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Introduction

C. C. LAMBERG-KARLOVSKY

This volume of essays is an unanticipated outcome of a formal archaeological exchange program initiated between the U.S.S.R. and the U.S.A. This collaboration is sponsored by the American Council of Learned Societies and the Soviet Academy of Sciences and is administered in the U.S. by the International Research and Exchange Board. To date three symposia have been held: Cambridge (1981), Samarkand (1983), Washington (1986), and a fourth is to be held in Tbilisi (1988). In addition, collaborative excavations at the Bronze Age site of Sarazm, Tadjikistan, S.S.R., have involved the participation of Philip Kohl, of Wellesley College, and myself. A number of years ago Professor Kohl conceived of the idea to edit a volume of essays addressing aspects of method and theory as practiced and written by Soviet archaeologists. That book, being edited by Philip Kohl, contains over twenty essays, and will be published by Cambridge University Press. There is a widely held belief among American archaeologists that Soviet Archaeology conforms to a particular "school," loosely referred to as Marxist, which implies that all Soviet archaeologists interpret the past according to the evolutionary dictates as set down by Marx and Engels. This is a narrow, ill-informed, perhaps even politically motivated, conception of the real diversity of approaches which characterize Soviet archaeology.

Structural and methodological differences, however, are readily apparent in the archaeological practice of the U.S.S.R and U.S.A. Some of these may be enumerated.

1 Within the Soviet Union there is a greater degree of centralization within archaeological research. Most research and excavation in the U.S.S.R. is carried out by scholars affiliated with Institutes of Archaeology of the Soviet Academy of Sciences. Each of the fifteen Soviet Republics maintains its own Institute of Archaeology or, in its absence, has numerous archaeologists affiliated with the Institutes of History. These centralized institutes, rather than universities as in the U.S.A., coordinate and undertake the majority of archaeological research.

2 Long-term archaeological field programs in the U.S.S.R. representing substantial funding, often for well over a decade, have uncovered broad horizontal exposures from the Paleolithic to Medieval Ages. Soviet archaeologists frequently find Western research programs with their smaller-scale horizontal exposure and statistically derived cultural reconstructions insufficient for meaningful commentary or adequate reconstruction of economic and social organization.

3 Both the U.S.S.R. and U.S.A. maintain varying degrees of interests in (a) an

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ecological perspective; (b) the evolution and role of technologies and (c) settlement survey. In reference to (a) there are perhaps more trained paleoethnobotanists, zooarchaeologists, geomorphologists, et al. in the U.S.A. than exist in the U.S.S.R., but interdisciplinary programs are by no means uncommon. In reference to (b) there is an equal concern over the role of technology and social change but the U.S.S.R. is not well developed in archaeometric studies.

4 The U.S.S.R. has not developed a major focus or facility for archaeometry. Few radiocarbon laboratories exist, provenience studies are all but unknown and a strong materials science approach to metals, ceramics, and other organic remains is limited.

5 Sampling strategies, though less frequently adopted within the U.S.S.R., are nevertheless undertaken by Soviet archaeologists. It is difficult to generalize in this instance for there are important regional and problem-specific differences in the archaeology of the U.S.S.R.; some have generated, others eliminated, the adoption of sampling strategies.

6 Though settlement pattern studies have been undertaken in the U.S.S.R., indeed one can make a very strong case that the settlement pattern approach to archaeology was first developed by the Soviets (Tolstov 1948), there remains a strong focus on single sites rather than regions. Spatial information, size, number, and precise locations of sites are often difficult to come by. The extensive and interdisciplinary settlement surveys which characterized the work of R. McC. Adams (1981) in the Near East and MacNeish (1972) in Mexico are all but unknown in the U.S.S.R.

