It is argued that "behavioral archaeology," as it has been advocated by M.B. Schiffer, represents a series of partially misguided postures relative to some important points of debate common in the 1960s. Due to failures to understand some points at issue, behavioral archaeologists appear to embrace many of the methods and goals of traditional archaeology, and thereby present a "reactionary" position rather than a "revisionist" position, as it has claimed. The issues upon which behavioral archaeology appears most reactionary are points most urgently in need of change, if archaeology is to progress as a discipline contributing both to our knowledge of the past and to our understanding of historical trends.

WALTER TAYLOR'S (1948) CALL for more attention to "context" and "affinities" stimulated a healthy series of discussions concerning "interpretation" in archaeology. In general, however, archaeologists reacted with skepticism to Taylor's slate of interpretive goals. Most considered the achievement of the "conjunctive" approach to be primarily conditioned by the preservation manifest in the archaeological record. Stated another way, most archaeologists of the time were strict inductivists, and considered that one's ability to construct "cultural" contexts was a function of the relative preservation of associations with clear "indicative" properties. In short, the degree to which the archaeological record preserved what were perceived as clear and directly "meaningful" associations and patterned structure was seen as the factor limiting the attainment of Walter Taylor's goals for archaeology. Skepticism born of an inductivist's approach was voiced by Hawkes (1954) and supported in the writings of many, including Robert Ascher (1962:368), who stated:

The foregoing example is designed as a paradigm to illustrate how observations on ongoing cultures might be adapted to the problems of archaeological reconstruction . . . Unlike endeavors which exclusively emphasize those aspects of culture which archaeologists cannot learn about [e.g. Thompson 1939], it directs attention toward the archaeologically possible (emphasis added).

Ascher's reference to Thompson (1939) is to the provocative paper in which he discusses seasonal behavioral changes which he believes would mislead many archaeologists, given then current interpretative conventions:

. . . the camps and the house types, the weapons and the utensils are of a specialized type and related to the seasonal life, so that viewing these independently at different periods of the year, and seeing the people engaged in occupations so diverse, an onlooker might be pardoned for concluding that they were different peoples (1939:209).
This exchange is typical of the time. A claim is made regarding the nature of living systems, with clear implications for the patterning found in the archaeological record. The archaeologist replies that the cited characteristics are aspects of culture “which archaeologists cannot learn about” (Ascher 1962:368).

It was in the context of further elaborating this skeptical point about our ability to reconstruct the past that Robert Ascher began developing a point of great interest—I have in mind his very early and in many ways pioneering ethnoarchaeological studies (Ascher 1962, 1968), and his discussions of what Schiffer (1972) would later term “formation processes”:

Every living community is in the process of continuous change with respect to the materials which it utilizes. At any point in its existence some proportion of materials are falling into disuse and decomposing, while new materials are being added as replacement. In a certain sense a part of every community is becoming, but is not yet, archaeological data . . . (Ascher 1961:324)

Ascher’s arguments regarding formation processes were extremely insightful. He argued that the archaeological record is some combination of interruptions in an entropy-linked process, and that it is therefore limited in its information potential. He cited the disorganizing effect of the operation of past systems, as well as the disorganizing processes associated with subsequent modifications of the archaeological record, as the sources of “distortion” in the archaeological record as seen by an archaeologist. In short, as he put it, what the archaeologist digs up is not “the remains of a once living community stopped, as it were, at a point in time”; such an “erroneous notion, often implicit in archaeological literature, might be called the Pompeii premise” (Ascher 1961:324). Ascher argued rather that the archaeological record is the disorganized arrangement of matter regularly generated after the point in time which would be of interest to a “reconstructionist.” Ascher was attempting to force archaeologists to realize that the archaeological record is ravaged by “time’s arrow,” and should be treated as such, rather than as a preserved “past.” In this important point I agreed with Ascher.

