical inferences beyond the data—the very identification of a body of empirical phenomena as "archaeological" presupposes a vast network of such inferences—but also that this restrictive policy is profoundly counterproductive. Archaeologists should be warned, he insisted, that unless they develop the methods and theory needed to make effective use of their data as evidence relevant to larger anthropological and historical problems, they will risk "finding themselves classed with Aldous Huxley's figure who devoted his life to writing a history of the three-pronged fork" (Kluckhohn 1940:43). Kluckhohn's intent was, then, to challenge the complacent strategy of evading crucial methodological issues, thus encouraging archaeologists to confront the evolutionist debacle directly, to learn from it and to begin to develop interpretive procedures for "giving meaning" to archaeological data without resorting to arbitrary speculation.

If not its immediate impetus, this challenge at least coincided with a reawakening of interest in interpretive inference among archaeologists, one component of which was an explicit concern to formulate strategies for better handling analogical inference. Ascher's seminal paper, "Analogy in Archaeological Interpretation" (1961), was a synthesis and extension of proposals that had been put forward for improving the standing of analogical inquiry during the two decades following Kluckhohn's critical essays. I suggest that two strands of thinking can be discerned in the ongoing efforts to improve analogy reported by Ascher. The

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first consists of attempts to rectify specific errors associated with evolutionist interpretation while the second constitutes a response to more general worries that were raised during this period by the development of increasingly comprehensive criticisms of analogy. I have considered, first, those proposals that take the recognized weaknesses of evolutionist reconstructions as their primary concern; these are incorporated into Ascher's account as the first of three recommendations that he makes for securing analogical inference, though with some important amendments (to be discussed at the end of this section). I then considered the further strategies for strengthening analogy that were proposed in response to the objections of its new and increasingly vehement critics. Three distinct strategies are represented in this literature in my assessment, one of which comprises the basis for the second two of Ascher's recommendations. In reviewing these pre–New Archaeology responses and counter-responses to worries about analogy, my purpose is to show that Ascher's proposals emerged out of a particular problem context and have considerably more credibility than is acknowledged when they are judged in terms of the ideals and objections later formulated by the advocates of a scientific archaeology.

As Ascher's analysis suggests, much of the thinking about analogy during this period took the recognition of specific weaknesses of evolutionist interpretation as its point of departure; attempts were made to show that these weaknesses might be avoided if the interpretive principles underlying evolutionist reconstructions (the reified assumptions of uniformity) were restricted and adequately substantiated. Thus, it was required that, rather than presume genetic connections to exist whenever general theories suggest that particular prehistoric and contemporary cultures may be "comparable," archaeologists should choose (or seek out) analogs among living cultures where actual historical ties to the prehistoric subject can be demonstrated to exist. One proponent of this view, Clark, argued that archaeologists "might well pay more attention to the Folk-Culture of the area in which they happen to be working" (1951:55) on the principle that where cultural continuity can be demonstrated, some features of antecedent, prehistoric ways of life may be expected to survive in the highly conservative "rural substratum" or "peasant basis" of contemporary societies.

His reason for recommending a strategy of exploiting real genetic connections whenever possible was the widely shared conviction that "analogies torn from their historical contexts may be very deceptive" (1951:55). The assumption seems to be that if historical continuity is established, then "historical context" can be assumed to be more or less constant. This, in turn, was seen to justify the assumption that known material similarities between a prehistoric culture and its historically connected analog are likely not just accidental; the attributes in question may be presumed to have arisen under similar conditions, in association with the same behavioral or functional variables. This assumption seems typical of almost all subsequent literature on analogy inasmuch as "direct historic"

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analogies are routinely presented as the preferred mode of analogical interpretation.

Given, however, the "vast temporal and spatial tract" (Ascher 1961:319) for which there is an archaeological record but no surviving, historically connected analogs, even strong advocates of "direct historic" analogy like Clark recognize a legitimate role for "new" or "unconnected" analogs. Clark's specific proposal was that archaeologists should adopt a limited "comparative" method for selecting analogs that are similar in relevant respects to the subject of prehistoric contexts in question; the notion of relevance is defined through refinements of the interpretive principles that had informed evolutionist reconstructions of prehistory. Clark clearly rejects any assumption that cultures represent determinate "stages" in a "unique and universal" model of cultural development (Ascher 1961:319), or that membership in such evolutionarily defined classes assures extensive similarities. He does, however, recommend that archaeologists seek analogs, on a case-by-case basis, among cultures "at a common level of subsistence . . . existing under ecological conditions which approximate those reconstructed for the prehistoric culture under investigation" (Clark 1953a:355). In this, he accepts (though he does not explicitly articulate) a loose, neo-evolutionist principle to the effect that, where cultures are similar with regard to the environmental factors that impinge on them and in their technological means of dealing with these factors, it is especially likely that they will be similar in other respects as well (environmental and technological similarities are taken to be especially relevant indicators of further, cultural similarities). Presumably the 'other respects' in question are features of cultural systems that relate directly to survival in a given environment by given technological means, but Clark does not specify what or how extensive they are. In his own interpretations of Star Carr (1954), he takes an exceedingly wide range of cultural phenomena to be reconstructable on the basis of this principle. He formulates interpretive conclusions not only about the subsistence but also about the demography, the internal division of labor, and the social organization of the Mesolithic community at Star Carr, all on the basis of limited similarities between the environmental context and material technology of this community and that of the "hunting peoples of North America and Greenland" (1954:12). This, then, comprises the second of two principles for selecting analogs that resulted when, on Asher's account, the broad uniformitarianism of classical evolutionary theory was "partitioned" and "set in a restrained format" (1961:318–319) by archaeologists who took up the challenge I have found articulated by Kluckhohn.

New Skeptical Doubts

Ascher takes these refinements of evolutionist reasoning to be a promising development—as indicated, he incorporates these interpretive principles into the first of his three proposals for "placing analogy on a firmer foundation"

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(1961:322). He and, later, Orme (1974), acknowledge the parallel development of an increasingly pessimistic tradition of criticism of analogy. This took, as its point of departure, a candid mistrust of the sanitized and restricted forms of analogical reasoning that was expressed even by their strongest proponents.

Clark, among others, was quite explicit on the point that "We know from our knowledge of living peoples [that] great diversity of cultural expression may be found among communities subject to the same economic limitations and occupying similar, if not identical environments" (1953a:355). Clark was even prepared, in fact, to recognize that this potential plasticity and diversity of cultural expressions might undermine the reliability of Folk-Culture analogies. He pointed out, in the same contexts where he advocated the use of historically connected analogs, that primitive cultures and the primitive components of the "highly civilized parts of Europe" might themselves have a developmentally complex history; historical continuity might be continuity through profound change and this change might well have affected even the most apparently stable and anachronistic aspects of the descendant cultures.

In response to his doubts about Folk-Culture analogy, specifically, Clark suggested that archaeologists use a "critical historical method" to "strip away the civilized accretions and reveal the essential barbarian core" (1951:57) whenever they appeal to Folk-Culture analogs in interpretation. Where this still admits a worrisome degree of arbitrariness—Clark observes that "prehistorians are liable to select evidence from Folk-Culture which suits their own interpretations of the archaeological evidence" (1951:61)—it is recommended that Folk-Culture analogy be reinforced by establishing economic commanalities between the prehistoric subject and its historically connected analogs. "Economic history," Clark says, "forms a true connecting link" (1951:61) on which archaeologists should rely as much as possible. In effect, then, Clark argues that whether, or in what respects, descendant cultures may be counted analogs of their forbears depends, ultimately, on whether and to what extent they are technically and adaptively similar. The "critical" use of historically connected analogs is a matter of applying, to them, the evolutionist-derived criteria he proposes for selecting "new" unconnected analogs. Presumably the combination of historical connection and ecological–economic similarity is held to assure that the "historical context" of proposed analogies has been stable over time and that this greatly improves the reliability of these interpretive inferences.

Inasmuch as this leaves historic analogy ultimately dependent on what has since been called "new" analogy, Clark's reservations about the latter must be taken to apply to the former. That is to say, historically connected analogies must be presumed to be on the same footing as unconnected analogies with regard to error due to the possibility, cited above, that cultures may diverge sharply in their responses to given economic–ecological constraints. This underlines the inescapable fact, which has counted heavily with the critics of analogy, that none of

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the criteria for selecting analogs—neither historical connection nor economic—ecological comparability—can guarantee that any given contemporary culture will replicate in itself the complex association of attributes distinctive of the prehistoric cultures represented in the archaeological record. One critic, whose position is discussed by both Ascher (1961:322) and Orme (1974:203), put this concern particularly clearly. M. A. Smith argued that if the full extent of ethnographic diversity is recognized, it becomes inescapable that "between the human activities we should like to know about and their visible results there is logically no necessary link". She concluded that, consequently, it is "a hopeless task to try to get from what remains to the activities by argument" (1955:6, as quoted by Orme 1974:203). With this sort of skeptical criticism, worries about notoriously bad uses of analogy, specifically those due to the evolutionists, are generalized so that all uses of analogy, analogical inferences considered as a class, are called into question.

Three Responses to the New Skepticism

Though skeptical doubts of this sort were increasingly influential, they did not deter Clark and many of his other contemporaries in their efforts to develop methodological strategies for improving the interpretive arguments by which archaeologists typically get from "what remains" to "past activities." In fact, these doubts seem to have called forth the series of further proposals for refining and substantiating analogical inferences that were mentioned above as the immediate precursors to Ascher's synthesis. One is a direct extension of those already described; it calls for a further restriction of the interpretive principles governing appeals to analogy. The other two are recommendations for improving these inferences by improving the source material on which they draw (i.e., the repertoire of analogs) and by testing them against the surviving evidence of their prehistoric subjects.

A Further Restriction of Principles for Selecting New Analogs

As an example of the first of these strategies, consider Hawkes's recommendations for limiting interpretive inferences. Hawkes is deeply mistrustful of interpretation based on "ideas of anthropological 'process' or of ecological determination" (1954:160). He recommends that, so far as is possible, archaeologists should base their reconstructive hypotheses on "historical" and quasi-historical "modes of cognition"; they should always seek "some point of reference within the historical order" (1954:160), as a source not of direct historic analogs but of documentary evidence on archaeological subjects. When they deal with truly prehistoric periods and cultures that lie beyond even the most extended historical "diffusion sphere," Hawkes proposes that reliance on anthropological

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and ecological principles of interpretation be tempered by the recognition that, as you move away from the reconstruction of strictly technical or technically determined realms, the reliability of the interpretive inferences drops dramatically. The underlying insight here seems to be that the more autonomous an aspect of culture is of determinate physical, natural constraints—"the more specifically human are men's activities" (Hawkes 1954:162)—the greater the scope is for a "diversity of cultural expressions," to use Clark's terms. Consequently, as Hawkes puts it, "the more human, the less intelligible" or, less aphoristically, the more distinctively cultural the interpretive subject, the "harder [it is] to infer" (1954:162) without the benefit of textual documentation in which the specifically human, intentional component of past forms of life are (more or less) directly revealed.

