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THE ANCIENT MAYA AND THE POLITICAL PRESENT

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Can archaeologists depict the past with any accuracy, and is that their goal? Where do archaeologists' ideas come from in the first place? This paper suggests that archaeological discourse has a dual nature: at the same time that it pursues objective, verifiable knowledge about the past, it also conducts an informal and often hidden political and philosophical debate about the major issues of contemporary life. This paper investigates this second, hidden dialogue within a single subfield of archaeology, Maya prehistory. What gives power to the past, and to archaeology, is the way it is used to political and philosophical ends. The task is to recognize the nature of the dialogue and to take responsibility for it.

PROFESSIONAL ARCHAEOLOGISTS ARE ACCUSTOMED to changing explanations and theories; they interpret such change as a sign of progress in the discipline. Consciously or unconsciously, archaeologists tend to see the constant proposal, evaluation, and rejection of new ideas and theories as a part of the scientific method, one which leads us gradually closer to objective truth.[1] Yet to many laymen, amateurs, and students, the frequent change of explanations and reconstructions of the past (excluding of course those based on "new discoveries") is somewhat bewildering. The perception that the latest theory is likely to be short lived and that all aspects of the past are subject to interpretation does not instill in nonarchaeologists a fascination with archaeological methodology or epistemology. Rather it leads to cynicism—to the perception that views of the past are determined by fashion, by competition for status within the profession (between individuals and/or groups and schools), by borrowing from other disciplines, or by a reluctant accommodation to new discoveries. The credibility of archaeology suffers when serious scholarship is perceived to be some sort of arcane game.[2]

Defensiveness in the face of such criticism leads many archaeologists to emphasize the part of their work which is rigorous and scientific. Frequent assertions by professionals that archaeological hypotheses are formulated and evaluated in an objective and scientific manner are a direct bolster to the "progressivist" view that archaeology is gradually homing in on explanations that are right and true. This is an implicit, and sometimes explicit, denial of the substance of the lay critiques—that archaeologists are just making up stories about the past.

In this paper I suggest that neither the critique or the defense do justice to the true complexity (or beauty) of archaeologists' relationships to the past. The process of explaining the past is not a frivolous game, but neither is it a simple scientific quest for objective truth. There are elements of truth in lay perceptions of the profession: hypotheses about and explanations for the past are not generated in an abstract, objective way, and the acceptance and/or

rejection of these hypotheses is not necessarily based on rigorous scientific testing. But to accept this as fact does not require that we adopt the cynical pose that all prehistory is just academic gamesmanship lacking in any scientific credibility.

Archaeology has a dual nature; it simultaneously engages in a fairly rigorous pursuit of objective facts about the past and an informal and sometimes hidden dialogue on contemporary politics, philosophy, religion, and other important subjects. It is this second dialogue, based on archaeologists' perceptions of the present and their experience of the world (including their experience of fieldwork), which brings motivation, passion, interest, and relevance to the whole enterprise. This is what makes archaeology an essentially "reflexive" science, one which reflects back on the present as much light as it sheds on the past.

I have no intention of taking an extreme position like that of some anthropologists (e.g., Sahlins 1976:220), who feel that the past is nothing but a cultural construct, lacking any objective reality. This would be as fallacious as insisting that the past is like an object which the archaeologist merely uncovers and puts together like a broken pot. The true situation is much more complex and messy. Objective knowledge of the past is interwoven and intertwined with reflexive commentary, and for usual intents and purposes it should probably remain so.

In this paper, however, I will try to disentangle, isolate, and dissect the reflexive component of a particular corpus of archaeological explanation of the past. My intent is not to single out one group of archaeologists and point an accusing finger or hold anyone up for ridicule. Certainly any other group of archaeologists would serve just as well. I have chosen Mayan prehistory only because I know this literature best.

The point is to untie the reflexive element of archaeology from its status as a naive lay criticism, to show that it really (even "objectively") exists. Once the topic can be discussed freely, we may reach the conclusion that there is nothing shameful about a certain lack of objectivity in our choice of explanations and hypotheses concerning the past. Rather than being considered a pollution of science, the reflexivity of archaeology should be viewed, I believe, as the very element which makes it interesting and relevant. The ultimate point is not that we have to do something about the situation, but instead that we may be able to draw upon it and convert it into a strength.

CONVENTIONAL WISDOM ON THE SOURCES OF THEORY

Considering how much introspection has gone into recent archaeological writing, the endless discussions of epistemology and the place of archaeology in the philosophy of science, it is strange to find so little has been written on the ultimate origins and sources of explanation and theory. We have heard a great deal about what to do with an explanation, a hypothesis, or a model once it has been found or formulated. But where do they come from in the first place?[3]

One of the major elements of the self-conscious scientism of the "New Archaeology" was a critique of inductive reasoning, then seen to be a characteristic of the old cultural-historical approach (see, for example, Watson, LeBlanc, and Redman 1971:28; Fritz and Plog 1970:411–12). Binford (1968) and others thought that the idea of archaeological explanations emerging from data through empirical analysis was bogus; when "old archaeologists" said their theory emerged from the data they were really using a whole series of unspoken, implicit assumptions, which could never be tested because they remained concealed.