A more complete discussion of Soviet archaeology as specifically practiced in Central Asia is available in Kohl (1984b:237–48). Lastly, we are left to comment briefly on Marxist theory as applicable to archaeology in the U.S.S.R. Too frequently I have heard in the U.S.A. the expressed belief that archaeologists in the U.S.S.R. mindlessly relate the archaeological data to the Marxist model of historical materialism. This is definitely not true, as is readily evident in a major theoretical article on the epistemology of archaeology, co-authored by a Soviet archaeologist and philosopher, in a recent issue of their principal journal *Sovetskaja Archeologija* (Bashilov and Loone 1986:192–208). Soviet archaeology is Marxist in so far as it offers a strongly evolutionary emphasis, concerns itself with aspects of production, technology, and social formations in terms of class relations; in short, it emphasizes a materialist perspective. Its materialist perspective, however, as distinct from that in the U.S.A., is virtually devoid of ecological determinism. Environmental determinism is far more prevalent in the West than in the U.S.S.R. Kohl (1984b:246) in commenting upon Soviet archaeology has written "that ideological practices are emphasized to a degree that seems almost surprising if one expects Marxist-directed or inspired reconstructions to downplay superstructural features . . ." It is not, however, surprising at all if one follows Marx in the belief that material relationships form the basis of mental production (see p. 9); a logic which I find hard to fault. In light of the above, it remains puzzling that American Marxist archaeologists are largely hostile to those who consider ideology a significant force in social formations.

Lastly, from experience based upon our own direct involvement in Soviet archae-

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ology over the past half-decade the following statement by Kohl (1984b:246–7) deserves restatement:

It also must be emphasized . . . that diversity of opinions exist in Soviet archaeology, and one often reads articles highly critical of the works or theories of fellow colleagues. A monolithic structure promoting uniform, dogmatically held policies is a caricature of the real situation and says more about Western stereotypic conceptions of Soviet society than it does about the real nature of Soviet archaeology.

The preparation of the Soviet volume by Philip Kohl led him to suggest that I undertake a companion volume of essays, stressing theory and methodological approaches, as undertaken by archaeologists on this continent. Bruce Trigger, in his chapter for this volume, indicates that: "There is . . . no unity to archaeology as a discipline in the United States." I wholly agree, there is no single dominant paradigm, no "school" which encompasses inelastic theoretical or methodological imperatives within American archaeology. In light of the above, the selection of contributors was necessarily arbitrary, representing a mosaic of different theoretical and methodological approaches in the archaeological undertaking. Nevertheless, each contributor is noted for stressing certain approaches for reconstructing the past; approaches which are not necessarily compatible with each other. This volume does not pretend to provide a "holistic" overview of the archaeological perspectives in the Americas; nor does it pretend to offer geographical or chronological comprehensiveness. The perspectives of American anthropological archaeology dominate the volume; humanist approaches, incorporating art history; classical archaeology, Egyptology and so on are entirely absent.

Initially I formulated a list of twenty contributors, assigning to each of them a topic on which they had made notable contributions. Three of the requested contributors, Robert McC. Adams, Gregory Johnson, and Kent Flannery, declined the invitation in light of prior commitments. All others, somewhat to my astonishment, agreed to participate and all complied to a rigid time schedule, which, in a volume dependant on numerous contributors, was even the more astonishing. It was clear from the start that the volume would not represent all "schools" of thought nor would it strive to promote the mission of a single cause.

Over the past two-and-a-half decades American archaeology has been partially dominated by what Trigger refers to as "the third major paradigm in the history of American archaeology," namely, New or Processual archaeology, which is based on a neoevolutionary perspective (see Trigger, this volume). The materialist bias of this perspective is discussed by Arthur Demarest in chapter 6. Prior to turning to the epistemological framework, in which most of the New Archaeology was initially undertaken, a few comments on its contextual emergence are offered. Recent years have seen an increasing interest in the "uses of the past" as a symbolic resource for legitimizing authority (Fowler 1987). Equally significant is an effort to comprehend the intellectual and sociopolitical contexts in which archaeology is conducted.

It is frequently said that each generation must construct its own interpretation of

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the historical past. Such a statement is a tacit recognition that interpretations of the past are imbedded in issues and concerns of the present. In this vein it may be of interest to reflect upon the social and intellectual climate in which the New Archaeology emerged. A distance of over twenty years allows for the linearization of perspective. The New Archaeology emerged in this country in the mid-sixties at a time of increasing social turmoil. The younger generation became increasingly distrustful of the views and interpretations being offered by its leaders. The principal issue was, of course, the U.S. government's tragic involvement and defense of its military confrontation in Vietnam. Simultaneously, the injustice of the military draft and the iniquities of race and gender relations set a younger generation in opposition to the "informed wisdom"; that is, the perception and guidance of its elders, including within the family unit. Struggle, opposition and mistrust of the traditional, indeed revolution characterized the 1960s and early 1970s. A younger generation had successfully confronted the establishment and won, at least partially, the directive of its causes. It was within this social milieu that the New Archaeology was born. It was explicit in castigating all that came before it as an inadequate, misconceived "traditional" archaeology. Certain elders of the "traditional" approach were caricatured as mentalists communicating with sherds. The New Archaeology was trumpeted as a "paradigm shift," in the Kuhnian sense, totally replacing the archaism of the "traditional." Its participants were involved with a revolutionary struggle directed toward the total displacement of a previously "informed wisdom." Leslie White (with his materialist and evolutionary perspective) and Albert Spaulding (advocating a quantitative methodological rigor) were the ancestral Darwins and Lew Binford their self-appointed Huxley (see Binford 1972:13). To the above was added an epistemological approach for the undertaking of science; the philosopher of science Carl Hempel's nomothetic deductive (N–D) positivism became the *sine qua non* for advancing the New Archaeology as a science revitalized.