In making these proposals Hawkes seems to take seriously the lesson that Clark draws from his consideration of ethnographic variability, namely, that cultures at similar techno-economic levels will not necessarily conform in other respects to any set pattern of response or "cultural expression." Though, in Ascher's estimation, this amounts to the admission that "the new analogy is ineffectual in important areas" (1961:321)—Ascher cites Hawkes as a critic of analogy—it is striking that Hawkes did not conclude, with Smith, that the interpretive reconstruction of prehistoric cultures is entirely hopeless or, more to the point, undifferentiated in its insecurity. His response is the constructive one of attempting to determine just how far formal, material analogies can reliably carry interpretive inference given an appreciation that different aspects of culture are liable to different degrees of divergent variability. Rather than reject Clark's "comparative method" outright or assume comprehensive similarity where the environment and material culture (the products of technological activities and knowledge) are concerned, he treats "natural science"-based reconstruction of technology as, itself, the primary and most reliable form of inference available to archaeologists. Ethnographic data can be used to postulate, with decreasing reliability, the subsistence patterns or economic system, and the sociopolitical and spiritual–religious institutions that may have been associated with this technology in the prehistoric subject. Rather than simply seeking analogs in cultures that existed under similar conditions, Hawkes' "ladder of inference" schema suggests that analogical inference can and should be discriminating with respect to the aspects of past cultures that are inferred on the basis of material (and perhaps technological and environmental) similarities. With this, I suggest, he has further, and significantly, restricted dependence on an evolutionist rationale for inference as this persists in Clark's proposals.

Test Controls

A quite different methodological response to the problem of controlling analogical inference is associated with the "Direct Historic Approach" advocated by

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American archaeologists in the 1930s and 1940s. The proponents of this approach recommended that archaeologists should work progressively back from the historically, ethnographically known to the unknown, using what seems a combination of Folk-Culture analogy and Hawkes' "tele-historic" mode of historical cognition, to construct a "sequence of roughly sequential [antecedent] epochs" (Strong 1942:393; also described in these terms by Steward 1942:337). Like Hawkes and, to some extent, Clark, however, they recognized that even historical continuity in the same environmental context does not guarantee the similarity of prehistoric and historic or ethnographic "cultural expressions". But, unlike Clark and Hawkes, and in striking anticipation of the New Archaeology, they very explicitly insisted that such reconstructions should not be accepted solely on the basis of the plausibility of the interpretive arguments used to generate them, however controlled and restricted these might be. They insisted that these reconstructions be systematically tested archaeologically. Strong, for example, argued that "archaeological research can correct as well as confirm hypotheses derived from ethnological data" (1936:363) and he was able, in his own research, to demonstrate just how effective a tool it can be in exposing errors of interpretation due to mistaken analogical assumptions. He and Wedel were able to repudiate decisively the widely held assumption that the nomadic, hunting way of life encountered in the American Plains at the time of contact must represent prehistoric adaptations as the only means of subsistence possible in this arid environment. They established archaeologically that the "environmental limitations of the [plains] are not so drastic as have often been believed" (Strong 1935:300, see also Wedel 1938) and had, in fact, supported sedentary horticulturalist adaptations in prehistory.

Set against Clark's discussions of Folk-Culture interpretations, Wedel and Strong's work dramatically confirms the worry that even where the prehistoric culture of an area seems represented in directly descended and apparently traditional Folk-Cultures, and even when the environmental constraints are more or less constant, the "economic history" of the encompassing cultural tradition may be profoundly discontinuous. The case is not just a cautionary tale, however, since it also demonstrates how effective a test the archaeological record can provide of analogical hypotheses and their assumptions about the similarities between particular past and present contexts. In this it vindicates Clark's persistent optimism that analogical inference can be systematically evaluated and strengthened despite the fact that "all analogies are very approximate and to a large extent subjective" (1953b:241). He himself sometimes suggests, in fact, that this optimism is predicated on faith in archaeological testing as a means of controlling—exposing error in and selecting among—analogies. He argues, in this connection, that the "main function [of ethnographic comparisons] is precisely to stimulate and give direction to prehistoric research" (1953a:355), and that while these comparisons can "prompt the right questions . . . only archae-

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ology in conjunction with the various natural sciences on which prehistorians freely draw can give the right answers (1953a:355). Thus, he too seems to affirm the view that analogical inference must be reinforced, not only by introducing. on the hypothesis-formulation side, improved and restricted "boundary conditions for the choice of suitable analogs" (Ascher 1961:319), but also by systematically checking the resulting interpretive hypotheses against the surviving material record of the prehistoric subject of interpretation.

Ethnographic and Ethnoarchaeological Controls

By the time Ascher published his synthesis of postevolutionist (classical evolutionist) treatments of analogy, a third and final complementary strategy for reinforcing both "new" and "historical" modes of analogical interpretation had begun to emerge. This was a strategy of improving the standing of analogical interpretation by improving the stock of analogs available as a basis for interpretation and it was, at least implicitly, a response to the practical objection that analogical inference was frequently weakened by a "paucity of ethnographic studies in areas relevant to archaeology" (Orme 1974:205). Ascher himself provides its fullest explication; he proposes, in the second two of his proposals for improving analogy, that archaeologists should make fuller, more systematic use of the existing ethnographic literature and, most important, that they themselves should undertake to fill the gaps in this literature. In particular, he urged them to develop their own ethnographic studies of the "process[es] of continuous change" by which living communities create, use, recycle and discard material things that thereby gradually "becom[e] . . . archaeological data" (1961:324). It is this latter sort of study, he insists, that "holds the most fruitful promise for analogy in archaeological interpretation" (1961:324). Thus, he seemed to feel that however carefully archaeologists might control their selection of analogs (that is, however closely they might restrict their interpretive principles), any significant improvement in analogical inference would ultimately depend on improvements in the background knowledge on which interpretation is based, where this knowledge is both the source of analogs and the ground for selective judgments of relevance.

Similar proposals for upgrading archaeological interpretation by improving its sources had been made 3 years earlier by Kleindeinst and Watson (1956), in extension of Taylor's "conjunctive approach," and research along these lines had been carried out by Thompson in his study of Yucatecan pottery production (1958). While Kleindeinst and Watson are cited by Ascher as proponents of ethnoarchaeological research who share his enthusiasm for its potential to improve archaeological research, Thompson's ethnoarchaeological interests are not discussed at all. He receives mention in Ascher's analysis only as a critic of analogy who insists that archaeological interpretation is irrevocably subjective but no connection is made between these conclusions and Thompson's research

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interests. This is an interesting omission inasmuch as Thompson's reason for undertaking his Yucatecan ceramics study was explicitly to "contribute to our understanding of the processes, limitations and potentialities of inference in archaeological research" (1958:30); his pessimistic conclusions concerning "limitations and potentialities" of inference grew directly out of this early involvement in ethnoarchaeological research. By the time Ascher was writing, then, it was at least controversial whether improvements to the sources of analogical inferences would, in fact, raise the quality of archaeological interpretation as he maintained. Ascher's general discussion does, however, suggest a way of responding to Thompson and it is a response that has significance beyond providing support for his own position inasmuch as Thompson has been an important impetus for more recent reactions against analogy. In what follows, Thompson's arguments for subjectivism are examined in some detail as a basis for drawing out what I take to be the really innovative and compelling, though largely implicit, defense of analogy afforded by Ascher's synthesis.

The Subjectivist Challenge

Thompson's Model of Research Practice

On Thompson's account, archaeological research consists of an "indicative" phase in which interpretive conclusions are formulated, and of a "probative" phase where an effort is made to substantiate these conclusions. In the initial phase, investigators exercise their (trained) intuition to identify what Thompson calls "indications" that their data was associated with "a particular range of sociocultural behavior" (1956:329). An effort is then made, in the probative phase of the inferential process, to display and justify these explicitly subjective inferences and the resulting hypotheses about the cultural, evidential significance of the data. It is in this evaluative stage that analogical inference plays a role. Thompson argues that interpretive hypotheses are substantiated if it can be shown that the proposed correlation between archaeological (i.e., material) and behavioral phenomena has an analog in some known ethnographic context; then the "indicated conclusions" can be considered at least plausible.

Though Thompson believes the probative testing phase of inference to be an important check on the intuitive process of hypothesis formation, he is adamant that it is ultimately and irrevocably subjective; this is why his position is treated as a profound criticism of analogy by Ascher. For one thing, the probative use of analogy depends on the possibility of subsuming material and behavioral correlates under abstracted type descriptions so that the terms of the interpretive hypothesis can be compared with ethnographic information. Whether or not an ethnographic counterpart is found for the archaeologically-indicated correlation thus depends very heavily on how these type descriptions are framed and this,

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Thompson argues, is itself a matter of subjective interpretation. Consequently, he concludes that "the archaeologist injects a subjective element into his inferential reconstruction at least twice" (1956:331): once in formulating the original interpretive hypothesis in the indicative phase of research and then, again, in the probative phase when ethnographic analogs are sought that will vindicate (or, render plausible) this reconstruction. The result is that, on Thompson's account, archaeological interpretation inevitably involves an unsecurable intuitive leap.

Given this understanding of the process of archaeological inference, Thompson draws his well known subjectivist conclusions that, in the end, the credibility of interpretive, essentially analogical, reconstructions of the past depends on the professional competence of the individuals whose subjective intuitions find them "indicated" by the archaeological record. Though he agrees that current methods of assessing competence in terms of intellectual honesty and ability are "certainly inadequate," he maintains that "there does not seem to be any practical means of greatly improving the situation despite the insistence of many of the critics of archaeological methods" (1956:332). All he can recommend is that closer attention be paid to the education of archaeologists so as to ensure that, in honest exercise of their subjective intuition, they have the benefit of "a rich background in anthropological theory and fact and a reasonable amount of familiarity with archaeological materials" (1956:320).

By sharp contrast to Ascher, then, Thompson suggests that improvement in the source material available to archaeologists promises only to put them in a position to intuitively grasp (i.e., to find "indicated") and to justify (i.e., to find anthropologically plausible) a wider range of possible interpretations of their data. Far from eliminating the insecurity of interpretive inference, expanding the repertoire of interpretive options would seem to make the intuitive leap from data to a particular interpretive conclusion more, rather than less arbitrary. If the choice is delimited at all, it would have to be by manipulating definitions on Thompson's account; the typologies used to set up the analogical comparisons that warrant interpretive inferences would have to be designed so as to admit only a narrow range of "behavior:material" correlations, despite broadening the ethnographic data base. So long as the definition of these typologies is subjective, improvement either in the sources of interpretive hypotheses or in the use of these sources (as by education) will not significantly improve the status of (analogocal) interpretation. Thus, insofar as Thompson is one who has, in Ascher's words, "abandoned hope of making any impartial judgment of the reasonableness of an archaeological interpretation" (1961:321), he is a critic of analogy who calls into question the very options for upgrading analogy that Ascher thinks most promising.