So the "New Archaeology" was involved in a critique of the origins of explanation and theory from the start. What alternative was offered to "unscientific" inductive generalizations based on shaky assumptions? There were two. One was to be explicit about assumptions, to differentiate between what was a hypothesis to be tested and what was data, and to be conscious of the process by which hypotheses are constructed and used (best exemplified by the arguments in Watson, LeBlanc, and Redman 1971). The other was a bit more contentious and considerably more muddled—a call for a "deductive nomothetic" methodology by which specific hypotheses about prehistory would be derived from "established social-science laws" (Trigger 1973:107). Rather than generate their own hypotheses from observations of the past, archaeologists were to derive explanations from elsewhere and then explicitly test them; following Hempel, a past phenomenon would be explained when it was subsumed under such general laws.

But where *were* the general laws of society? The very existence of such laws had been a subject of heated debate since the Enlightenment. Sociocultural anthropology only offered some cross-cultural statistical regularities which were not particularly useful (e.g., Murdock 1949 and the Human Relations Area Files research). For a while there appeared lists of where the laws were going to come from: cultural evolutionism, general systems theory, locational analysis, demography, and population theory (see Trigger 1973:101–4; Leone 1972:25; Redman 1973:11–20; Watson, LeBlanc, and Redman 1971). But it became clear that these bodies of knowledge had only general, and often untested, assumptions to offer rather than hard "laws," and they were often contradictory. Furthermore, there was some dissatisfaction with the idea that archaeologists could only test the laws produced by others—they should be able to be law producers as well as law consumers (Reid, Rathje, and Schiffer 1974).

Finally, a common position on the sources of theory and law was borrowed from the philosophers of science: that it *did not matter* where hypotheses came from, whether from general laws, from observation, or from imagination. Scientists should only be concerned with how hypotheses were tested (Hempel 1966:15, 16; Binford 1977:2) not where they came from. Using this logic, Watson, LeBlanc, and Redman (1971:33, 7–8) condemned "old archaeologists" for unscientific derivation of explanations and hypotheses *and* followed Hempel in stating that the sources or derivations of hypotheses are unimportant. So the critique that the "old archaeologists'" hypotheses were derived incorrectly was negated almost as soon as it was raised.

The acceptance of Hempel's assertion that the origin of theory and explanation is irrelevant to science masks the importance of the issue of where archaeologists' ideas come from. Binford still takes archaeologists to task for being empiricists and thinking that theory emerges from observations and generalizations. He recently has restated his belief that "theory represents *inventions* of the human mind. . . . We invent, rather than discover, theories or parts of theories" (Binford 1985:583). Binford is remarkably consistent in his critique, but like most other archaeological theorists he leaves unexamined here the issue of *how* those theories are invented. Do they appear in the brain by divine inspiration, by dint of training in a good graduate school, or by some more complex mechanism that does not invite close examination?

Conventional histories of archaeology, like histories of anthropology in general, take what could be called a "normal science" view of the origin of hypotheses (see, for example, Willey and Sabloff 1973; Bernal 1980). Scientists work within a scientific milieu, deriving and testing hypotheses within the traditions of their own field or subfield. They get their ideas from each other, directly or indirectly, and the coherence of this transmission allows the identification of "schools" and lines of descent from one group of scholars to another. Through a series of "begots" the historian traces ideas back to influential scholars through their students, keeping things well within the bounds of the discipline (Willey and Sabloff 1973:187). Of course at times there is cross-fertilization between disciplines, the collision or melding of different research traditions, and even unaccountable wild innovation.[4] These histories depict a discipline somewhat isolated from the world, engaged in dialogue with itself and a few close relatives, with an occasional visitor from far away dropping by for a chat. While contemporary politics and cultural currents could influence archaeological debates and archaeology could be turned to political purposes (as in early disputes over American Indian origins, see Willey and Sabloff 1973), these influences are considered indirect in modern times. And some archaeologists find even the possibility of such influence to be very threatening to their image of objective science (e.g., Ford 1973).