No one can doubt that the New Archaeology has advanced the discipline. I dare say that there is not a contributor to this volume who does not owe the New Archaeology a substantial debt. It has, however, fallen far short of being a "paradigm shift." In fact, it could never have become such due to its exclusive dependency on the N–D theories of explanation and cause. It should have been recognized, and, indeed, it was by a few archaeologists, and a great many philosophers of science, that the N–D theory of scientific explanation was deeply flawed. The archaeologists who contested this model of scientific method were deemed "traditional" and the philosophers of science who challenged the primacy of the N–D model, or presented other models of scientific explanation, were simply ignored in the archaeological literature. The commitment to the Hempelian model remained at the very basis of the New Archaeology – it was, and remains, in its exclusionary commitment to this model of science quite simply wrong! Before commenting further on the N–D aspect of archaeological science, one other aspect for the contextualization of the New Archaeology deserves comment: the job market. The 1960s and early 1970s saw an increase in jobs within academe – a time of promised "guns and butter." Departments of Anthropology were splitting off from Sociology – as I myself first experienced at Franklin and Marshall College in 1964 –

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creating new jobs in anthropology. Additionally, entire universities, indeed entire university systems, were being created, as, for example, the state university system in New York at Binghampton, Stonybrook, Albany, and so on. The newly formed Departments of Anthropology, as well as departments already established (usually dominated by social anthropologists) found an undeniable appeal in an archaeologist participating in a would-be paradigm shift; suggesting their greater relevance toward an understanding and explanation of social anthropological systems. Their failure to deliver this "paradigm shift" involves the commitment of the New Archaeology to the N–D model.

That there is an amelioration in the rigidity toward explanation and the use of the N–D model in archaeology can be perceived in the following passages (Redman 1987):

It is widely recognized that a project should have a problem orientation and almost every research proposal written is replete with goals. What often happens is that goals are indeed put forward, but they are too general, too numerous, and too unrealistic. Other scholars take a different position and argue that our interpretive goals should emanate from our work, not be formed in advance. This is a more complicated issue than was acknowledged by new archaeologists who years ago downgraded the latter position as "narrow inductivism" . . . I agree, but only in part, with the position of new archaeologists that one needs specific hypotheses in order to define relevant data to collect (p. 257).
Nevertheless, I also think that to speak to the entire discipline, not just to those who adhere to a new archaeology position, we should acknowledge that, in fact, much good work is done without *a priori* problem definition and without explicit concern with middle-range theory (p. 259).

These passages, written by Professor Redman, appear to temper the views expressed in his earlier work (Watson et al. 1971) in which there was advanced one path, and only one path, toward explanation in archaeology, namely the Deductive-Nomological model as advanced by the philosopher of science Carl G. Hempel. Hempel's approach to science, the approach adopted by the New Archaeologists, was advocated by L. R. Binford in the influential volume *New Perspectives in Archaeology* (Binford and Binford 1968). What was "new," and perhaps all that was "new," was the dependence on an epistemology derived from Carl Hempel. Binford (1968:17) put it this way:

The changes in archaeology which are documented in this book are more than simply new methods and new theories: the changes consist of theories and methods developed in the context of a *new epistemological perspective on such basic issues as the appropriate scientific procedures* to be followed in investigating the past (emphasis mine).