Certainly, as I've indicated, Ascher does not supplement his recommendations for codifying and expanding the ethnographic source material used by archae-

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ologists with any direct response to Thompson's challenge. He does nothing to show that work on the sources of interpretation will reduce the subjective element Thompson finds inherent in all judgments of interpretive plausibility. And, without such methodological amendment of Thompson's own model of research and practice, Ascher's main proposals—his argument for improving the ethnographic bases of interpretations—do not constitute, in themselves, a rebuttal of Thompson's skeptical challenge.

Ascher on the Insecurity of Interpretation

Ascher does provide a general argument against Thompson's skeptical conclusions in his discussion of the conventional wisdom about choosing analogs that are "close" to their subject either in terms of "fit" or by virtue of historical connection. He characterizes these directives not as principles that will raise interpretive inferences to a level of certainty, eradicating all possibility of error, but as principles for systematically reducing the field of alternative interpretive solutions until a "best" solution emerges. Analogical conclusions must then be accepted tentatively, as warranted or plausible relative to the alternatives considered; they are, properly, "solutions to [an interpretive] problem [and] are at best approximations arrived at by the elimination of those least likely" (1961:323). Ascher applies this moral to Thompson's skeptical conclusions, insisting that,

> If a systematic approach were used . . . and the alternative solutions for a particular situation stated instead of the usual statement of a single solution . . . there would be no need to examine credentials . . . but only the argument and the result. There is no touch of alchemy in the procedure outlined. (1961:323)

When Ascher's advocacy of ethnoarchaeology is viewed in light of these proposals, it would seem that he expects the expansion and refinement of background (ethnographic) knowledge to provide, not just a wider range of interpretive options but also the grounds for systematically weighing these options. In the first place, the statement cited above suggests, contra Thompson, that even if residual uncertainty is revealed when the considerations leading to an interpretive hypothesis are made explicit, it does not follow that interpretation reduces completely to subjective intuition. Ascher insists here that it will always be possible to express this uncertainty as a reasoned assessment of options against specifiable background information. Presumably one advantage of this is that it would make clear precisely what additional information, either archaeological or ethnographic, is needed to reduce the uncertainty. In the second place, Ascher's discussion of ethnoarchaeological research suggests that, in his view, there are fairly determinate facts of the matter to be established about the range of behavioral and other processes capable of producing various types of archaeological remains. Thus, there may be some flexibility in how these pro-

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cesses (or their variability) can be characterized in correlating them with archaeological types of material, but the choice of organizational or typological categories is not, for all that, purely arbitrary and subjective as supposed by Thompson. These choices are empirically constrained and can, again, be made as reasoned assessments of typological options in light of the available evidence (and, perhaps, in consideration of the interpretive problems in question). This is, at least, a response to Thompson's crucial argument about the subjectivity of analogical inference and the interpretive use of ethnographic data that Ascher could have given, consistent with his general arguments against Thompson's subjectivism, and certainly something like this seems necessary to complete his optimistic argument for upgrading analogy by improving its ethnographic sources.

It is important to note that, to defend the possibility of improving the epistemic status of analogy, Ascher need not claim that Thompson's "subjective element" can be completely expurgated at all the levels of inquiry where Thompson finds it in evidence. The strength of his defense of analogy depends on the implicit critical argument that the general skepticism affirmed by critics like Thompson and Smith does not follow from their various demonstrations that analogical inference is unavoidably insecure.

This argument turns on acceptance of the fact that, as the critics of analogy stress, analogical inferences are all, by definition, "ampliative"; they inevitably claim the existence of more extensive similarities in their conclusions than has been or could be established in their premises, thus, they are always liable to be in error. What Ascher resists is the assumption, made by critics like Smith and Thompson, that where a genre of interpretive inference inevitably falls below the level of logical certainty—where any given inference may be in error—all such inferences must be condemned as equally and indiscriminately at risk of error. All that follows from the insecurity of analogical inference, on Ascher's argument, is that analogical conclusions must be treated as tentative and must consciously be held open to revision as archaeologists expand and refine the background knowledge (and archaeological evidence) on which they are based. This is a response to the "chronic ambiguity [suffered by thinking about] analogy since the nadir of classical evolutionary simplicity" (1961:322) that trades on an appreciation that archaeologists can and routinely do discriminate between more and less well-supported, credible, interpretive arguments. On my reconstruction of Ascher's position, then, its central tenet is that archaeologists should give up the paralyzing demand for certainty and make fuller, more systematic use of the means available for assessing the relative strength and cogency of analogical arguments. The point and value of such arguments is, after all, precisely that they are a means of using background knowledge about more accessible, familiar contexts to reach beyond the archaeological record and provide an account of how, under what conditions, and for what purposes the surviving materials might have been generated.

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Summary

What has been described here is a series of arguments and counterarguments through which the "ambivalence" about analogy noted by recent commentators took definite shape; it reflects an increasingly acute concern that analogy seems to be both indispensible to interpretation and always potentially misleading. At a more fundamental level, these debates can be seen to express a fundamental dilemma that archaeologists confront whenever they seriously undertake to use their data as evidence of the cultural past, namely, that any such broadening of the horizons of inquiry seems to be accomplished only at the cost of compromising actual or potential methodological rigor. Put more generally, with considerations of rigor weighing fully as heavily as interpretivé ambitions, this constitutes the dilemma described by DeBoer and Lathrap as "the familiar quandary of choosing between a significant pursuit based on a faulty method or one which is methodologically sound but trivial in purpose" (1979:103; see also Klejn 1977:6–11). Where analogy is the specific "method" in question, the difficulty is that if it is as profoundly unreliable as its critics insist and if, as many believe, "the 'dead' materials of the past are always interpreted explicitly or implicitly on the basis of [analogical appeals to] the 'living' materials of the present" (Watson 1982:445), then the archaeologist faces a situation in which:

> Either he [or she] becomes a practitioner of an overextended uniformitarianism in which past cultural behavior is 'read' from our knowledge of present cultural behavior, or he [or she] must eschew his [or her] commitment to understanding behavior altogether and engage in a kind of 'artifact physics' in which the form and distribution of behavioral by-products are measured in a behavioral vacuum. (DeBoer and Lathrap 1979:103)

I suggest that each of the critical reactions against analogy and each of the ameliorating responses described here represents an attempt to come to grips with this dilemma. Each either endorses one of the methodological options it defines, accepting that research is unavoidably limited or unavoidably speculative, or rejects these options and attempts to show how one or another of the premises yielding the dilemma may be amended and the dilemma itself escaped. During the period of "chronic ambivalence" described above, those who adopted this last position generally attacked the premise that analogy is radically insecure. Subsequently, this premise was accepted and, indeed, extended; it was then the assumption that analogy is necessary to the nontrivial end of using archaeological data to understand the past that came under attack.

Let me briefly characterize the positions discussed in terms of their response to this dilemma. For a start, Kluckhohn's analysis documents what is, in effect, the impact on anthropology of a first widespread appreciation of this dilemma. Criticisms of evolutionist theorizing were understood to affirm the major premise of the dilemma—they made it clear that an essential component of such theoriz-

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ing, analogical inference, is inherently insecure—and the anthropological community, accepting the terms of the dilemma this posed, felt compelled to embrace its "trivial but safe" horn, avoiding any form of interpretive inference beyond the archaeologically given data that might risk error or speculation. Where Kluckhohn's diatribe against "narrow empiricism" established that this risk-minimizing strategy is not only undesirable but untenable, he forced archaeologists to reconsider the dilemma and the options for practice it posed. Those who were not prepared to embrace the speculative horn of the dilemma undertook to demonstrate that analogical inference could be fortified against the notorious failings of evolutionist interpretation; Clark proposed a restriction of the interpretive principles that had governed the selection of analogs in classical evolutionist contexts. This restriction was supplemented and extended by further restrictions of interpretive principles (Hawkes's strategy), and by strategies for improving the background knowledge on which interpretations are based (Ascher; Kleindeinst and Watson) and for checking specific postulates of similarity archaeologically (Wedel and Strong). In all cases, these developments were informed by the conviction that significant ends can be pursued without embracing a wholly faulty method; they implicitly challenge the skeptic's dilemma by challenging its major premise concerning the limitations of analogical inference.

When critics like Thompson and Smith object that none of the refinements proposed insulates analogical inference against error, they effectively reaffirm this major premise and the intransigence of the dilemma that it poses for archaeology; the significance of their objections is that they thus reaffirm that archaeologists must, after all, resign themselves to accepting one or the other of the dilemmatic options described above. The choice of options that these critics recommend depends on whether or not they follow Kluckhohn in accepting the second premise presupposed by the dilemma, the premise that analogy is indispensible. Smith, as described by Orme (1974) and Ascher (1961), seems inclined to reject this premise and to urge the first option, namely, abandonment of interpretive ambitions as unrealizable. By contrast, Thompson insists that analogical inference and its associated subjectivity is an unavoidable feature of all archaeological injury; thus, he recommends the second option. In his view, archaeologists should recognize that they can never establish an objective, secure understanding of the past, and simply accept the conventional (and in this sense, arbitrary or speculative) nature of their methods.

Read in terms of this struggle over dilemmic options and their presuppostions, Ascher's response to the new skeptical criticisms constitutes an interesting and important attempt to resuscitate the strategy of resisting the dilemma by calling its assumptions about the nature (the radical unreliability) of analogical inference into question. He insists that, even though analogical forms of inference will always remain flawed in the sense that, by definition, they never establish their conclusions with certainty, they are not so radically faulty as to preclude any

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differentiation of relatively credible or strong interpretive inferences from those that are patently (or relatively) speculative and implausible. The import of this is, of course, that it holds out the promise of methodological options that escape the skeptic's dilemma.

The insight central to Ascher's rebuttals of critics like Thompson and Smith—the reason for thinking that they may have underrated or overlooked some important methodological options—seems to be that assessments of the relative credibility of analogical arguments are routinely made and, moreover, that they are systematic and informed, and can be significantly refined by upgrading the methodology and the background knowledge on which they are based. These sorts of assessments and the potential for improving them are, in fact, quite directly illustrated by the cases that have been discussed here. It seems clear, for example, that Sollas's interpretations, based as they are on highly speculative interpretive principles, can be readily distinguished from, and eliminated in favor of, interpretations that are formulated under the control of more restricted principles. For example, analogical interpretations based on Clark's revised evolutionist criteria for selection of analogs, like those he himself proposed in interpretation of Star Carr, are indisputably superior in information content and credibility than Sollas's. It also seems clear that Clark's relatively expansive interpretations would have been much strengthened if he had followed Ascher's second proposal and made more systematic use of the ethnographic information he cites. He might, for example, have specified more clearly the range and variability of adaptive responses to tundra environments represented in the ethnographic literature on contemporary hunting populations, as David has since done (1973). Even without expanding the ethnographic data base, his own interpretive conclusions about prehistoric demography, subsistence patterns, and social structure might then have been framed more accurately, with full consideration of the margin of error due to variability in the sources of interpretation and with an appreciation of alternative interpretive options. David's study points out however, that such improved use of available sources has definite limitations; his analysis exposes a number of uncertainties and gaps in crucial aspects of the ethnographic record on which archaeologists had typically relied. This reaffirms the need, articulated by Ascher, for archaeologists to take to the field and develop the specific source materials that they require for interpretive purposes. Ethnographic and ethnoarchaeological research along at least some of the lines recommended by David has since been undertaken and, certainly, Clark's account would have been substantially improved had he had access to its results. Finally, where the improvement of interpretive sources may often expand the range of plausible answers that can be given to interpretive problems, Wedel's and Strong's studies suggest controlled archaeological testing as a method for quite decisively ruling out some intrepretive options in favor of others by exposing points at which crucial assumptions of similarity do not hold.