In the last few years however, archaeology has become more introspective, and alternative histories of the field have appeared. Several recent analyses of "regional traditions of archaeological research" make strong cases for direct influence of political change on archaeology (see Trigger and Glover 1981; Trigger 1981; Chang 1981; Lorenzo 1981a; Bulkin, Klejn, and Lebedev 1982; Bar-Yosef and Mazar 1982). The best cases are made for the Soviet Union and China, though Israel should be a strong runner-up in the race for direct political involvement in the interpretation of prehistory. In a similar vein, Kristiansen (1981) offers a perceptive analysis of the history of Danish archaeology, paying close attention to the class position of the archaeologists and their audience and to the cultural and political content of research and publications.

Contemporary research has also been the target of sociopolitical analysis, in which the social role and status of the profession is shown to have guided the choice of research areas, topics, and methods (see Lorenzo 1981b and

papers in Gero, Lacy, and Blakey 1983). But even these finely textured and highly introspective analyses do not go so far as to suggest that the actual *content* of explanation and theory is affected or determined by contemporary events. They suggest that archaeology may be politically motivated or serve political purposes; presumably this occurs because archaeologists are aware of who they are and what the past signifies. But it is another thing to say that the theories, explanations, ideas, and specific reconstructions of past events are unconsciously but directly reflecting current events.

Just such an argument is offered by Trigger (1981), who, for example, links the popularity of catastrophe theory among prehistorians with the increasing perception that Western society is heading towards a (presumably nuclear) catastrophe. Similarly, Leone (1972:24) suggests that interest in general systems theory stems from "the pervasiveness of certain aspects of technology in modern American Culture." And Rathje and Schiffer (1982) link interest in migration in early archaeology to waves of immigrants coming to the United States and interest in diffusion to colonialism. Tenuous and isolated connections such as these may appear to have only a minor influence on the main flow of archaeological discourse. If one believes that social science is generally objective and value free, then a few minor connections between current events and the interpretation of the past can be excused as regrettable, but understandable, deviations.

An alternative view is that archaeology, like other social sciences, *always* draws on current events and politics as a source of general orientation, as criteria for the choice of research questions, and as sources of specific hypotheses and explanations about prehistory. From this perspective, the depiction of the past is inseparable from the present in which it is presented (Leone 1981). The empirical question then becomes one of just how close the link between present and past really is. I will suggest that the connections are much more common, specific, and direct than most archaeologists accept.

While the thematic connections I draw are more direct, I do not think that they flow from the conscious expression of political philosophy by archaeologists. Rather, I believe that correlations between what happens in the present and what is depicted to have happened in the past flow from unconscious processes. The exact nature of these processes remains obscure, but they clearly involve the application of ideas, conclusions, and questions derived from daily life and thought about current events to the professional work of archaeology.

THE PRESENT IN THE PAST: THE MAYA CASE

The analysis that follows is meant to be indicative rather than exhaustive. I have not pried into the private papers or unpublished thoughts of any Mayanists, but have instead depended on the writings which best present current "mainstream" interpretation and explanation to other archaeologists and the public.[5] Mainstream Maya archaeology is best represented in English by a

series of influential topical and synthetic volumes, which usually include papers presented at conferences (such as those at Cambridge University or the School of American Research). The participants include a mixture of older established authorities, a highly competitive middle-aged peer group involved in active fieldwork, and a few younger, ambitious researchers who are trying to establish their reputations. Papers in the major journals are also important but tend to be much less adventuresome and more oriented towards the presentation of data. It should be noted that these sources generally do not include the work of French- and Spanish-speaking Mesoamericanists, whose contributions are therefore not included in my analysis.

Maya archaeology is a particularly good field in which to study the influence of the present on the past, because Classic Maya culture is known entirely through archaeological rather than historical evidence. To be sure, ethnographic analogy plays some part, but most prehistorians have assumed a major disjunction to exist between the Maya of history and tradition and those of the Classic period, a barrier which conveniently corresponds with the "collapse" of Classic Maya society just before the earliest reliable ethnohistoric evidence. The reality or solidity of this barrier has always been a matter of some dispute.

In the early years, when little actual excavation had been done, the imagination could run riot, and images of the past tell more about the culture of the prehistorian than about the Maya. For the first half of this century, Maya archaeology was more a means of escaping the present than a reflection of it, and there are few direct parallels between current events and theories of the past. Rather, the past comes across as an antithesis of the present, as a model of how things could or should be in opposition to the way they are. Early views of the "Old Empire" as being ruled by theological lords of the jungle, a unique, peaceful, and artistic group holding sway by dint of their intellectual accomplishments (i.e., the ritual calendar), are clearly projections. They fit well with an early twentieth-century disillusionment with the lack of harmony and spiritual values in the industrial age, attributes which had supposedly been lost in the recent past.