On the other hand, the implications of this empirical point as it reflects upon the aims and goals of archaeology was a point of contention between Ascher and myself. In his view, the archaeological record, viewed from a strict inductivist position, does not generally contain information regarding “cultural context,” “social behavior,” and other aspects of interest to those who seek to understand the past as it existed. His reasoning was simply that since the Pompeii premise was erroneous, then most reconstructionist goals, as advocated by Taylor (1948) and others, were equally unrealistic. In his view, we should restrict our discussions to subjects which are archaeologically possible.1

From this perspective, it follows that those who advocate the discussion of “cultural context” are really only advocating speculation and “theorizing”:

One reason for archaeology’s continued lag as a contributor to culture theory . . . is that we continue to see our main goal as the reconstruction of ancient ethnology. Since we have so great a handicap to begin with here, where speculation and inference mix too confusingly . . . I think we are overlooking another order of interpretation . . . one can manipulate the artifacts statistically without much concern whether one understands precisely what they originally were, exactly how they were used, and just what they meant to the ancients” (Wauchope 1966:19).
Clearly what was advocated was a "realistic" appraisal of the "limitations of the archaeological record," with the understanding that this would guide us to an appreciation of "realistic" goals for archaeological research.

My approach to the situation was different, and took exception to both points of view being discussed; I disagreed with Taylor's reconstructionist goals, as well as with inductivist skepticism regarding our ability to learn about the past.

I have never viewed the reconstruction of prehistoric lifeways in the form of prehistoric ethnographies to be an appropriate goal for archaeology in general. It has been clear to me that the time frame of ethnography is largely inappropriate for archaeological research. Rates of deposition are much slower than the rapid sequencing of events which characterizes the daily lives of living peoples; even under the best of circumstances, the archaeological record represents a massive palimpsest of derivatives from many separate episodes. Any structure and repetitive patterns of association and covariation must derive from the operation of "systemic events," or dynamics, with a much longer term, more rigidly determined organization than is true of those observed in the lives of persons and groups which embody the ethnographers' perception of time and human systems. My view was that we should be seeking to understand cultural systems, in terms of organizational properties, and in turn, to explain differences and similarities among these cultural systems, rather than to generate set pieces of descriptive history. This means, however, that those "things" of interest from the past are organizational properties, which cannot be dug up directly. Stated another way:

Granted we cannot excavate a kinship terminology or a philosophy, but we can and do excavate the material items which functioned together with these more behavioral elements within the appropriate cultural subsystems (Binford 1962:218-19).

In this view, a strict inductivist approach to inference is clearly impossible, since we need to have an understanding as to how living systems functioned in order to make inferences from their static by-products; in short, we need to link our observations of the archaeological record to an understanding of systems dynamics, in order to make reliable inferences about the past. The way in which I diagnosed the situation regarding Taylor and his critics was that while I did not necessarily agree with Taylor's goals, the relevance of his critics' arguments depended on a strict inductivist approach to inference.

From the beginning of my professional career, I have considered the archaeological record to be an ordered consequence of levels of adaptive organization which are difficult to appreciate directly through the observation of events and episodes in the "quick time" perspective of the ethnographer or the participant in a cultural system. Nor have I been surprised or alarmed to find that the archaeological record does not generally carry the intuitively obvious information regarding the "quick time" events and "human episodes" which one would expect from an ethnographer or a preserved Pompeii. I have considered it necessary for archaeologists to investigate the archaeological record as a different order of reality, the patterned structure of which represents not a simple accumulation of little events, but rather some of the basic organizational constraints and determinants operating on the events or episodes of daily living. The archaeological record is therefore not a poor or distorted manifestation of ethnographic "reality," but most likely a structured consequence of the
operation of a level of organization difficult, if not impossible, for an ethnographer to observe directly. This level of organization is in turn likely to be the unit upon which ecological-evolutionary selection operates, rather than the level of the specific event.

Consistent with such a viewpoint, I criticized the idealized view of culture (Binford 1962), and suggested that the meanings of artifacts derive from their function in living systems, not from some manufacturer’s “ideas” as translated by our classifications for things that archaeologists have found. More directly, my view was that the “Pompeii premise” is important only if one adopts a strict inductivist approach to the archaeological record, expecting to uncover archaeological “facts” with self-evident meaning for the past. I reasoned that if our methods of inference were so faulty that the “realities” of an organized system as reported by Donald Thompson (1939) had to be considered something that “archaeologists cannot learn about” (Ascher 1962:368), then clearly we needed a change in our methods.