The "appropriate scientific procedures" were those advocated by Hempel. If archaeologists followed those procedures they could become "scientists" – explaining the past and predicting the future. "By use of scientific laws and theories, explanations

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can be given and predictions made" (Redman 1971:vii). Much of the contemporary dispute in archaeology – over hard versus soft paradigms and methods – is part of an older and larger conflict: the confrontation between positivism and romanticism. Today this debate in archaeology has passed the point of diminishing returns; further heat has yielded less and less light. This is not the place to review or outline Hempel's model (Hempel 1966). Suffice it to say that according to Hempel's (1948) classic account to explain a fact is to show how, under specific circumstances, it was only to be expected. This is accomplished by deducing a specific fact from different premises about the prevailing conditions and laws of nature. It is astonishing that Binford's "appropriate scientific procedure," which depended exclusively upon Hempel's model was, already, in the early 1960s a philosophical approach in deep trouble! Hempel's model, first adumbrated in the early 1940s, was being attacked already in the 1950s. Under this attack Hempel was forced to modify his model and offered numerous exceptions to his general theory – all of which went unnoticed in the archaeological literature. (For an analytical and historical view of the Hempel model see the various essays in Feigl and Brodbeck 1953; the chapters on "Explanation and Laws" in Gardiner 1959; Dray 1966; and Hexter 1971.)

Today it is possible to say, "it is well-known that the D–N (deductive nomological) model is wrong" due to, in part, "its implicit commitment to a defective theory of causation" (Horwich 1987:9,143). It is ironic that the general model of science which the New Archaeologists adopted, the D–N model, on which they predicted a paradigm shift, is defective in detailing cause – precisely the aspect which together with explanation was the New Archaeology's paramount concern. Fortunately, not all archaeologists embraced the rigors of the D–N model. Many, early on, shared the sentiment that "Unfortunately, his [Binford's] students and followers have been overimpressed with the great simplicity and apparent logical power of the deductive-nomological paradigm of scientific explanation and have pushed the discipline in a questionable direction" (Sabloff, Beale and Kurland 1973:105).

The unreflective nature of the New Archaeology was astonishing but not more so than its almost anti-intellectual insistence upon the absolute truth of a single epistemological framework of science. There is further irony in the fact that the whole-cloth adoption of this epistemology became, in practice, less of a method than an ideology, an attribute of culture which was all but banished from archaeological discourse. The D–N model became, in fact, a commodity reduced to a fetish; it became something quite fashionable which without presented alternatives was responded to by "I'll buy that!" Its purchase provided instant membership in a self-proclaimed paradigm shift. Never mind that no-one explicated what its shift was directed toward, other than a concatenated scientism. By the mid-1970s the "school" of New Archaeology had become so absolutist that one of the major government funding agencies favored only proposals whose research design was framed according to explicit deductive-nomological hypothesis-testing approaches. In discussing the New Archaeology and the Hempelian model two British archaeologists have recently observed: "The unfortunate spectacle is one of archaeology embracing thoroughly discredited and outmoded ideas as the framework for its own advances" (Shanks and Tilley 1987:32).

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The New Archaeology is not only comprehensible in the context of the above discussed sociology of the mid-1960s but also in the logic of post-modernist construction, namely, that which makes a community is its specific form of "sociality" (see Kroker and Cook 1986). What counts is if a commodity sells, if it is popular, if it's the in-thing – not whether it is credible, correct or even comprehensible. The construction of the New Archaeology community, its "sociality", was explicitly configured: "We had fun that night. All my students were there . . . People from other places who were doing innovative things were at the party . . . We laughed, we sang, we joked . . . Change could occur. The younger students began to refer to us as the 'Mafia'" (Binford 1972:13).

The rapid spread within archaeology of the "culture of narcissism" so fundamental an aspect of post-modernist society, quickly reached England and is well captured in an essay by Kohl (1985:107): "Thus, Renfrew slapped his American counterpart, Binford, on the back and in an obsequious foreword to an 'outstandingly important book' by the 'outstanding archaeological thinker of our time . . .'" The result has been what some have come to refer to as the Anglo-American school of archaeology.