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It is in consideration of at least some of these concrete strategies for delimiting and controlling the insecurity of analogy that Ascher draws his optimistic conclusions about the possibility of "placing analogy on a firm [or, at least firmer] foundation". Insofar as analogy is ineliminable, certainty in inferences concerning prehistory is clearly unattainable; if certainty is the epistemic standard against which interpretive methodologies and conclusions are measured, the skeptic's dilemma will surely follow. What Ascher suggests is that, despite falling short of this ideal standard, archaeologists can systematically evaluate their interpretive conclusions and establish them as "best solutions" to given interpretive problems. In this, they exploit a genuine and viable alternative to the dilemmic options of either avoiding interpretive extensions beyond the data or embracing and endorsing arbitrary speculation.

THE NEW REACTION AGAINST ANALOGY

In the decade immediately following Ascher's synthesis and extension of proposals for improving the standing of analogy, a uniquely unequivocal reaction against analogy took hold. This reaction was, initially, an outgrowth of the wider New Archaeology reaction against traditional forms of research and their complacent assumption that archaeology is inevitably limited in what it can understand of the past. On one hand it was insisted, in tones reminiscent of Kluckhohn's diatribe against narrow antiquarianism, that archaeologists can and must use their data as anthropological evidence of the cultural past; the New Archaeology demanded a pursuit of "nontrivial" ends, to use the terms of the dilemma described above. On the other hand, however, proponents of this research program decisively rejected the notion that, to achieve significant ends, archaeologists must embrace "faulty" methods or tolerate speculation as an unavoidable evil. In this, the commitment to anthropological objectives was coupled with a "strongly positive" conviction that uncompromising standards of logical and empirical certainty could be realized if only a properly scientific research program were developed.

As this program was characterized in the early programmatic literature, it required an upgrading of research in two crucial and interrelated respects. First, it required improvement in research methodology; research should be designed, it was insisted, as a deliberate test of interpretive or explanatory conclusions about the past phenomena that produced the archaeological record. And second, it required that the substantive assumptions informing inquiry should be made explicit and should, themselves, be subjected to systematic testing (these would consist of the colloquial or intuitive knowledge that Thompson, for example, found to play such a significant role in the formulation of interpretive and explanatory hypotheses). Though Ascher and his predecessors had recommended

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both the testing of hypothetical claims about the past and improvement of the background knowledge on which they are based, these proposals have special import in the context of the New Archaeology. They are not treated as a means of strengthening essentially analogical arguments concerning the past; rather, they are meant to eliminate the dependence on analogies and on any other broadly inductive forms of inference that leave their interpretive or explanatory conclusions uncertain. The expectation is that, insofar as testing might establish conclusive empirical grounds for accepting or rejecting hypothetical claims about the past, the analogical considerations that led to their formulation (and tentative acceptance as at least plausible) could be entirely disregarded.

In its first and less extreme form, the new reaction against analogy is an attempt to eliminate analogical reasoning from the process of hypothesis evaluation on this basis. Likewise, the rationale for the most recent and radical reaction is, in essence, that if archaeological inferences can be based on laws or lawlike propositions that capture genuine uniformities in the systems and processes producing the archaeological record, then there is no need to rely on analogical inference even in the formulation of hypotheses about the prehistoric subject. These hypotheses can be established with logical certainty as the conclusions of deductive interpretive or explanatory arguments. In both cases, analogy is denied a role in crucial, and increasingly extensive, aspects of the research process because it is an unavoidably ampliative, "inductive" form of inference and because its critics are convinced that there are rigorous scientific methods and bodies of knowledge capable of establishing knowledge claims about the past conclusively. Though neither critical argument succeeds in demonstrating that analogical reasoning is dispensible, I argue, both do provide valuable insights about how best to implement the conventional wisdom that systematic testing and the development of secure interpretive and explanatory principles will much improve the standing of archaeologically-based hypotheses about the past.

The Elimination of Analogy from the Context of Verification

The main objection brought against analogical (or, more broadly, inductive) forms of reasoning in the early programmatic statements of the New Archaeology was just that they are liable to error, not that they are categorically misleading, as was claimed by the later, more radical critics to be discussed below. Consequently, analogical reasoning was conceded to be a useful, even indispensible, tool for formulating interpretive and explanatory hypotheses; the issue was whether it could provide grounds for accepting the hypotheses it suggested or indicated as especially plausible. The conviction that analogical considerations should not enter into the evaluation of interpretive "conclusions," a hallmark of the New Archaeology, grew out of its vehement rejection

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of any suggestion, such as was made explicit by Thompson, that the subjective element of archaeological reasoning is ineliminable and that hypotheses about the past must, therefore, be accepted on a sort of faith, namely faith in the integrity and competence of those whose analogical intuitions have led them to propose these conclusions in consideration of the archaeological data. Binford, among other New Archaeologists, was not prepared to accept Ascher's ameliorating response to this subjectivism. In particular, he rejected Ascher's suggestion that analogical arguments might be made a reliable ground for accepting interpretive hypotheses by improving their sources. It was a mistake, he insisted, to assume that by "'placing analogy on a firmer foundation' we could in any way directly increase our knowledge of archaeologically documented societies" (1967:10); no amount of improvement in the knowledge of present contexts could establish the empirical credibility of the claims an interpretive hypothesis makes about the past.

Binford was, of course, confident that there are means of checking analogical hypotheses that are capable of establishing them with a degree of security that had been considered categorically unattainable by Thompson and even by critics of his subjectivism like Ascher. In specific rebuttal of Thompson's position, Binford declared that "the generation of inferences regarding the past should not [and need not] be the end-product of the archaeologist's work" (1968:18); as Wedel and Strong had demonstrated, archaeologists do have the option of treating interpretive conclusions as the starting point of research designed to test them. This presupposes, in Binford's early discussions, that a standard positivist/empiricist distinction can be drawn between "the context of discovery," in which hypotheses are generated by whatever means are effective, and the properly scientific "context of justification" in which their empirical claims are systematically assessed. In his account, it is only once a researcher has made the creative, often analogically-mediated leap from archaeological data to an hypothesis about the past that produced it that the "real work of science" begins (1978:2); the surviving record of this past can be aggressively probed for evidence that will tell for or against the accuracy of the hypothesis in question. Because at this time Binford held that such testing, conceived in the spirit of "a consciously deductive philosophy," could afford independent and conclusive "verification" of hypotheses (1968:18), he was able to argue, contra Ascher, that "the final judgment" of an archaeological hypothesis could and should rest with the assessment of its performance under test (i.e., "with testing through subsidiary hypotheses drawn deductively" [1967:10]). Thus, he proposed that the analogical, inductive considerations leading to the "formulation" of an hypothesis should be eliminated completely from the deductive "context of [its] verification".

The difficulties with this program for dealing with analogy are endemic to the positivist/empiricist philosophy of science that it presupposes. The sharp distinc-

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tion between the considerations and forms of inference controlling theory construction in inductive contexts of discovery and deductive procedures of theory evaluation is not a tenable one, particularly for archaeological inquiry that is committed to investigation of the cultural past. In the first place, it is an unavoidable commonplace that hypothetico-deductive testing is not strictly deductive in structure. Ideally, it is a method of checking hypotheses that makes general claims about the content, behavior, or structure of some given domain of observable phenomena and it proceeds by deriving and checking test implications concerning the individuals comprising this domain. That is, it conceives of testing as a matter of confirmation by instantiation. But insofar as an hypothesis is truly general, or makes any claims about unobserved phenomena, researchers will not have access to all the instances it is meant to cover; therefore, the crucial inferential move from limited test data to the assessment of the hypothesis against these data is necessarily inductive. This is just to make the familiar point that hypothetico-deductive testing, even in its ideal application, does not fully eliminate dependence on the inductive, ampliative forms of inference that are supposedly distinctive of the context of discovery. This point had been made in an archaeological context by Salmon in 1975 and 1976, by Hill in 1972 and, in direct rebuttal of Binford's arguments for excluding analogy from the process of verifying hypotheses, by Sabloff, Beale, and Kurland in 1973.

This general objection aside, however, there is a second problem, also frequently noted in discussion of this "deductive philosophy," that arises specifically when the hypothetico-deductive procedure is used as a model for testing in a field like archaeology. This account of confirmation presumes, in its original formulation, that it is possible to directly confront the hypothesis under test with a subset of the domain it is meant to cover or, under more liberal interpretations, it assumes that there is some body of data (whether the immediate referent of the hypothesis under test or not) that can provide a stable and unproblematic measure of the empirical truth (or accuracy) of an hypothesis. In the archaeological case, however, the interesting hypotheses are precisely those that make claims about past events and processes lying beyond the accessible data. To bear on an hypothesis at all, the observable archaeological record must be interpretively constituted as evidence in light of a particular theoretical framework (often by means of analogical arguments) and it may have very different evidential significance when viewed in light of an alternative explanatory or interpretive theory. Thus, the "facts of the matter" relevant for assessing the hypotheses of concern to anthropological archaeology are by no means stable and immutable givens. This point is made with some force by Hill (1972), and by Hill and Evans (1972), through appeal to the general arguments by which "contextualist" philosophers of science, like Hanson (1958) and Kuhn (1970), have established the "theory-ladenness" of all scientific observation.

Though archaeological evidence may, itself, be an interpretive construct, it

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does not follow that it can be made to fit or support any interpretive theory whatever. What these arguments underscore is the creative, interpretive nature of the hypothesis testing process itself. Far from being an objective confrontation of "ideas" with "facts," it is a complex, thoroughly inductive process of continual adjustment between the theoretical frameworks that allow you to describe or interpret archaeological data as evidence of the cultural past and the facts about that past that they bring into view. As Binford described it in the late 1970s, the situation is one whereby "the scientist must use conceptual tools to evaluate alternative conceptual tools that have been advanced regarding the ways in which the world works" (1978:2; see also 1980). The implication of this, which has been of increasing concern to Binford, is that the testing procedures comprising the "context of verification" simply do not afford a form of "proof" of hypotheses that will allow you to sidestep dependence on inductive forms of inference or to eliminate the uncertainty associated with them. He now argues, in explicitly Kuhnian terms, that it was "an illusion" to think that archaeological research could provide an objective test of theoretical claims about the past, given the extent to which observational experience is "conceptualized" in "paradigmatically subjective" terms (Binford and Sabloff 1982:138): "What neither of us faced squarely at the time [that is, neither Binford nor early New Archaeologists who, like Hill, attempted to institute a testing program] was that we could not use the archaeological record to test the accuracy of meanings assigned to archaeological facts" (Binford 1983:12).