In the context of arguing that cultural systems are internally differentiated (Binford 1962), rather than internally homogeneous (the idea threatened by Thompson’s descriptions), and that such organizational differences would be manifested in the archaeological record, I wrote (Binford 1964:425) that:

The loss, breakage, and abandonment of implements and facilities at different locations, where groups of variable structure performed different tasks, leaves a ‘fossil’ record of the actual operation of an extinct society.

Some years later (Binford 1968a:23) I addressed quite directly the problem of the “limitations of the archaeological record,” and amplified my views on its information potential:

The practical limitations on our knowledge of the past are not inherent in the nature of the archaeological record; the limitations lie in our methodological naivete, in our lack of principles determining the relevance of archaeological remains to propositions regarding processes and events of the past.

The degree of preservation of the archaeological record has never been considered limiting in a practical sense, since it has also been maintained that “facts do not speak for themselves,” and that all arguments from the archaeological record as to the character of the past are inferences. These in turn are only as good as our understanding of the linkages between dynamics and statics, the causes in the past and the consequences remaining for us to observe archaeologically.

The reader cannot imagine my surprise when Michael Schiffer (1976a) years later announced to the world his discovery that the “new archaeology” had been laboring under a false principle—the Pompeii premise!

The early years of the new archaeology witnessed the frequent and unquestioning repetition of major methodological principles. One such principle was enunciated by Binford (1964:425) in perhaps its most explicit form:

The loss, breakage, and abandonment of implements and facilities at different locations, where groups of variable structure performed different tasks, leaves a ‘fossil’ record of the actual operation of an extinct society.

As often happens . . . few investigators have noticed that the principle is false. It is false because archaeological remains are not in any sense a fossilized cultural system. Between the time artifacts were manufactured and used in the past and the time these same objects are unearthed by the archaeologist, they have been subjected to a series of cultural and noncultural processes which have transformed them spatially, quantitatively, formally and relationally . . . If we desire to reconstruct the past from archaeological remains, these processes
must be taken into account, and a more generally applicable methodological principle substituted for the one that asserts that there is an equivalence between a past cultural system and its archaeological record. The principle I offer is that archaeological remains are a distorted reflection of a past behavioral system (Schiffer 1976a:11-12).

This is exactly the point which Ascher had made years earlier; namely, that the archaeological record is a distortion of the Pompeii that he claimed we thought we were finding. Schiffer (1976a:14) describes his Pompeii premise as follows:

If the human participants and all other energy sources completely halt their actions, the activities cease, as does the operation of the behavioral system, what remains (assuming no modification by other processes) is the closest conceivable approximation of a 'fossil' of a cultural system—its material elements in a system-relevant spatial matrix.

Ascher had discredited the Pompeii premise as a means of discrediting the reconstructionist goals of Walter Taylor. I had rejected these same goals as unrealistic and wasteful relative to the processes which were likely to be responsible for the archaeological record, i.e., the long-term structure and evolutionary dynamics of a cultural system. In turn, Ascher's arguments against Taylor were beside the point.

Archaeological theory consists of propositions and assumptions regarding the archaeological record itself—its origins, its sources of variability, the determinants of differences and similarities in the formal, spatial, and temporal characteristics of artifacts and features and their interrelationships. It is in the context of this theory that archaeological methods and techniques are developed (Binford 1968b:2).

When Schiffer, years later, claimed that my approaches presuppose the Pompeii premise, my initial response was to ignore his misunderstanding. However, I have come to realize that instead of minor misunderstandings, there are substantive points of disagreement between Schiffer and myself.

WAS DISTORTION GOING ON AT POMPEII?

Schiffer proposed that in order to grasp fully the inferential challenges facing the archaeologist, we have to understand in some detail the sources of, or causes for, the properties remaining in the archaeological record. In this I agree. He further proposed that we must always keep clearly in mind the difference between systemic context, or the dynamics within which matter “participated” in the past, and archaeological context, or the static form in which we find culturally generated matter in the archaeological record; with this I also agree. According to Schiffer it is the “transformation” of matter from the systemic context into the archaeological context that should receive our focused attention. He called these transformational conditions formation processes, and recognized two basic types: C-transforms, or cultural formation processes, and N-transforms, or noncultural formation processes. The question now is why does Schiffer consider the archaeological record distorted?