Becker (1979) has published a particularly astute analysis of the Maya archaeology of the "middle period" from 1924 to 1945. He traces clear connections between upper-class anti-urbanism and J. Eric Thompson's highly influential model depicting Maya cities as empty "Ceremonial Centers" where only a religious elite resided (Becker 1979:10–12). Thompson (1927) also popularized the idea that the Classic Maya collapse was the result of class warfare, as the peasants overthrew an oppressive elite. Becker traces this theory to Thompson's class background and early experience on his family's Argentine estate and also to contemporary political events during Thompson's career. "The beginnings of Thompson's popular peasant revolt theory could have been the historical events taking place in modern, not ancient, Mexico" (Becker 1979:13).

When an ethnographer drew on experience with "untouched" Maya in the highlands to support the view that the ancient Maya were an egalitarian, agrarian, and nonurban society, building only religious monuments, the same kinds

of projection were operating (see Vogt 1961, 1964). We might ask why this model was so popular and lasted so long (see Sanders and Price 1968; Price 1974), even after its ethnographic basis was cast into doubt (Harris 1964:26–31). I think the answer is that the image of village democracy, of egalitarian, rural people managing their own affairs without the interference of political ideologies, was an important one in the age of the Peace Corps (founded 1961). Here was a model of democracy at the village level, a system which led to the construction of massive monuments and sophisticated art on a voluntary basis, without coercion, bureaucracy, class structure, or powerful leaders. Here, ancient history served as an antithesis to the present, an instructive example of how things could be.

The parallels between historical events and archaeological interpretation become more pronounced and direct during the late sixties, at the very time that "relevance" became an important concern of college students and teachers. Certainly the overriding historical event at this time, from the standpoint of the academic community, was the growing escalation of the war in Vietnam. And indeed, the ancient Maya also went through a period of militarization.

While the Bonampak murals depicting violent Maya conflicts had been known since 1946, they were not interpreted as evidence for widespread Maya warfare. Stelae portraying bound war captives under the feet of spear-wielding rulers were also ignored. Instead it was long believed that "The Maya . . . were one of the least warlike nations who ever existed" (Gann and Thompson 1931:63). Suddenly, in the late sixties interpretations began to change, and the militaristic aspects of Maya history assumed a new prominence. Fortifications were discovered at major sites; they had been walked over many times before but were never recognized before (see Puleston and Callender 1967; Webster 1972).

The first use of warfare to explain Maya prehistory in a systematic way was in 1964 in a paper entitled "The End of Classic Maya Culture" by George Cowgill. In the same year, Adams published an interpretation of ceramic evidence that led him to posit a foreign invasion of the Maya Lowlands just before the collapse. Was it a coincidence that this was the year of the Gulf of Tonkin Resolution, when American troop strength in Vietnam surpassed fifty thousand? As the war in Vietnam escalated, so did the number of papers which included warfare as a major element of Classic Maya history. Invasion by a foreign imperialist power from a more developed area was an accepted part of Classic Maya prehistory by 1967 (see Sabloff and Willey 1967), the year when U.S. troop strength in Vietnam reached a peak of half a million.

At first, warfare and invasion were implicated in the *collapse* of Maya civilization (see Thompson 1970; Sabloff and Willey 1967; Adams 1971) and were considered disruptive influences, symptoms of pathology. Shortly, however, conflict was elevated from a symptom of collapse to a general principle of cultural evolution, an essential part of the causal process in the *origin* of the Maya state (see Webster 1972, 1974, 1975, 1977; Adams 1977a). Again, foreign imperialist invaders with economic motives, this time from Teotihuacan, were

part of the process. Thus warfare was transformed from an aberration into a functional part of cultural evolution, a development which occurred elsewhere in anthropology as well (see Carneiro 1970; Chagnon 1967).

By the end of the 1960s and during the early 1970s, intellectual Americans became involved in a series of debates which were concerned with national and even global policy. Trigger (1981) links these debates, which he calls "middle class movements," to pessimism about the future and lack of confidence in technological progress. While the precise causes are likely more complex than he suggests, each of these debates was immediately reflected in thought about the Maya past. Opposition to the Vietnam War and a deep concern with the effects of militarization on contemporary society were certainly middle-class movements, and they are reflected in interpretations of prehistory.

While ecological protectionism arose as a national issue in the early sixties, the danger to America's natural environment did not become a pressing national (and political) concern until the late sixties, culminating with the establishment of the Environmental Protection Agency in 1970.[6] The peak of the movement came in the early seventies with the Endangered Species Act of 1973 and the Safe Drinking Water Act of 1974. Environmental issues became blended with those of energy, overpopulation, and resource scarcity after the oil embargo of 1973; no new significant environmental legislation was passed after 1974.[7]

Beginning around 1962 (see Cowgill 1962; Sanders 1962, 1963; Sanders and Price 1968), ecological and environmental causality came to Mesoamerican prehistory. In this early stage, environment was seen mainly as a limitation on cultural growth in a conventional cultural-ecological framework based on the ideas of Meggers (1954).[8] But as the idea of environmental destruction (rather than environmental limitation) became entrenched in the popular mind, the ancient Maya began to have more difficult relations with their rain forest habitat.