Today a new wind is passing over archaeology. Typically, what is new to archaeology is passing into old age within Anthropology or related disciplines (Leone 1972) – once again, reflexive evidence that archaeology derives its method on how it is to view the past from related disciplines. Archaeology appears to be consistently a consumer, rather than a producer, of perspectives, theory and methods derived from other fields. There is nothing inherently wrong with this but one must wonder why the perspectives, theories and/or methods take a decade to percolate into the archaeological consciousness. Today archaeologists are turning toward "symbolic" or "interpretive" modes in which, it is argued, the discipline must move to "hermeneutic, interpretive processes, dialectics, praxis and archaeology as cultural critique" (Shanks and Tilley 1987:243). This perspective, advanced most articulately by Hodder (1982b, 1986), returns to idealist constructs (see Demarest's chapter in this volume) in which the "meaning" of the past is to be seen as a Geertzian "text" of contextualized relativism. As in the manner of hermeneutics we are now informed that each interpretation creates a new meaning. Within two decades archaeologists have gone from a formal rigor of methodological scientism to archaeology as "interpretive quest." One is reminded of the lapel button: "Roses are red; violets are blue; I'm schizophrenic and so am I." Today with such "interpretive quests," which relativize all categories as mere symbols, denying even their material content, we are informed that even power doesn't exist, that it was always only a "perspectival simulation" of itself (Baudrillard 1980). This form of self-indulgence, in which anthropology is reduced to a relativism of interpretive quest, is a greater immaculate deception than what preceded it. Archaeologists swing between the polar oppositions of a crude scientism in which culture is only comprehensible as a mechanical adaptive process to a culturalist flattening of the world into "symbol" and "text."

Colin Renfrew, who has carried the drum for Binford's bandwagon and early identified the decade of the 1980s as one of "Cognitive Archaeology," now carries the drum for Ian Hodder (Renfrew 1982b). In his inaugural address, "Towards an

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Archaeology of Mind," Renfrew (1982b:1) begins with the current fascination for relativist positions:

> Each age and every society has its own image of the human past; an image which shapes its attitudes to the present and governs the nature of what is seen to be possible and appropriate action in the world.

There is a curious mirroring within archaeology of the debate which characterized the field of history in the 1930s (!) in the conflict between the "New Historians," the "Scientific Historians," and the emergence of "Historical Relativism" linked to the names of the historians Carl Becker and Charles Beard. As Becker (1932:253) put it: "Every generation, our own included, will, must, inevitably understand the past . . . in the light of its own experience": and, as Beard (1934:220) wrote:

> Each historian who writes is a product of his age, and his work reflects the spirit of the times, of a nation, race, group, class or section . . . Every student of history knows that his colleagues have been influenced in their selection and ordering of materials by their biases, beliefs, affections, general upbringing and experience.

The disturbingly recent recognition of this reality has directed archaeological thought toward an increasingly idealist and relativist position wherein all objects are merely symbols and these symbols offer a subjective "text" of meaning. In this approach propositions become true or false only in reference to the assumed state of the world rather than to objective knowledge. If archaeology in past decades stood in danger of being a bad science in adopting the rigors of a Hempelian model, it stands today in equal danger of being a poor philosophy in the call to "move beyond the opposition of subjectivity and objectivity to a hermeneutic interpretative process" (Shanks and Tilley 1987).

That American archaeological thought has, nonetheless, not become buried in an epistemological vice nor a philosophical quagmire is well evidenced in the following chapters. The obligatory reference to Hempel, the penchant to offer a test of a specific hypothesis, the elucidation of a newly discovered general law of cultural process, or the post-modernist obsession slowly creeping into archaeology which attempts to discover the relevance of Wittgenstein or Critical Theory are all but absent. This is not to say that the authors are unaware of the above – it does suggest, however, that they are willing to leave to others the residue of intellectual "showboating" in the "hermeneutic circle."

The majority of chapters in this volume offer a formal argument applied to a concrete work. The Maya, the Inka, China, Mesopotamia and the Paleolithic are all represented. Conclusions from single instances or arguments based on single factor causes are absent, as are "explanations" based on origins. There are few, if any, American archaeologists who would agree with Peter Winch's (1958) wrong-headed belief that because we are "cognizers and agents we can ignore nature." To a certain extent archaeology today in the U.S. offers no explicit creed, only aids toward learning; no universal principle except "It all depends . . ." George Cowgill and

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before him Albert Spaulding have sensitized a generation to the importance of the careful application of statistical techniques to elucidate patterns. Cowgill's cautionary advice forewarns those who too readily would see a social "reality" behind a numerical "fact."