I am quite comfortable with the idea that there may be natural, or noncultural, processes which condition the character of the archaeological record so as to “distort” the organization as it was generated in the “systemic” context. I am somewhat at a loss, however, to understand why events which modified the formal properties of matter during the operation of a cultural system should be considered distorting.

The principle I offer is that archaeological remains are a distorted reflection of a past behavioral system. However, because the cultural and non-cultural processes responsible for these distortions are regular, there are systematic (but seldom direct) relationships between archaeological remains and past cultural systems (Schiffer 1976a:12; emphasis added).
It is clear from Schiffer’s statement that he considers C-transforms potentially distorting. From my perspective, archaeologists must understand formation processes—the dynamics of cultural systems which yield derivative residues both in properties of form and spatial distribution—while from Schiffer’s perspective, archaeologists must identify distortions between the “systemic” and the “archaeological” contexts.

For instance, Schiffer would view the event of a young boy cleaning out a hearth, taking the ash and other unwanted contents out of the house, and tossing it to one side as a C-transform distorting the juxtaposition of hearth and ashes which obtained during the period of active fire in the hearth. From the perspective of the occupants of the site, it was cleaning up. We may reasonably ask how cleaning up should distort the relationship between the archaeological record and the cultural system from which it derives; some might even argue that cleaning up was essential for the continued use of the location. Similarly, one might argue that adding fuel to the fire was essential to maintaining the fire, but alas, it was also “distorting” the prior relationship between the hearth and the adjacent pile of firewood.

Thus, I totally disagree with Schiffer’s idea that the archaeological record is a distortion of a past cultural system. Such a position could only be true if the archaeological record as produced in the past were destroyed or modified by post-depositional events. The archaeological record is a normal consequence of the operation of living systems, all of which are dynamic, “flow through” systems, in which energy is captured and its potential reduced. Entropy is the inevitable product of a living system, and it is generated continuously. The archaeological record must therefore be viewed as matter transposed and organized during the process of energy use and entropy production. It is the functional linkages between the organization of a system and its energy-capturing tactics, together with its patterned residues (entropy), that yield information about the organization of past systems.

Our inferences from the archaeological record to the past may be wrong or unjustified, not because the archaeological record is a distortion of the past, but because we do not accurately understand the relationship between statics and dynamics. The archaeological record can only be considered a distortion relative to some a priori set of expectations; certainly it is not a distortion of its own reality. It is a faithful remnant of the causal conditions operative in the past, and our task is to understand those causal conditions. Put another way, a pattern or arrangement among artifacts at an archaeological site can only be viewed as distorted if one is not interested in the cultural system as manifest, but rather in some property of a cultural system chosen a priori to receive special inferential attention. Cleaning up is distorting only if it destroys some patterned association in which one might have a research interest.

PARADIGM CONFLICT: DESCRIBING PAST CULTURAL SYSTEMS
VERSUS RECONSTRUCTING THE PAST

I have argued that the idea of the archaeological record as “distorted” by C-transforms makes sense only if we imagine a set of past conditions which would be “optimal,” or ideal, for us to find. The archaeological record is not distorted with respect to its own reality, but only relative to an archaeologist’s preexisting expectations. The latter will of course be derived from a specific paradigm, or what we expect the world to be like (see Binford 1981).
What would Schiffer like to find? In the answer to this question rests the clue
to the really major differences between our views of the archaeologist’s challenge.
Schiffer wants to find Pompeii.

Let us begin by visualizing an ongoing cultural system. . . . What one pictures is a system of
action. . . . If the human participants and all other energy sources completely halt their actions,
the activities cease, as does the operation of the behavioral system, what remains (assuming no
modifications by other processes) is the closest conceivable approximation to a ‘fossil’ of a
cultural system . . . (Schiffer 1976a: 12-13)

What Schiffer has described is as close as one can come to the ethnographer’s
notion of the “ethnographic present”—a synchronic cross-section of all the events
or actions transpiring at a given time. It is not a picture of a cultural system, but a
slice of “history” in the literal sense of the word. A cultural system is not a summa-
tion of all the events, behaviors, or other transactional phenomena going on at a
given time, but rather the conditioning organizational framework within which all
these events transpire. This organizational framework includes all the places, things,
and characteristic social relationships and intellectual conventions in terms of which
events are played out. It is true that a cultural system has dynamic properties; it
is not true that it is completely dynamic and transitory, as is implied by Schiffer’s
writings.