Once the Kuhnian point is taken, Binford argues, it becomes clear that background, "actualistic" research must be undertaken to determine how archaeological material is produced and what its meaning is as a record of the operation of past cultural systems. This effectively reaffirms and gives first priority to the early proposal that the assumptions underlying interpretive arguments should be drawn out and established as (or replaced by) "covering-laws," thus securing their conclusions with deductive certainty. These considerations indicate, then, that even its original proponents have concluded that this first strategy for eliminating dependence on faulty methods of inference like analogy fails, as it stands, to circumvent DeBoer and Lathrap's dilemma.

Alternatives to Reliance on Analogy in Formulating Hypotheses

The second methodological response to this dilemma, which is of a piece with Binford's recent proposals, was originally articulated by Freeman in connection with his more radical view that argument by analogy is not just prone to error but patently misleading. He insisted that analogical inference should be eliminated from all archaeological contexts, particularly those where interpretive hypotheses are formulated, and he argued that "an understanding of the archaeological

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residues'' (or, properly, ''frameworks of theory'' that might lead to such an understanding) could be ''based directly on the comparison of these residues'' (1968:262). He never made it clear, however, just how systematic comparison or analysis of the data could, in itself, transcend the level of a purely descriptive ''artifact physics.'' This is a serious shortcoming inasmuch as it is widely recognized that, as Dell Hymes observed in comment on David Clarke's *Analytical Archaeology,* the ''step or leap from debris to a general theory of what the debris represents . . . is not to be gotten by pressing the analysis of the debris as far as it will go'' (Hymes 1970:19). Certainly, in their objections to the ''inductive'' practices of traditional research, the New Archaeologists made a compelling case against the notion that ''formal comparative study of the remains themselves'' is capable of yielding any interpretive understanding of the ''causes of these remains.'' The threat of reducing inquiry to ''trivial but methodologically safe'' descriptive systematics seems unavoidable if one adopts Freeman's strategy of eliminating analogy in favor of purely empirical comparative analysis.

More recently, Gould has taken up Freeman's cause against analogy and has addressed the residual problem of specifying, more fully, how one might move from ''analysis of the debris'' to an understanding of its cultural antecedents without resorting to analogy. Unlike Freeman, he does not, in the end, recommend that interpretive theory be formulated without any input from our experience and knowledge of contemporary situations. What he resists, specifically, are appeals to this source material that depend on untenable principles of ''generic uniformity''. He argues, in this connection, that archaeologists should ''be concerned with . . . the interconnectedness of things, not merely their correlation'' (Gould and Watson 1982:30), and that they should ground their interpretive arguments in ''general principles'' that ''posit necessary relationships between the various kinds of observed evidence'' (Gould and Watson 1982:30). Though the New Archaeology ''covering-law'' based model of interpretive inference is never directly invoked here, what Gould recommends is, in essence, that interpretive hypotheses be established deductively, as the necessary consequences of secure laws or law like principles.

The sort of principles Gould has in mind are, primarily, laws established in the natural, biological sciences. He observes that ''many principles developed in evolutionary biology and ecology can safely be assumed to have operated uniformly in the past as they do in the present'' (1980:50) and he goes on to argue that insofar as human behavior is subject to these laws, it too conforms to certain uniformitarian principles: ''Do we seriously doubt that because people, along with everything else in nature, are subject to the effects of gravity today, they have been subject to these same effects in the same ways at all times and everywhere in the past?'' (1980:112). Given this, he recommends that archaeologists interpret their data by means of an ''ecological connection''; they should identify the physical, biological ''limiting factors'' that impose invariant con-

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straints on human behavior, isolate the "aspects of human behavior that are most closely related to [them]" (in the sense of being dependent on or determined by them; 1980:50), and then formulate hypotheses about the broad behavioral complexes that must have been instantiated in particular past contexts given the conditions under which the human population lived. The inference from present to past is thus mediated by well established and closely circumscribed—"genuine"—uniformitarian principles (ones that have been firmly established in the natural and biological sciences) and it projects onto the past only those invariant regularities that exist in the biologically, physically constrained dimensions of human behavior.

It seems that Gould's ambition of achieving a nonanalogical mode of understanding—that is, of taking archaeological interpretation decisively "beyond the realm of analogies and into a different order of discourse at the level of general principles" (1980:112)—can be achieved in those rare limiting cases where the reconstructed behavior is, by nature, a direct and exclusive consequence of impinging ecological or material conditions. Here, complete explanatory closure is realized; improvements in the background knowledge supporting interpretive inferences raise them to the level of deductive security. As Gould himself acknowledges, however, the range of behavior constrained in this way is extremely limited. It might include, for example, cases where a particularly restrictive natural environment limits the options for survival (for a population with restricted technological capabilities) to a single response pattern or, more narrowly, where the material comprising a given artifact type admits of only one production technology or sustains evidence of wear that could be produced in this material by only one pattern of use. But whether, or indeed how far, material, ecological factors determine human behavior beyond these limiting cases is an open and contentious question. This is, in essence, the question raised earlier in connection with Clark's directive to seek analogs among ecologically and technologically similar cultures. Many, including Clark, have answered it with the observation that there is tremendous scope for idiosyncratic variability, at a cultural or individual level, in most areas of human response to biophysical limitations. Indeed, there is a strong case to be made that this variability is the distinctively human and cultural feature of the archaeological subject; hence, it should be the special interest of an anthropological archaeology. This, of course, is the position taken by the proponents of symbolic or structuralist archaeology (notably, Ian Hodder 1982, 1983). It is vehemently opposed by Dunnell (1982) who has been prepared to reject, out of hand, the anthropologial preoccupation with cultural or ideational idiosyncracies on the ground that they are not susceptible of properly scientific investigation, and by Binford who draws the same conclusion but on the grounds that these phenomena do not comprise a genuinely autonomous subject of inquiry in any case; they are a function of, hence, are explicable (in scientifically respectable terms) by appeal to adaptive principles operating at a systemic level.

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Given Gould's strong advocacy of an ecological mode of understanding the archaeological subject, it is important to note that, unlike Binford and Dunnell, he is, in fact, sympathetic to the point that much human behavior may be semiautonomous of, and therefore inexplicable in terms of, ecological constraints. He insists that "human beings are not particles or inanimate entities whose behavior can be explained solely in relation to general laws like those used in the physical sciences" (1980:xi); and he explicitly rejects any attempt to restrict inquiry, for the sake of methodological purity, to just "those aspects of behavior that can be reliably covered by laws" (1980:37). Consistent with this, he even directly qualifies his advocacy of law-mediated interpretation, noting that humans can evolve "traditional skills, knowledge, and technology [that] can all serve to overcome . . . limiting factors" and can, in fact, "act as limiting factors" in themselves (1980:53), that is, can function as the determinants of behavior, independent of environmental or material limiting factors. Indirect reasoning through "ecological connection" is to serve primarily, then, to establish the parameters within which idiosyncratic, highly variable cultural patterns of behavior can emerge; and any fine-grained anthropological understanding of past behavior is recognized to require a supplementary mode of interpretation. Gould claims to supply this in the form of his method of "argument by anomaly."

Consistent with his general methodological position, Gould's "argument by anomaly" is just as sharply contrasted to analogical forms of inference as was his method of "indirect" reasoning by ecological connection yet it is, itself, thoroughly, indeed, almost explicitly analogical. What Gould recommends is that though much human behavior is not, in fact, fully or directly determined by biophysical conditions, it can be treated as significantly "like" the adaptive behavior of nonhuman, biological species in its outcome. That is to say, the behavioral patterns that emerge can very largely be explained as, or "as if" they were, ecologically adaptive in the way that directly conditioned adaptive behavior is explained in biology; they can be explained as one component of a comprehensive strategy that functions to minimize the risks to population survival posed by environmental factors.

Gould states this central interpretive principle explicitly when he observes that "limiting factors operate in the realm of human behavior and produce the same effects as they do upon species in nature" (1980:109), namely, they impose limits on the size of populations that can survive in any given environment such that "even under the most optimal conditions, the behavior of all people, everywhere is constrained by limiting factors of some kind in the past as much as in the present" (180:111). He is, moreover, quite clear on the point that this principle has been imported from biological science (where it is formulated as the "principle of the limit" [1980:52]) on the basis of a comparison—an analogy—drawn between humans and other biological species. Both types of population, he argues, are implicated in a complex net of causal relationships that ensure that, as

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in the case of insecticide poisoning (he cites Carson's *Silent Spring*, 1980:49), they will inevitably be affected by perturbations in other (material, biological) components of their encompassing ecological system no matter how isolated or culturally insulated they may seem to be.

Given this central principle, Gould goes on to elaborate a series of "principles about human adaptation in general" (1980:109) that draw out the implications of this biological limits theory for a species that has unique social and ideational resources to deploy in its accommodation to biophysical constraints. He notes, for example, that the more imposing the risk created by a particular limiting factor, the more extensive the socially mediated response to it will be; he describes general conditions under which technological elaboration, "optimizing behavior," will occur (namely, under conditions of relative freedom from stress; these are Principles 5 and 7, 1980:110). These principles should, he says, provide an explanation of all aspects of human behavior. That is, they should serve as a source of explanatory hypotheses that specify what patterns of behavior would, under given biophysical conditions, be most "rational" from an ecological point of view, hence, are directly explicable (in functional–ecological terms) as "adaptive," that is, as "rational" responses to given limiting conditions. These principles, then, constitute a "baseline" for interpretation of the record and the "method of anomaly" enters as a testing procedure in which archaeological evidence is used to check for areas where actual past behavior deviates from these "eco-utilitarian" expectations.

If the anomalies brought to light by this interpretive procedure persistently resist explanation in ecological terms, then, Gould suggests, an appeal may be made to "ideational" factors. There is, however, very little that Gould does not think will yield, ultimately, to explanation in terms of limiting factors and ecological rationality in adaptation to them. He states that "behavior that might appear maladaptive at one level of interpretation . . . may be viewed as adaptive at another level" (principle 4, 1980:109) and typically, in his analyses, even the most arbitrarily "symbolic" aspects of behavior can be seen to serve some role in articulating the human population with its environment. (See, for example, Gould's discussion of the "disproportionate expenditure of time in hunting game by desert Aborigines" [1980:10] and of the role of "righteous rocks" [1980:141] as anomalous behaviors that indirectly serve ecological ends. In the latter, a case is made for seeing functionally inexplicable behavior as reinforcing social networks that, considered in the long term, provide insurance against the risk of rare but life-threatening local shortages of resources.)

Gould's alternative method of interpretation consists, then, of two components. The first is "indirect reasoning" strictly by ecological connection that allows for law-mediated reconstruction (and explanation) of those aspects of past behaviors that are directly conditioned by biophysical "limiting factors"; this may be nonanalogical in its limiting cases. Where this covers only a narrow

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range of the behaviors of interest to archaeologists (that is, a limited range of the behaviors responsible for the record), Gould proposes a second interpretive strategy—his argument by anomaly—which proceeds by extending the ecological model analogically to cover even those behaviors that seem most independent of ecological conditions. For explanatory purposes these behaviors are to be treated "as if" they were adaptive and serve the biological ends of the population, hence, "as if" they could be explained in functional–ecological terms (i.e., by direct analogy to the adaptive behaviors of nonhuman, biological populations) as behaviors that serve the biological ends of the human population, albeit through the medium of an eco-utilitarian rationality.