Sanders (1972, 1973) restated his earlier work more forcefully, claiming that agricultural overexploitation had led to environmental degradation through grass invasion and erosion. Despite the lack of material evidence, ecological catastrophe continued to be popular as at least a contributing factor in explaining the Maya collapse (e.g., Turner 1974; Harrison 1977; Rice 1978). The logical underpinnings of such arguments are made clear in statements such as this: "Following this [systems] model it is assumed that each sociocultural system seeks equilibrium or harmony with its environment" (Sharer 1977:541). Involved here are important philosophical and political issues of balance and harmony and the dire consequences of disrupting that balance.

Interest in environmental matters was expressed in other ways. Although supported by remarkably little hard evidence, papers on ancient Maya agriculture burgeoned in number, peaking in 1975–76 (perhaps related to the "back to the land" movement?). Others (e.g., Hosler, Sabloff, and Runge 1977) drew elaborate flow charts which showed how everything was related to everything else. The end of the Vietnam War and the rise of the environmental movement were paralleled in Maya archaeology by a shift from "external" to "internal" models of culture change (see Sharer 1977; compare Puleston and Puleston

1971 with Puleston 1979:70; Adams 1973 with Adams 1981; or Cowgill 1964 with Cowgill 1979).

Closely interwoven with themes of environmental disruption are those of runaway population growth, leading to stress on resources, social decay, and impending crisis. Given intellectual armor by Ehrlich (1968) and works like *The Limits to Growth* (Meadows et al. 1972), population increase became a common explanation for starvation and poverty around the world. At the same time, population growth became a significant explanatory variable in Maya prehistory. Again the emphasis was first on population growth as a danger, contributing to the instability of Maya society and eventually to its downfall (see several papers in Culbert 1973; Culbert 1974; and earlier works by Sanders). But population growth was quickly transformed, admittedly under the influence of Boserup's (1965) work, into the driving force of cultural evolution, responsible for the rise of Maya civilization (see papers in Adams 1977b and in Harrison and Turner 1978). Despite a general lack of data to support these hypotheses (see Cowgill 1975), for a brief period they achieved the status of a universal explanation.

In passing I should mention another human-environmental interaction which achieved prominence during the late 1960s (especially on college campuses): widespread use of hallucinogenic and euphoric drugs. Ancient Mesoamerica quickly produced its own literature on the subject, including discussions of the importance of hallucinogens in the art and iconography of ancient cultures (see Furst 1970, 1972; Dobkin de Rios 1974).

The stirrings of the women's movement also had a brief impact on the ancient Maya, as Molloy and Rathje (1974) proposed that "sexploitation" was a part of the Classic political system. It has not been until much more recently, however, that studies of women in Maya society have become more common (see Pohl and Feldman 1982; Nimis 1982). A real feminist critique of Maya archaeology has yet to be published however.[9]

It is interesting that the late 1970s, an uncertain time in American politics, was also an uncertain time in archaeology. An "empirical revival" of sorts seems to have occurred. While new discoveries were being made, especially in the fields of Maya origins and agricultural production, no clear, new explanatory trends developed. The Hammond and Willey volume of 1979 is remarkably free of the ecological and population pressure models of earlier years. But already in this volume were the seeds of future developments, in a paper on the Maya collapse by Dennis Puleston (1979). He suggested that religious prophesies forecasting the doom of Maya society actually had a strong influence on pushing the society to its destruction. One can almost visualize the Maya priests scurrying around taking survivalist courses and reading up on how to prosper during the coming Postclassic years.

Puleston's paper was both a sensitive reaction to contemporary changes in the American political and social environment and a harbinger of things to come. With the growing conservatism and the increasing power of fundamentalist religious movements in American society, religion began to figure more prom-

inently in prehistory as well (see, e.g., Marcus 1978; Coe 1981). Freidel (1979, 1981a, 1981b) argued that ideology, particularly the religious ideology of the politically powerful, was far more important in shaping Classic society than was population growth or pressure on resources. Indeed, a spate of anti-ecological models has spread all over Mesoamerica, each stressing the importance of political and religious ideology over economic maximization (see, e.g., Brumfiel 1983; Blanton 1980; Kowalewski 1980; Freidel and Scarborough 1982). The study of elites and elite culture became respectable once again, after years of emphasis on the common folk, and the tracing of kingly lineages became increasingly popular (see, e.g., Haviland 1981; Adams and Smith 1981).