Given Schiffer’s view of a cultural system, placing trash in a dump is a C-trans-
form; as such, it is a distorting action, marking the transition between systemic
context and archaeological context for the items discarded. They were in fact
“used” at another place, at a prior time, and as such the presence of the item in
the dump is a distortion of such a prior state. But is it also true that it is a distortion
of the cultural system? Only if the cultural system is equated with specific actions
or the “context of use” for the items in question. However, if one considers the
maintenance of life space part of the cultural system, the act of cleaning up and
disposal itself as a part of the cultural system, even the presence of a dump as a
component of the cultural system, how are things distorted? Answer: they are not distorted. What is distorted is Schiffer’s view of a cultural system, and his ideas
as to how to understand one.

The history of anthropology follows research oriented toward offering some
answer to the question of why different “peoples” consistently behave differently;
in short, why their behaviors are so frequently variable in the face of seemingly
similar stimuli. Early answers to this question cited “race” differences as the causes
of behavioral differences; to counter this position anthropologists pointed to “cul-
ture” as the cause of patterned, long-lasting behavioral differences among “peoples.”
In this context there was a conflict between those who saw differences in culture
as referable to differences in individual psychology, which then would feed back
to reinforce such populational differences, and those who sought the explanation
for different cultures not from the properties of the participants, but from the
ecological history of the system itself, seen in interaction with its environment. The
psychologically oriented view sees cultural systems as abstractions or generalizations
of all the specific behaviors considered “cultural” in character. Change reflected
through such a normative view is the result of microfactors, which push and pull
the frequencies with which specific behaviors are executed. 4 In turn, change is seen
in an almost Lamarkian framework, where

“most changes result from attempts to cope with the unanticipated consequences of previous decisions” (Schiffer 1979b:366).

I wish to understand in organizational terms the frameworks within which behaviors were carried out, the structural properties of systems within which the dynamic functioning we see as behavior occurred. In turn, I wish to understand in Darwinian terms the pressures on such systems, which result in their structural change, diversification, and evolution.5

Consistent with this contrast between the systems and the interactionist views, Reid and Schiffer state (1973:2) that:

It seems appropriate that archaeologists recognize that culture is knowledge—a system of learned information and the rules for processing and transforming that information into action or behavior. Strictly speaking culture is not behavior.

Here we see another fundamental point of disagreement between processualists and Schiffer and his colleagues. For the latter, not only is a cultural system considered to be behavioral in the literal sense of the word, but culture itself is considered to be mental phenomena—a system of learned information. Culture is then manifest in things and behavior, but is not a material phenomenon itself. This is traditional archaeology. Culture provided a mentalist explanation for variability in behavior and manufactured items for the traditionalist; it is again cast in that role in Schiffer’s thought. The only difference is that culture is extended to include more than mental templates for producing things, to include plans for “living,” manifested behaviorally and transactionally.

Ironically, instead of viewing his ideas as conflicting with those advocated by myself and others in the 1960s, Schiffer saw them contributing to harmony in the field:

Although the debates of the past decade have resulted in significant advances in archaeological method and theory, they have also led to a fragmentation of the discipline and to overt and covert hostility between rival sub-paradigms and between traditional and processual archaeologists. Thus, before we treat some of the important issues that contribute to the revisionist version of processual archaeology, we offer a scheme for reintegrating the divergent aims and interests of all archaeologists (Reid and Schiffer 1973:2-3).

The intellectual peacemaking which Reid and Schiffer were engaged in would have constituted an intellectual capitulation of processualist views to major components of the traditionalist position. This is neither compromise nor evaluative scientific judgment; it is simple paradigmatic bias.