In both of its components, it seems unavoidable that Gould's alternative to analogical reasoning is, itself, pervasively analogical. In fact, in the end, it seems that Gould can only claim that his own methods are nonanalogical by fiat of definition; he insists that analogical inference is to be narrowly identified with the kind of uncontrolled, single-source analogical arguments that prompted the reaction against analogy in archaeology (see Wylie 1982). He justifies this by appeal to various dictionary definitions which, he has recently claimed, demonstrate that it would be "stretching the concept of analogy far beyond its logical or commonly accepted meaning" (Gould and Watson 1982:25) to characterize his own law-mediated method of interpretation as analogical. Certainly, however, the standard analyses of logical usage take a much broader view of analogy than Gould claims is correct. This is not just a matter of semantic interest. Insofar as this logical usage does cover many of the inferences Gould considers nonanalogical, it makes clear the extent to which even Gould's interpretive conclusions remain ampliative. This, moreover, emphasizes the need to treat his methodological proposals as directives for assessing and improving the status of these conclusions, not as defining a procedure that, once implemented, will establish them beyond doubt or need for revision. In this, the broader usage establishes an important continuity between Gould's proposals and those put forward in contexts where the role of analogical, inductive reasoning has not been definitionally obscured.

In Definition and Defense of Analogical Argument: Rebuttal to Charges of Radical Insecurity

Analogy in Logical Usage

Typically, when a constructive account is given of the "logic" of analogy, it begins with the observation that, by sharp contrast to Gould's claims, analogical inference consists of the selective transposition of information from source to subject on the basis of a comparison that, fully developed, specifies how the "terms" compared are similar, different, or of unknown likeness. To use the

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terminology introduced by Keynes (1921), and elaborated in important ways by Hesse (1966), these dimensions of comparison establish the "positive," "negative," and "neutral" components of an analogy. It is clear, then, that Gould departs significantly from logical usage when he claims that analogies are just comparisons for similarities: these standard sources, which Binford introduced to the archaeological literature in 1967, consider the premises of an analogical argument to establish a relationship of partial similarity that involves a consideration of differences as well as of similarities. In fact, it is a recurrent theme in the philosophical literature on analogy that it is a "glaring error" (Bunge 1973:130) to claim that analogy is exclusively a relation of similarity. Fischer describes it as a fallacy—the fallacy of "Perfect Analogy"—and insists that arguments that consider only similarities are either arguments based on (or inferring) a relation of identity and are not analogical at all, or are examples of "false analogy" (1970:259). An argument by analogy, proper, involves the claim that, given the similarities and differences specified in the premises, some specific aspects of the neutral analogy may also be assumed to be similar or, to comprise further points of positive analogy (see also Scriven 1976:210–215; Mackie 1972:175). The justification for this assumption (or conclusion) that further similarity exists, hence, the strength of the argument as a whole, depends on the nature of the comparison presented in the premises.

At its simplest, the comparison supporting an analogical inference is a purely formal, point for point assessment of similarities or differences in the properties of source and subject. Interpretive conclusions are drawn, in this case, on the principle that where two objects share some properties, they may be expected to have others in common and they are, at their most simplistic, entirely indiscriminate with respect to what properties may comprise the additional (underdetermined) positive analogy. It is this sort of inference that concerns Gould and Freeman and, as they point out, it is justified only insofar as it can legitimately presuppose a comprehensive principle of uniformity affirming that the patterns of association observed among properties in familiar contexts hold for all contexts. Otherwise the similarities between source and subject may be entirely accidental and may not be indicative of further similarities. Given the contingent nature of such patterns, this assumption is clearly untenable; the danger of relying on it is exemplified by the cases responsible for the original reaction against analogy in which the details of prehistoric forms of life and culture were simply "read off" from contemporary contexts on the basis of a relatively limited comparison for formal similarities between the ethnographic source and the prehistoric subject.

Fortunately, analogical comparisons can and generally do incorporate what the standard logical analyses characterize as considerations of "relevance." Relevance is typically understood to be a function of knowledge about underlying "principles of connection" that structure source and subject and that assure, on

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this basis, the existence of specific further similarities between them. As Copi puts it,

> Although there may be disagreement about what analogies are relevant, that is, what attributes are relevant for proving the presence of certain other attributes in a given instance, it is doubtful that there is any disagreement about the *meaning* of relevance . . . One attribute or circumstance is relevant to another, for purposes of analogical argument, if the first affects the second, that is, if it has a *causal* or determining effect on that other. (1982:400; original emphasis)

Considerations of relevance enter analogical arguments, then, when analogs are compared for the relations that hold among the properties they share rather than for the simple presence or absence of these properties considered independently of one another; that is, analogies that incorporate considerations of relevance are typically "relational" analogies. As Uemov has argued (1970), a number of different sorts of relational comparison are possible. The relations compared may be formal; they may be relations of proportionality. They may be contingent relations of constant conjunction, or they may be more substantive relations of functional–structural or causal–consequential dependence, these last being the sorts of connections on which Copi would base considerations of relevance. At their strongest, relational comparisons involve a demonstration that there are similarities between source and subject with respect to the causal mechanisms, processes, or factors that determine the presence and interrelationships of (at least some of) their manifest properties. This is the sort of analogical reasoning by which established theories are extended to new domains, and it is the primary subject of concern in the philosophy of science literature on analogy (see, in particular, Hesse 1959, 1966).

It is a commonplace in discussions of relational analogy that once the background knowledge of causal or functional connections is fully developed and a "complete theoretical acccunt is available" for the subject domain (Shaw and Ashley 1983:430), analogical inference can be replaced by a theoretical explanation. It is this possibility, it would seem, that has inspired the current, most radical reaction against analogy in archaeological contexts. It is striking, however, that, in Shaw and Ashley's discussion, archaeological inference is identified as precisely the sort of case where analogical inference is not likely to become redundant; they observe that "many useful analogical arguments (e.g., those made by an anthropologist about social functions in a primitive tribe) occur which we are not at all in a position to replace with a full explanation" (1983:431).

This philosophical intuition is not developed in any detail, but read against archaeological discussions of analogy, the literature on the logic of analogy and on considerations of relevance suggest at least two reasons why it should seem plausible. In the first place, however fully established anthropological theory (or

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for that matter, psychological, sociological, or ecological theory) may be, its use in archaeological inference to establish claims about cultural prehistory is always, to some extent, an extension of these theories to new domains. This is a point that has been made not only by the skeptical critics of archaeological inference like Smith, but also by its most optimistic proponents. As described earlier, Clark was exceedingly candid in his appreciation that source and subject contexts cannot be assumed to manifest the same principles of connection, even where they are historically continuous with one another or are subject to the same ecological constraints. This concern reappears, in the recent literature, in the guise of the objections that critics like Hodder have brought against the uniformitarianism of eco-utilitarian approaches. In all cases, the point being made is that past cultural systems may be different enough from those we know in the present that they cannot be considered part of the same domain; they cannot be considered to exemplify the principles of connection embodied in our theoretical knowledge of cultural systems.

The second reason why even theoretically grounded inferences about the cultural past are likely to be irreducibly analogical is that, far from being a potential basis for interpretation, relations of dependence among properties and the causal "dynamics" responsible for them are necessarily among the features of past cultural contexts that archaeologists are concerned to reconstruct inferentially. While this rules out the possibility of establishing any direct "analogy of relations," it does not mean that archaeological inference must rely solely on a purely formal, superficial "analogy of properties" (to use Uemov's terms, 1970:271). Certainly, to follow Copi's more general discussion, a consideration of causal and functional relations as they hold in source contexts will provide an understanding of how, or why, and under what conditions the properties compared across source and subject contexts can be produced or, will co-occur. Even if this does not provide grounds for concluding that the subject must be similar in further, specific ways to known source contexts, it provides the baseline for making a reasoned and informed assessment of the relevance of known similarities to those inferred.

The standard literature on the logic of analogy does acknowledge, then, what might be characterized as a continuum of types and strengths of analogical inference ranging from those that are based simply on a formal comparison for similarities in the presence or absence of properties, through the various types in which a comparison is made for relevant similarities. Where relevance is established, by appeal to principles of connection holding among the properties compared, it is clear that analogical arguments can, and typically do, incorporate precisely the sort of information that Gould took to be distinctive of his indirect reasoning by ecological connection and to assure its nonanalogical status. Thus, it would seem that, contra Gould, standard logical usage does readily stretch to cover the forms of interpretive argument that he recommends.

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Of course, in its limiting case, where the extension of explanatory theory is complete and the causal, functional relations structuring the subject domain are understood in their own right (i.e., not just by analogy to more familiar domains, as in earlier stages of theory development), the continuum of types of analogy may give way to nonanalogical forms of inference, namely, to theoretical explanation. Gould's ambition is, clearly, to bring about this final tranformation of archaeological inference but, however defensible this may be as a regulative ideal, his proposed methods of inference do not realize it; for the reasons given they remain pervasively analogical. The irony is that insofar as Gould succeeds in showing that his methods can raise interpretive inference above the level of untenable direct projections of the present onto the past, he inadvertently demonstrates that analogical inference can escape his own charge that it is radically faulty.

Criteria of Strength in Analogical Argument

There are various ways in which, following standard logical usage, the different types of analogical inference can be systematically strengthened and evaluated. My objective in discussing them is to show that both Gould's proposals for improving archaeological interpretation and those put forward by the proponents of analogy exploit a basic set of strategies for controlling and strengthening the inferences involved as analogical inferences. This will prepare the ground for showing that the second radical objection to analogy—that it is unavoidably distorting of what we can understand of the past—can be decisively turned. (This analysis and the concept of a continuum of types of analogy on which it bears was originally developed in a brief conference paper [Wylie 1980]; this earlier treatment has been adopted by Hodder [1983:16–23].)

Even where analogies are based primarily on a comparison for total similarities, there are a number of criteria that can be used to determine their relative strength. The value and also the pitfalls of these criteria are aptly illustrated by an analogical interpretation of stone gorgets proposed by Curren (1977:97–101; see also Salman's analysis of this case, 1982:60–63). He suggests that these groundstone artifacts may have been pottery making tools and he supports this interpretation by noting that an extensive positive analogy holds between modern potters' tools (or "ribs") and the gorgets, particularly with respect to their shape and edge treatment, all are thin with curved and beveled or serrated edges, and with central perforations. He also takes into consideration the primary negative analogy—that potter's ribs are never stone, the material of which most gorgets are made—and argues that this may not be a significant difference because modern potters use ribs made of a variety of materials, including wood, metal, and bone. Insofar as this initial, very simple analogy is plausible, it is because it is based on a systematic comparison of source and subject that establishes not only a number of similarities between them but also weighs these against the

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differences so as to determine the overall extent of the similarities. These constitute the two primary criteria for evaluating a formal analogy and both have to do with the fit of source (or analog) to the subject of interpretation.