To religious fundamentalism, the "New Right" political agenda of the late 1970s and early 1980s adds an emphasis on the family as a basic building block of society and a belief that "big government" is responsible for America's economic decline. Each of these auxiliary themes is reflected in recent explanations of the Maya past. It is remarkable just how little interest Mayanists have shown in Maya family and household organization through the years, but this has changed recently. I (Wilk and Rathje 1982) am guilty of pushing the household and family as important units in understanding Maya prehistory but had little difficulty finding others to participate in a symposium on "Mesoamerican Houses and Households" co-organized with Wendy Ashmore at the 1983 meetings of the Society for American Archaeology (soon to be an edited volume).

Furthermore, the current idea that big government and the expense of supporting it are a burden on the populace seems to be reflected in recent work on the origin and demise of the Maya state. Where, previously, political elites were considered functional (contributing to the maintenance of the system), now they appear as pernicious growths, maintaining themselves at the expense of the body politic through force (see Haas 1981). The "peasants-rebelling-against-the-burden-of-elite" argument for the collapse of Maya society has been revived (see Hamblin and Pitcher 1980). Cowgill (1979:62) hypothesizes that the Maya collapse came about when the elite drove the system into the ground in their efforts to expand the size and scope of the state. Hosler, Sabloff, and Runge (1977:560) blame the collapse on "inadequacy of bureaucratic technology." How long will it be before the Maya collapse is interpreted as an attempt to "get government off the backs" of the ancient Maya, perhaps accompanied by a tax rebellion?

Figure 1 summarizes the close correspondence between current events and explanations in Maya archaeology. I have taken seven major edited volumes of papers by prominent Mayanists and have placed them on a time line according to when the papers were presented at symposia or submitted for publication, rather than when they were actually published. The papers in each volume were then placed in categories according to explanatory content. A single paper was allowed to count in several categories if it was judged to deal with each in a substantive way.[10] The volumes are close to a standardized sample, as