Numerous chapters in this volume point out that materialist approaches have dominated American archaeology over the past several decades. More recently, as argued most explicitly by Demarest, the materialist emphasis must be tempered by considering the role of ideology. Gilman advances Marxist perspectives critical of "adaptationist" and positivist approaches and avoids the issue of the role of ideology in generating change. In light of the ongoing debate this is somewhat surprising, for Marx was very explicit concerning the role of ideology within a materialist framework. The importance of ideas to material production, of increasing concern within the materialist versus idealist debate in archaeology, merits our recalling the full quote from Marx (Marx and Engels 1970:64):

> The ideas of the ruling class are in every epoch the ruling ideas, i.e. the class which is the ruling material force of society is at the same time its ruling, intellectual force. The class which has the means of material production at its disposal has control at the same time over the means of mental production, so that thereby, generally speaking, the ideas of those who lack the means of mental production are subject to it. The ruling ideas are nothing more than the ideal expression of the dominant material relationships, the dominant material relationships grasped as ideas . . .

This passage leaves little doubt that material relationships, as conceived by Marx, are the basis for "mental production." Max Weber added to this the notion that every system of power, authority, or whatever, strives to *legitimate* itself through an ideology.

The question becomes, of course, whether we can put the role of legitimation and ideology in terms of causation – the causality of the infrastructure on the superstructure – or is it to be expressed by another conceptual framework? It remains one of the challenges for a Marxist history of ideas to relate a more plausible connection between a system of *interests* and a system of *thought*. It is essential to articulate the relationship between an interest and its expression in ideas within a system of legitimization. Such a framework must then introduce the notion of motivation in attempts to justify a system of authority (for the complexity of the process see Ricoeur 1986:68–102). Explanation in terms of motives is not the same as explanation in terms of causes. Motives or intentions do not "cause" actions; they do, however, provide a teleological account of them.

Gilman is, I believe, correct in pointing out that "differences between Marxist and non-Marxist work are often slight." His observation, however, that most recent American Marxist archaeologists "received their intellectual formation during the Vietnam war years" and reflect "the need of younger scholars to establish distinctive niches in the academic struggle for survival" appears to duplicate, but with a different voice, what we referred to in speaking of the New Archaeology as a "community of

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sociality." This appears to be particularly true in the penchant for Marxist archaeologists to quote each other's work in a favorable light and to discover in those works, to the elimination of other obviously relevant and prior studies, the starting point of all important perspectives. At times this ideological tribalism resembles a form of intellectual Darwinism – a communal solidarity of shared intellectual ideas engaged in the struggle for survival against competing attitudes. Gilman's chapter is unique in his startling requirement for a political commitment to the Marxist intellectual framework: "The critical definitive criterion is, then, political commitment." As one who has gained a very great deal from the Marxist framework is it naive to ask, "A political commitment to what?"

Archaeology in the Americas is increasingly characterized by rationalizations of its application to society. Perhaps in reaction to the past emphases on materialist "explanations" and the scientism of the "New Archaeology," a fundamental conflict is once again emerging between scientism and humanism, between rational calculations and humane values.

In archaeology this conflict appears in the dispute between praxis-oriented versus value-neutral explanations. In its more committed form deterministic theories of structure are opposed to voluntaristic theories of consciousness. Perhaps each generation must confront, with varying degrees of scholasticism, this presumed polarization. Archaeology today begins to grapple with the old-fashioned hermeneutics of Wilhelm Dilthey (Makkreel 1975), who asserted that human action can only be understood through an interpretive rather than through the causal logic which explains the natural world. It seems to me that numerous archaeologists, not only in this country but also abroad, are undertaking floral arrangements of ideas which have little more than a metaphysical connection to archaeology (for its exaggerated practice see Shanks and Tilley 1987). One is reminded of Gellner's (1987) apt phrase, "All or most of us in the academic profession are guilty of some humbug some of the time." From an earlier period of archaeologists offering very factual facts to the present where "facts" are not even believed to exist (Sabloff, Binford and McAnany 1987) is an archaeological sojourn of less than twenty years.

Today, as Demarest points out in his chapter, there is an increasing polarization between idealist versus materialist perspectives. Such a dichotomy falsifies their dialectical relationship. The construction of meaning and the use of symbols is inherently a matter entailing the construction of political and economic interests while the concerns of a political economy are inherently conflicts over meanings and symbols.

In the provocative book *Anthropology as Cultural Critique* by George Marcus and Michael Fisher (1987), the authors note that anthropology moves away from the conception of a "natural science of society" toward anthropology as an interpretive quest in search of meaning, symbol and language. Within this country, of recent years, there are increasing evidences for the use of archaeology as "cultural critique." The study of Mark Leone at Annapolis; William Rathje's various Garbage Projects utilizing archaeological methods for the study of present-day consumption patterns, class structure, etc., and James Deetz's various studies of colonial America serve as

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