In addition, there are real contrasts between myself and Schiffer regarding the characteristics of evolution—Darwinian versus Lamarkian—and hence the appropriate types of facts needed to understand cultural change. There are paradigmatic differences regarding the concept of culture, and there are also major contrasts in our views on how to do science: Schiffer is primarily an inductivist, seeking to elevate empirical generalizations to the status of “laws,” and worrying about the probabilistic “value” of such propositions. Nevertheless, we agree on the basic need for increasing the power of our inferential strategies, and that this depends on giving priority attention to understanding the archaeological record. Since his major work has been in this latter area, we may justifiably ask if his call for a “revisionist” new archaeology should be taken seriously. Has Schiffer illustrated any methodological gains which would back his call for a “revisionist” perspective?
In Schiffer's model of site formation there are three modes of deposition, resulting in primary refuse, secondary refuse, and defacto refuse. Matter discarded or dropped in the immediate context of use is primary refuse; secondary refuse is matter which has been moved from its primary location, generally through maintenance or clean-up operations; and defacto refuse consists of items lost or abandoned during the abandonment of a site. By definition, defacto refuse is usable items occurring in the context of "late" or terminal use of the site. We may ask how Schiffer would distinguish defacto refuse, deriving from the abandonment by sedentary residents, from site furniture (see Binford 1979), maintained and regularly used in the residentially abandoned rooms by hunting parties or other logistically organized groups who use the "ruin" as a camping spot. The ruin would, of course, offer shelter and raw materials, and water would necessarily have been nearby, since it was once a residential site. The answer seems to be that Schiffer could not recognize such differences, since he never included the successional alternative in his "paradigm" of possibilities for interpreting pueblo "formation processes."

In this regard it is interesting that in studying debris associated with what Schiffer considers to be deposits accumulated at the Joint Site during the main occupation, as contrasted with associations developed during the "abandonment," he finds that "the flow rate of chalcedony increases by a factor of two during the abandonment period (Schiffer 1976a:170). Schiffer rightfully seeks an understanding of this interesting observation, by adopting a form of argument from elimination (see Binford 1981:83), proposing four "hypotheses" and seeking to "test" each of them. After rejecting three, he is left with hypothesis number four, namely, that "chalcedony was used for the same tasks, but the rate of task performance increased" (Schiffer 1976a:179). He then suggests that a biased use of chalcedony is most likely to have arisen in hunting. In evaluating this suggestion Schiffer (1976a:175) argues that:

The ratio of food species to total chipped stone tools should increase appreciably between the main occupation and abandonment periods. In Table 12.6 the ratio of the total number of identifiable bones (of the five most common food species) to the total number of stone tools is presented for the two periods. Quite clearly, the ratio increases by a factor of at least 10 for all species.

Schiffer's (1976a:177) "historical" scenario is as follows:

It seems as though the environmental conditions conducive to subsistence agriculture in the Hay Hollow Valley deteriorated considerably during the second half of the thirteenth century. In response to the stresses created by a high rate of crop failure or by reductions in available agricultural products, the inhabitants of the Joint Site diversified their procurement strategies to include more species of wild foods, and a greater dependence on the wild foods ordinarily consumed. The heavier emphasis on hunting brought with it a greater demand for arrow points and other hunting and butchering tools. Because the increased rate of hunting by groups of males resulted in more trips to the vicinity of Point of the Mountain, chalcedony, a material favored for manufacture of arrow points, was retrieved more frequently . . .

We have here a fine example of a set of untested conventions used as a method of inference. Given the use of a limited set of formation conventions, we are lead to a set of conclusions about the past which, at least in my view, are truly bizarre.

We may reasonably ask: (1) If game density was sufficient to support the local group in the immediate area, why did they practice agriculture in the first place? (2) Wild resources are normally increasingly depleted in the immediate area of a
sedentary site as a function of increasing occupational duration. If this was the case here, how were the people able to compensate for "crop failure" at the end of their occupation by shifting to the exploitation of wild foods? (3) In general, the speed of game depletion is inversely related to body size; larger body sized animals are overexploited first, so that there is an increasing trend toward animals of smaller body size during the tenure of a sedentary occupation. We note that there is a relative increase in mule deer relative to antelope between the "main occupation" and the "abandonment phase" assemblages. This proportional shift is also true with respect to the smaller animals; there is an absolute increase in mule deer relative to other animals. I find this pattern almost impossible to imagine as characteristic of the closing phases of a permanent settlement. On the other hand, it is perfectly consistent with the post-abandonment utilization of the ruin by hunting parties operating out of another residential base. Presumably, some years after abandonment, the local flora and fauna would recover, and the site of the old sedentary village would make a perfect camping spot for hunting parties exploiting what may well have been a temporary, localized abundance of game. This abundance would be due to the patterns of recovery within the floral community, providing more forage for animals than with climax vegetation.