A third quite different criterion is exemplified when Curren turns the observation of dissimilarities to his advantage by showing that the correlation between morphological and functional attributes—between the known and inferred similarities—holds consistently across a wide range of source contexts despite variability in the materials of which the tools are made or in the type of ceramic production involved. Here, rather than elaborate on the comparison of source and subject, he expands the bases for comparison; the argument is strengthened because systematic examination of a diversity of possible sources shows that they all support the proposed interpretation. A final, encompassing criterion is that an analogy is strengthened to the extent that the breadth and specificity of the similarities established in the premises outweighs that of the additional similarities claimed in the conclusions. The standard criteria for evaluating what I have described as formal analogies are, then, number and extent of similarities between source and subject, number and diversity of sources cited in the premises in which known and inferred similarities co-occur as postulated for the subject, and, finally, expansiveness of the conclusions relative to the premises.

With the treatment of dissimilarity, it is evident that Curren has begun to move beyond a narrowly formal comparison and to introduce what amount to premiliminary considerations of relevance. In fact, I would argue that the criteria listed are a good measure of the strength of interpretive arguments precisely because they direct attention to the sort of formal comparability or association among attributes we consider good initial evidence of an underlying principle of connection that would support direct claims of relevance if confirmed. Viewed in this light, these criteria can be seen to yield two quite distinct strategies for strengthening an analogy that turn on the provision of two different sorts of indirect evidence that a binding relation holds between the compared and inferred attributes. The first strategy is to broaden the base for interpretation as Curren does; the invariant association of formal with functional attributes that he documents provides at least good preliminary grounds for supposing that the formal attributes in question are deliberately created or selected for because they meet the functional requirements of ceramic production and, consequently, that a binding quasi-causal relation of dependence holds between the (formal) attributes he finds shared by source and subject, and the (functional) attributes he infers on this basis. If the implied principle of connection holds, that is, if the configuration of formal attributes shared by source and subject is functionally determined, then patterning at a formal level is to be expected in the examined source contexts and may be treated as a "relevant" indicator (in Copi's sense) that the subject contexts are like them in the further, functional respects postulated by Curren's interpretive argument.

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When pressed, comparisons for the extensiveness of similarity comprise a parallel second strategy. To cite a different example, Hill defends the initial plausibility of the hypothesis that prehistoric pueblo room types served the same functions as their formal analogs in contemporary pueblos on the ground that "the similarities between the suspected analogs is so great that they almost cannot be coincidental" (1966:15). That is to say, the closeness of mapping, or of fit, between source and subject is so complete it seems to indicate that they are structured by the same causal or quasi-causal principle of connection; it suggests that a relational analogy may underly the formal analogy. In this case, if the principle were made explicit, it would seem to be one of determination of the form of the rooms by a collective intention to use them in particular ways that both informed their original construction and determined what activities they would actually house once constructed. Again, if the principle were directly demonstrated to hold for the source contexts, it would provide grounds for arguing the specific relevance of the formal similarities cited to the inference of further similarities of use or function.

The formal criteria can be deployed, then, in such a way that they serve as surrogates for direct, relevance-establishing (or relevance-measuring) appeals to relational considerations. This yields various transitional forms of analogical argument that lie between those that make claims about unknown features of a subject on the basis of purely formal comparisons with better-known sources and those that depend on developed theoretical knowledge about the principles of causal, functional dependence that structure the source, and perhaps the subject. In their most fully developed form, these latter arguments of relevance give way to direct demonstrations that a relational analogy holds and the inference of further formal or behavioral similarities approximates the ideal of a deductively structured explanation. Even at the lower end of the continuum, however, where formal criteria prevail and considerations of relevance remain implicit, the worst-case instances of direct projection of present onto past that initiated the reaction against analogy can be decisively ruled out as implausible. In the interpretations offered by classical evolutionists, for example, formal comparison across contemporary "primitive" source contexts and between these sources and prehistoric subject cultures is radically unsystematic; dissimilarities between sources and subjects are rarely considered, much less weighted against the similarities; and, though a wide range of sources are cited, there is no demonstration that specific configurations of attributes are invariant across them. The fragmentary similarities established in the premises fail, then, to provide grounds even for postulating an underlying relational analogy, let alone for assuming, as the classical evolutionists tended to do, the literal identity of the causal dynamics of relations of interdependence structuring prehistoric "primitive" cultures and their contemporary analogs. As indicated earlier, their conclusions claim similarities between these sources and subjects that so far overreach what the prem-

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ises establish, the arguments risk exemplifying Fischer's "fallacy" of simplistic analogy.

Clark's response to these excesses represents a significant advance inasmuch as he calls for at least minimally systematic use of formal comparison to establish the plausibility of the principles of connection on which these arguments implicitly depend, and he explicitly acknowledges the limits and tenuousness of these principles. His own interpretation of Star Carr is interesting, in this connection, because it illustrates how the two strategies described above can be used together to introduce and then establish the plausibility of ecological principles of connection. He proceeds, not just by citing a range of source contexts that are similar with regard to to their environment, their technology, and the resources exploited, but by observing that these variables are consistently associated, in these sources, with distinctive features of community size, mobility, division of labor, and internal social organization. In this he makes effective use of Curren's strategy, expanding the bases for interpretation to include a number of sources that exhibit an association of the inferred properties with those compared across source and subject. The sociocultural features of the prehistoric Star Carr community are then inferred by analogy to this range of sources on the grounds both that there are strikingly extensive similarities between them with regard to the former, techno-ecological variables and that, as in Hill's interpretation, the fit of source to subject is so extensive it seems it cannot be accidental. To recapitulate, insofar as Clark's sources do provide grounds for these conclusions, it is because analysis of them suggests that the inferred sociocultural variables are dependent on (are controlled or conditioned by) techno-ecological circumstances, hence, that cultural contexts like the sources examined are structured by a principle of techno-ecological connection. And, insofar as the argument as a whole is compelling, it is because the striking fit of sources to subject suggests that the Star Carr community may have been determined in its organization and demographic aspects by material limiting factors in the same way that its contemporary analogs are said to be. Taken together, the development of formal comparisons in these two interlocking ways suggests that a further relational analogy may hold that would guarantee the relevance, in Copi's sense, of known to inferred similarities if established.

Source and Subject-Side Strategies for Establishing Relevance

The weakness of Clark's ecologically based interpretation and also of the Folk-Cultural analogies he recommends is that they provide no direct evidence or test of the crucial presumption that a causal connection holds between the variables found associated in source contexts and imputed to the subject of inquiry. Thus, as the critics of analogy quickly pointed out, these interpretations depend on general assumptions about the uniformity of human response that are, at best,

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unsubstantiated and, at worst, highly implausible given the numerous counterexamples Clark himself acknowledges (i.e., cases that testify to the plasticity of human response to given environmental conditions and to the potential for sharp divergence or transformation of this response even within a continuous historical tradition). In short, interpretative arguments governed by Clark's neo-evolutionist criteria for selecting analogs are inherently incomplete; they represent as refined a use of formal analogy as is possible but, as such, they trade on what remains merely an intimation of relevance.

To move beyond this limited use of analogy and develop relational grounds for interpretation, archaeologists must work aggressively on both sides of the analogical "equation" (to paraphrase Watson 1979:281) and, most important, they must work specifically to establish the principles of connection—the considerations of relevance—that inform the selection and evaluation of analogies. That is to say, the two strategies developed for strengthening formal analogy—the strategies of expanding the base of interpretation and elaborating the fit between source and subject—must be treated as directives for the active investigation of sources and subjects rather than as criteria for assessing analogical conclusions reflectively, after they are formulated. And the inquiry they initiate must be specifically designed to determine what causal connections hold between the material and cultural or behavioral variables of interest, and under what conditions these connections may be expected to hold. The first point was appreciated early on in the context of attempts to build on Clark's neo-evolutionist proposals; Wedel and Strong demonstrated the value of subject-side testing as a means of checking source-based suppositions of plausibility (i.e., as a means of determining empirically the applicability of particular analogs to particular subjects), and Ascher established that archaeologists would have to undertake their own studies of source contexts if they were to be assured of background information relevant to their interpretive needs. But even though, as suggested earlier, the standing of particular interpretive hypotheses might be significantly improved by following these proposals (e.g., as above, Clark's interpetive claims would certainly be strengthened by more systematic use and development of the source material and by directed archaeological testing), the improvements realized will remain limited so long as supplementary research is designed primarily to strengthen the formal grounds for interpretation and not to substantiate the principles of connection that interpretive hypotheses and archaeological tests of these hypotheses necessarily presuppose.

The need to test suppositions of casual connection directly in source and subject contexts—the second, crucial requirement for making analogical interpretation fully relational—only became an explicit concern when New Archaeologists saw this as a means of eliminating dependence on analogy altogether. Their primary concern, in practice at least, was to institute a program of subject-side testing; source-side work was not so much the focus of meth-

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odological development despite a strong programmatic commitment to articulate and test the interpretive principles guiding interpretation. At its most effective, however, New Archaeology testing did involve at least preliminary specification and defense of the principles of connection presumed to hold between shared and inferred attributes (albeit often on the basis of impressionistic knowledge of the sources cited) and tests were designed, in light of this, to determine whether or not such connections could have held (or did hold) in the subject of interpretation. Thus, New Archaeology test procedures, as exemplified by Hill's tests of the functional interpretation discussed above, do illustrate how effectively subject-side work can reinforce and control intimations of plausibility, introducing direct considerations of relevance as a ground, beyond formal comparisons, for holding specific analogical conclusions.

Consider, briefly, how Hill exploits both source and subject-side considerations in developing a case for his interpretation of pueblo room function. In the first instance, Hill argues that the formal features that characterize archaeologically defined room types are precisely what the functions he attributes to them would require of any room that was specifically intended to serve them. For example, general "living" and food preparation activities, in contrast to storage functions, require just the features assuring light, ventilation, and relatively greater space, and just the special facilities for food preparation that are, in fact, associated with the prehistoric pueblo "living rooms." Thus, on the basis of conceptual argument and an appeal to colloquial knowledge about the material conditions necessary for certain sorts of activities, Hill claims at least minimal plausibility for assuming the existence of a causal connection between form and function; he shows that the formal features of pueblo rooms co-occur in distinctive configurations because the rooms were (nonaccidentally) created, indeed, were perhaps explicitly intended to house the sets of activities that comprise the functions distinctive of the various room types. This, however, is just a preliminary; what really strengthens Hill's account are his subsequent empirical tests for evidence that should (or could not) be present in the record if the prehistoric rooms had, in fact, been used for the purposes ascribed to them. Contrary to Hill's claims, I would argue that the confirming outcomes of his tests do not establish nonanalogical grounds for his interpretive hypotheses. Rather, they dramatically improve the credibility of these hypotheses by expanding the range of the positive analogy holding between source and subject in areas where similarities could not have been expected (or, at least, were very unlikely) unless the hypothesis under test were approximately true and, more important, unless the underlying quasi-causal principle of connection that it postulates (i.e., the intended and actual use of the rooms) actually held in the past and was responsible for the form and features of the prehistoric rooms as it is said to do in better known source contexts.