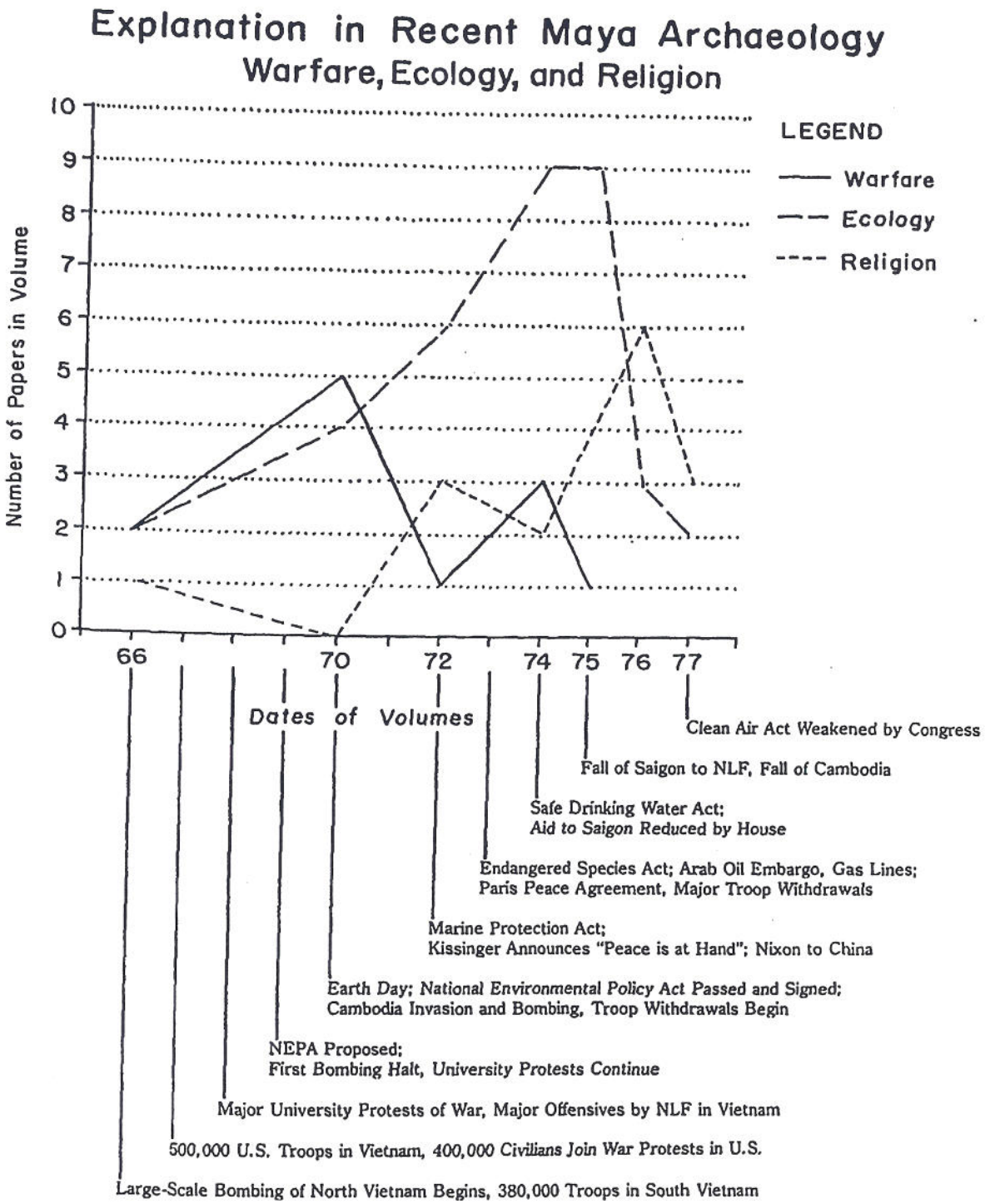

Figure 1. Explanation in Recent Maya Archaeology: Warfare, Ecology, and Religion

they tend to draw on the same small community of scholars, and the editors in each case tried to present current views and "hot" topics.

The trends are clear and correspondences are striking. Most important is the sequencing of peaks of interest, first in warfare, then in ecology, and lastly in religion. Also evident by the late 1970s is an unexplained drop in the number of explanatory papers and a return to earlier interests in culture history, architecture, art, and ceramics. Does this mean that the period of correspondence between Maya archaeology and current events is over? Or is there instead a shift to new topics and explanations which are difficult to establish or understand through lack of perspective? It does seem a general rule that it becomes more difficult to pick out trends as we get closer to the present.

SOME TENTATIVE CONCLUSIONS

I do not intend to prove that *every* explanation offered by Mayanists has its ultimate source on the pages of *Time* magazine; many do arise from elsewhere in the profession, from anthropology, and through genuine original thought. On the other hand, almost every trend of importance in recent United States history finds some reflection, sometimes after a lag of a few years, in learned analyses of the rise and fall of ancient Maya civilization.

But does the existence of this relationship mean that the field is methodologically bankrupt? Is it true that "the past is an empty stage to be filled with actors and actions dictated by our means and desires" (Fritz 1973:76) and that all explanations are therefore open to attack as projections (or at best collective representations)? Certainly, Binford and Sabloff (1982) seem to think that such arguments are attacks on the "rationality" of the field. They respond by emphasizing the importance of regional traditions of anthropology and of the paradigms of culture which guide archaeological research and by arguing for "middle-range" studies as solutions to the limitations of world view.

Several sociological aspects of the archaeological profession promote reflexivity and hinder objectivity. The rewards of the field, prestige and position, go to those who propose new explanations, who have intriguing and relevant hypotheses, and not to those who slowly and ploddingly test those hypotheses (Flannery 1982). As competition for positions and research funding increases, we can therefore expect more pressure for new explanations; and there are a limited number of sources for these. Scholars can hardly be blamed for looking to modern America (even if unconsciously) for inspiration.

Furthermore, most of the explanations and hypotheses being proposed are not subject to disproof with present techniques and knowledge, considering the general lack of bridging arguments and middle-range theory (Ascher 1961; Binford and Sabloff 1982). Explanation can therefore accumulate much faster than it can be evaluated; in the absence of disproof we have only disapproval. The stage is ideally set for trendy scenarios of the past, evaluated on the basis of what sounds good; and that is likely to be something which relates directly to the commonsense, everyday experience of the reader. This may be why

recent archaeology has depended on quite mundane models of past peoples, forgetting just how exotic and bizarre the ethnographic record can be. Explanations have to appeal to common experience.

That is perhaps the "dark side" of the picture I have painted, one which certainly alarms some of the leaders of our field (Flannery 1982; Binford and Sabloff 1982). As I said in the introduction to this paper, the changeability of the past in the hands of archaeologists can also lead to cynicism and disillusion on the part of lay people and academics in other fields. But there is another side to the matter, for it is the very fact that the past is to a certain extent a reflection of the present that makes the past so fascinating. If the past bore no relation to the present, and it can certainly be argued that the connection between the ancient Maya and modern Americans is tenuous (though Rathje [1982] disagrees), it would be dreadfully boring, even to archaeologists. By their commitment to studying the past as a profession, archaeologists affirm that there *is* a connection between past and present and that it is important and relevant. They tend to believe that the appreciation of the past has a positive social role to play in the present.

Instead of being an escape from the present, the past today serves specific purposes, in a social and political sense. The purposes can be generally lumped into "past as charter" and "past as bad example." In the first, the past, as a reflection of the present, serves to legitimize present courses of action or circumstances, much as the Old Testament is used by the state of Israel. In the second, the past is also a reflection of the present but serves as a source of moral or pragmatic lessons showing why a present policy, action, or trend is wrong or deleterious. In *both* cases, the connection between past and present must be shown before the lesson can be drawn. Is it any wonder that archaeologists participate in the process by drawing their hypotheses and explanations from the present?