Such an argument might well account for the "defacto garbage" on the floors (this is site furniture), as well as the presence of quantities of fire-cracked rock on the "floors" (see Schiffer's Table 11.2, 1976a:150) of these "abandonment phase" rooms. It is hard to imagine the use of roasting techniques or stone boiling inside the habitation rooms of an ongoing sedentary site, with the fire-cracked rock not cleaned up and disposed of as secondary refuse. On the other hand, fire-cracked rock as primary refuse in hunting camps is quite common, in my experience.

DO WE NEED A "REVISIONIST" BEHAVIORAL ARCHAEOLOGY?

First, if we view Schiffer's work through the same critical eyes he has turned on the work of many of the "new archaeologists," then we would have to conclude that Schiffer, too, has failed because of that pesky problem of the Pompeii premise. He has assumed a direct relationship between his unit of observation (the site) and a unit of systemic context (a single occupation, both ethnically and functionally conceived). The possibility that Schiffer has completely distorted the past rests with his failure to appreciate an important formation process (successional use) as a possible contributor to his "recovery context" materials (see Binford 1972:314-26). Using Schiffer's own criteria, then, we are clearly in need of a "revisionist" behavioral archaeology.

I think that the reader would agree that this would be an unwarranted conclusion to draw from Schiffer's quite probable failure, yet this is the type of conclusion which Schiffer draws from his critical evaluation of other archaeologists. The "new archaeology" has primarily involved a claim that we did not know all the sources of variability contributing to the archaeological record. Given this condition, new archaeologists have argued that it is premature to offer an a priori evaluation as to the "limitations" of the archaeological record for yielding up information:

There has been as yet no attempt to assess the limitations of the archaeological record for yielding different kinds of information, nor does there seem to be the means of accurately
determining these limits short of total knowledge of all the systematic relationships which characterize past cultural systems. Thus, present discussions of limitations...are inappropriate and are based on speculation (Binford 1968a:22).

Schiffer distorted this posture into a claim that we know all the sources, and therefore any failure to recognize, say, a particular "formation process," would be clear evidence of a false principle (the "Pompeii premise") at work. Schiffer's position is very close to sophistry.

Pompeii is only an ideal for one interested in events, specific behaviors, and event-centered "history." For Schiffer, who obviously has a reconstructionist goal in mind, Pompeii is the most desirable condition of the archaeological record.6 Sadly, after encountering the archaeological record he became disillusioned; it was not a series of little Pompeiis only in need of "dusting off" in order to yield "ethnographies," or complete pictures of the past.

Just because Schiffer was disillusioned because of his own false expectations, it is unfair for him to blame that disillusionment on the new archaeology; such behavior can only result in the silly posturing visible in Schiffer (1978). For instance, in striking out against the "Pompeii premise," Schiffer claims that the new archaeologists believe that a complete cultural system is capable of being "seen" at a single site, and that any site contains meaningful information on any subject (Schiffer and House 1977:250):7

While the new archaeologists can be applauded for their desire to promote...verifiability in archaeology, they must be faulted for not going far enough and especially for obscuring the fact that...not all sites are equal in research potential (Sullivan 1976).

I can only respond with Flannery's words "Leaping lizards, Mister Science!"

Although presented as a "straw man," this little example of disillusionment points to a major flaw in even the positive aspects of Schiffer's work, namely, his search for "precise moments in the remote past" (Roe 1980:107). Paradigmatic growth is crucial to the growth of science, providing as it does the concepts which may in turn be transformed into an observational language for the science. If we dismiss most of the archaeological record as distorted, mixed, or disturbed, and seek only those provenience units which appear to represent little capsules of human behavior, we will continue to have an impoverished, unrealistic view of the past. We must seek rather to understand the archaeological record in the state in which it is available to us. In most cases, the greater the apparent disorganization, the more intense the use of the place in the past; it is these disturbances we must understand, instead of seeing them as conditions which render the site "insignificant" and the past unknowable. Disturbed deposits, such as mixed "plow zone" aggregates of tools, are the most common remains we encounter; if we hold out for the very few sites where we may "recognize" undistorted "analytical units," then we will have very few remains from the past which to work.8 The challenge is how to use the "distorted" stuff, not how to discover the rare and unusual Pompeiis. It is clearly impossible to understand the settlement system of a group of mobile hunter-gatherers when you demand, as Schiffer has, "undisturbed deposits made by the same group in different locations in the environment" (Schiffer and House 1977:252).