The efficacy of Hill's testing strategy and the capacity of such tests to de-

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cisively disprove as well as support analogical hypotheses is dramatically illustrated by comparison with Curren's interpretive argument. He rests the case for his interpretive conclusions entirely on source-side arguments for the plausibility of the assumption that a quasi-causal relation (similar to that defended by Hill) holds between a ceramic-making function and the features of form shared by stone gorgets and potters' ribs. These are, in themselves, quite compelling arguments; Curren goes well beyond Hill in showing that the actual use of ribs as ceramic tools depends primarily on their shape and that it is shape, more than anything else, that potters' select for as the feature that determines the functional value of the tools (i.e., he demonstrates that the inferred variable, function, not only depends on the shared attributes of form but that considerations of intended function also determine form, by quasi-causal connection, in source contexts). It was objected by Starna (1979), however, that in developing his argument this way Curren "separated what are clearly two interdependent parts of a single process" (1979:337); he failed to "take the next logical step" (1979:337) of establishing that the principle of connection, the quasi-casual relation of dependence between form and function, could have held in the subject context. When Starna undertook an examination of the relevant archaeological material, he found that stone gorgets frequently occur in archaeological contexts that are preceramic or show no evidence of ceramic production, thus establishing that similarities of form are not a relevant indicator of ceramic-making function in this case, effectively disproving Curren's interpretive hypothesis despite its initial high plausibility.

Curren's interpretive account fails, then, for want of attention to the question whether archaeological evidence bears out his supposition that a relational analogy (i.e., a similarity in structuring causal connection) holds between its source(s) and subject. By the same token, however, the effectiveness of Hill's interpretive argument and, in particular, the credibility of the subject-side testing that supports it, is undermined by his failure to provide comparably systematic source-side tests of the principles of connection that it presupposes. Hill's assumptions about the quasi-causal relations that (may) structure a community's creation and use of architectural space not only establish the prior plausibility of his interpretive conclusions, they determine what test results—what further points of positive analogy—constitute relevant evidence that a relational analogy holds between his source(s) and subject; where they embody knowledge about the operation and effects of such causal dynamics, they determine what will count as evidence that these dynamics operated in particular past contexts. Though an interest in ethnoarchaeological studies has grown steadily alongside the development of a testing program by the New Archaeologists, the real importance of such source-side work is only now receiving full programmatic endorsement. The great value Binford's objections to early New Archaeology testing procedures and of the radical criticisms of analogy developed by Gould (among

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others) is, in this connection, the stress they lay on the insight that the development of subject-side testing will only improve the status of interpretive conclusions if complementary source-side work is undertaken to establish the credibility (empirical, conceptual) of the presuppositions that guide both the formulation and testing of interpretive hypotheses. Perhaps more important, these critics have given source-side work a clear definition of purpose that it has lacked to date. They specify that the object of such research must be to establish (i.e., to articulate and, through systematic testing, to restrict and/or substantiate) the principles of connection necessary for relational reasoning and they make it clear that this requires archaeologists to go decisively beyond the documentation of detail and pattern in potential (ethnographic or experimental) sources; they must specifically seek an understanding of the causal or other relations of dependence that determine the manifest structure—the detail and pattern—of cultural, behavioral contexts and their material record.

Consider, briefly, the sort of recursive testing program that results when the interlocking strategies of improving sources and testing fit are systematically exploited as a means of establishing relational grounds for analogical interpretation. In the case of Clark's interpretation of Star Carr, for example, the directive to test principles of connection would require that source-side work involve not only the systematic study of variability in adaptive responses to subarctic environments but also, following Binford's example, direct investigation of the "causal" dynamics shaping these variable responses; it requires that an understanding of relational dependence be developed that specifies how and in what respects the cultural response is constrained or determined by material "limiting conditions". This is a variant of the first strategy for improving analogy; it involves a qualitative rather than a quantitative expansion of the bases for interpretation. It constitutes a test of what amounts to an explanatory hypothesis suggested by (or, suggested to account for) the regularities of association between techno-ecological and sociocultural variables that Clark noted, albeit impressionistically, and that David's study suggests might bear more systematic, detailed documentation.

Once developed, the task that faces an archaeologist who would use this sort of knowledge about source-side principles of connection is to devise tests that will determine their applicability to particular archaeological subjects and with this, emphasis shifts recursively back onto the subject-side work of expanding the comparison between source and subject in relevant areas. One most telling form of test made possible by developed background knowledge and sharply formulated hypotheses is the search for (or identification of) anomalies that establish the limits of presumed relational similarity. The special value of such a Gould-type falsificationist strategy is not only the decisiveness of the tests it provides (i.e., as tests that expose error rather than add to the extant confirming evidence), but also its capacity to open up new lines of research. Though Hill did not deliberately exploit this strategy, his tests were sufficiently clear cut that

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unambiguous counterevidence could be recognized; his expectations concerning the association of pollens and other plant remains with supposed food-processing areas were decisively disconfirmed. These negative results were one of the most valuable outcomes of the study, not because of their implications for Hill's functional hypotheses, but because they directed attention to inadequacies of the larger model of prehistoric pueblo society and of the underlying principles of connection that informed the formulation and testing of these hypotheses; they challenged formerly unquestioned assumptions about the similarity between prehistoric and contemporary pueblo subsistence patterns. In this, the tests generated a series of sharply focused questions about the nature of the dynamics and relations of dependence structuring pueblo-type contexts around which a new, and newly purposeful, program of source-side research could be designed. Longacre's (1974) study of Kalinga ceramic production, use, and deposition is an example of research that was designed specifically to answer questions about interpretive assumptions (assumptions about principles of connection) raised by this sort of directed, subject-side testing of reconstructive hypotheses; the testing in question paralleled Hill's and the questions raised concerned the assumptions that informed his and Hill's reconstructive inferences about prehistorical pueblo social structure.

Fully developed as tests for relevance, then, the strategies for improving analogical arguments suggested by the logic of analogy constitute mutually reinforcing procedures for checking the adequacy of both interpretive conclusions and interpretive assumptions about the uniformity, in particular respects, of past and present. As procedures for selecting among interpretive options and for testing and refining the background knowledge in light of which certain conceptions of the past are warranted as best interpretive options, they subsume earlier proposals for strengthening interpretative inference made by both critics and proponents of analogy. Most important, where they are effective methods of systematically limiting and exposing error in our uniformitarian assumptions, these procedures ensure that analogical inference can incorporate considerations of dissimilarities between past and present. In this, arguments by analogy—specifically arguments by relational analogy that involve considerations of relevance—do escape charges of radical and undifferentiable unreliability; they need not be formulated as simple, indiscriminate projections of present onto past.

The Value of Multiple Sources: Rebuttal to Charges of Distortion

The real value of relational forms of analogical inference is not just that they are potentially better-supported arguments (at least—on the source-side of the equation). It is that where they involve a discriminating, selective transposition of information from source(s) to subject, they can be a profoundly creative, expansive form of interpretive argument. Because such inference trades on

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knowledge about the specific causal implications that particular known features have for undocumented aspects of a subject context, one need not assume (or establish) an extensive mapping between this subject and the source(s) used to interpret or reconstruct it more fully, though this is one way of strengthening an analogical argument. A source that shares as little as a single attribute with the subject in question may be used as the basis for a (partial) reconstructive argument insofar as it exhibits clearly the specific consequences or correlates associated with this attribute that may be expected to occur in the subject context (i.e., insofar as it suggests what the having of this attribute implies for the subject). In this way, an interpretive model may be built up by appeal to a number of sources, each of which brings into view different, otherwise unknown, features of the subject, namely features that would (probably or possibly) have been associated with various of its known attributes given source-based knowledge about the causal powers or properties of these attributes. The importance of this is that if the subject combines attributes in a configuration not duplicated in any one known context, the resulting model will be a unique composite of features that the subject may be expected to have as the likely correlates of these known attributes (see the discussion of "multiply connected" analogs in Harré 1970:47–49). In this, the model may be a conceptualization of a context that is substantially unlike any single, accessible analog.

Where Gould's own interpretive arguments involve an appeal to knowledge about the adaptive behavior of biological populations, tempered by common sense and ethnographic knowledge about the unique capacities of humans, they illustrate just this possibility of drawing on a range of analogs or sources, including some that are known to be extensively different from the subject of inquiry. In fact, it is a telling and relevant irony that Gould illustrates his strong claim against analogy—his claim that it is categorically limiting of what can be understood about the past—with an interpretive account that is, itself, analogical and that demonstrates in concrete terms, the creative potential of drawing selectively on a diverse range of limited analogies (i.e., when you are dealing with a subject that is not, generally, "like" any one known source context). He argues that it is misguided to interpret the archaeological remains of early human populations (especially, evidence of their "home base" and kill sites) in light of knowledge about contemporary hunter–gatherers because "early man" may have lacked, for example, the use of fire and this would have "changed the 'ground rules' for survival" (1980:30). In particular, it would have made it dangerous to bring meat from a kill back to a home base for social sharing because, unprotected by fire, this would have attracted other predators. Given this, Gould suggests that living, nonhuman carnivores might constitute what amounts to a more apt, supplementary analog for certain early human behavioral patterns associated with home bases and with the treatment of the spoils of successful hunting, more apt that is, than the standard ethnographic analogs. Thus, where early humans can be seen to have been in some respects "like" contemporary hunter–

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gatherers, and in others "like" nonhuman carnivores, it is possible to draw on knowledge of these different source populations for an understanding of what it means to be a hunter without fire ("like" the nonhuman carnivores) and a hunter with uniquely human cognitive and social capacities. Taken together, these two sources do effectively constrain one another, imposing significant limits on the assumptions about further similarities that might be drawn if only one or the other source were considered and were projected, as a whole, onto the subject. The resulting model may be of a context that is radically different from any one known contemporary context or population but is yet conceivable in light of knowledge about a range of analogs whose relevance to the subject is recognized to be limited and is clearly defined.

CONCLUSION

In conclusion, I suggest that, contrary to the claims of the perennial critics of analogy, analogical inference is not radically faulty or categorically misleading. There are criteria and associated methodological strategies for strengthening and evaluating analogical inferences, if not for "proving" them, that clearly provide a basis for weeding out and decisively rejecting those cases of false analogy that originally inspired the reaction against analogy. Where, at a more sophisticated level of interpretation, the improvement and assessment of analogical credibility is based on considerations of relevance, the objections responsible for recent reactions against analogy can be turned as well. Arguments by analogy clearly can incorporate the different sorts of background knowledge, and are susceptible of the kind of systematic testing that the recent critics have identified with distinctively nonanalogical forms of inference. This means that though argument by analogy is inevitably liable to error, it can be closely controlled and highly discriminating with regard to dissimilarities between past and present; not all analogical inference reduces to simple assimilation of the unfamiliar to the familiar. I propose, then, that the skeptical worries expressed in archaeology's "chronic ambivalence" about analogy be answered, not by attempts to restrict inquiry to safe methods and the limited ends attainable by them, but by exploring more fully the potential for raising the credibility of those necessarily ampliative and usually analogical inferences on which archaeology must rely if it is to bring unfamiliar and otherwise inacessible aspects of the past into view.

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