There is no reason for archaeologists to be defensive about explicitly or implicitly drawing on their personal, cultural, and political experience in their professional work. Like Hodder (1985), I think we should drop the pretense of absolute objectivity. Further, I suggest that drawing on present experience and interests is hardly "unscientific" and that it strengthens, rather than weakens, our work. The connection between present and past is a source of power, the power to offer legitimacy or attack it. Archaeologists have no monopoly on this power (though they do tend to resent others who intrude on their control of the past), but they do have a strong claim to it. Rather than condemning those who "pervert" the past to their own political purposes (Ford 1973), we should acknowledge that there is *no* neutral, value-free, or nonpolitical past—that if we take the present out of the past we are left with a dry, empty husk. The challenge is to be aware of the weight of the task and to take responsibility for the power inherent in interpreting the past. Let us not forget Orwell's epigram from *Nineteen Eighty-Four:* "Who controls the past, controls the future: who controls the present, controls the past (Orwell 1949:251).

NOTES

1. A version of this paper was presented at the 1983 meetings of the American Anthropological Association in Chicago. Many of the ideas presented here were developed during conversations with Diane Gifford, though they do not necessarily reflect her opinions. I want to thank Hal Wilhite, Orvar Löfgren, Robert Netting, Cheryl Claasen, Warren DeBoer, Anne Pyburn, Matt Cartmill, David Freidel, Bill Rathje, Michael Schiffer, and three anonymous reviewers who read and offered useful comments on various drafts of this paper. I particularly appreciate Freidel's support and interest, though he is far from agreement with the contents of the paper.

2. This perception is not limited entirely to laymen and students. The lead article in archaeology's major journal recently suggested that explanation in Maya archaeology has followed a circular pattern, with old ideas being rejected, allowed to rest, and then recycled, through ignorance and blind reaction against predecessors' theories (Marcus 1983).

3. It is remarkable that Salmon's recent (1982) study of archaeology from the perspective of the philosophy of science is almost devoid of any discussion about the origins of hypotheses. Apparently the issue is philosophically trivial, whatever its historical importance.

4. The lineal transmission of ideas from teacher to student seems to be the prevailing folk model among archaeologists. A rival model sees a source of innovation in the rebellion of students against their teachers (Binford 1972).

5. It would indeed be a fascinating study to look at the papers in Maya studies which have been rejected by the major journals or which were never even submitted by authors. It would also be interesting to contrast the content of the writings of North American Mesoamericanists with Mexican and French work to see if the different cultural backgrounds affect the explanatory content.

6. Rachel Carson's *Silent Spring* was published in 1962.

7. After 1975, in fact, most new legislation weakened existing environmental law. The Clean Air Act amendments of 1977 weakened standards and controls established in 1970.

8. A much earlier period had seen a good deal of descriptive work on the Maya rain forest environment and Maya agriculture (e.g., Lundell 1934, 1937; Roys 1931). This work seems largely devoted to showing that the Maya environment wasn't quite as hostile as it appears and that it was quite capable of supporting a civilization, a position that was later attacked by Meggers. There is no space in this paper to discuss how this whole environmental limitation argument is related to the great highland/lowland division between archaeologists in Mesoamerica.

9. An earlier paper on women in Maya art by Proskouriakoff (1964) merely points out that women did indeed exist in Classic times, a fact that had been mostly overlooked until then.

10. For the purposes of Figure 1, the categories have been condensed. Invasion and warfare are conflated, as are agriculture and population growth in the category of ecology. In each volume the majority of papers were concerned with the "three C's" (ceramics, chronology, culture history), while a minority (not tabulated) dealt with sundry topics like art history and trade. Tabulations for the 1966 papers (Bullard 1970) are somewhat more tentative than for later volumes. Because of the overwhelming culture-historical orientation of that volume, it contained only slight discussion of warfare, ecology, or religion. The dates for environmental events are mainly from Vig and

Craft (1984), while the Vietnam War was covered in Isaacs (1983) and Amter (1984). The volumes' proper references, the number of substantive papers in each (excluding introductions and summary papers), and the date the papers were presented or submitted for publication are as follows: Bullard 1970, 10 papers, submitted 1966; Culbert 1973, 15 papers, conference 1970; Hammond 1974, 14 papers, conference 1972; Adams 1977b, 13 papers, conference 1974; Hammond 1977, 22 papers, submitted 1975; Hammond and Willey 1979, 13 papers, conference 1976; Ashmore 1981, 13 papers, conference 1977.

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