Holding up the Pompeii premise as an ideal insures that the imperfect world of "dirt archaeology" will almost always be a frustrating one. Perhaps herein lies a
clue to Schiffer’s recent statements that archaeology is not “prehistory,” and that many archaeologists are “not concerned with objects at all and some do not even dig!” (Schiffer 1976b:9).

It is particularly crucial to have an adequate and realistic observational language for treating properties of the archaeological record as it is; it is the very act of conceptualizing archaeological observations that conveys meaning to them, and in turn implies conditions in the past. If our conceptual inventory is impoverished, relative to the processes which could be responsible for the objects remaining for us to see, then our views of the past will be correspondingly impoverished and unrealistic.

It is perhaps ironic that Schiffer, who for many has played the role of the master critic, taking many to task for having “failed” to appreciate the complexities of “formation processes,” is at the same time content to view so much processual evidence as distortions. Not only is the Pompeii premise rarely justified by the archaeological record, but seeking a reconstructed Pompeii is an unrealistic and unprofitable goal in the light of the knowledge we have and the data available to us in that record.

NOTES

1. See Rouse (1964:465) and Wauchope (1966:19) for strong statements supporting Ascher’s position on this point.

2. In the minds of many, my claims that we could learn about conditions in the past which were not “directly” recoverable seemed to be a continuation of Taylor’s position, and many authors have offered skeptical arguments in the form of “cautionary tales,” largely drawn from ethnography, to demonstrate how limited archaeologists’ data actually are! Richard Lee’s (1966) “lesson” on how limited archaeological remains are when compared to the “richness” of the actual life that left them was amplified in the writings of Carl Heider, who demonstrated how “spurious” associations between things could be generated, and how the dynamics of an actual system would “tend to mislead the archaeologist” (Heider 1967:57). These “cautionary tales” generally assumed a strict inductivist approach to inference, and were usually offered as a skeptical note on some of the goals of archaeological inference, as they were discussed during the 1960s. They were all arguments against a “direct” linkage between the facts of the archaeological record and the past, challenges to the “Pompeii premise.”

3. After listening to a major statement of the position presented by M. Collins (1975) in a symposium in San Francisco in 1973, and later championed by Schiffer, I replied as follows (Binford 1975:252): Raising objections that the archaeological record may be “biased” seems to stem from some strange expectation that the archaeological record is some kind of fossilized picture remaining to us from the past with built-in and self-evident meanings upon which ‘time’s arrow’ has played disturbing tricks. No such meaningful picture exists complete with semantic directions as to how to properly read the record. All meaning comes from us. . . . telling me that the archaeological record cannot be translated mechanically into statements about the past according to some set of rules for assigning meaning does not surprise or depress me . . .

4. Such a perspective is certainly not unique to Schiffer; (see Jochim 1979:83; Spiess 1979: 1-5).

5. The very practical consequences of these contrastive points of view are well illustrated by comparing Schiffer 1979a with Binford 1979 for vastly different approaches to lithic studies. Similarly, Schiffer’s (1976a:56-57) discussion of curation should be compared to my discussion (Binford 1976) for a graphic example of the differences between a search for systems properties and a concern with specific behaviors and their cumulative result.

6. See Dunnell 1980 for his opinion on “reconstructionism.”

7. This is also true of other recent critics,
who have suggested that I believe that the archaeological record contains information about all past events (Coe 1978), or that I consider there to be an isomorphism between past behavior and the archaeological record (DeBoer and Lathrap 1979:103, 134). Similarly, see Gould's (1980:6-28) "revelation" that sites are multicomponential and behaviorally stark. Cautionary tales continue to demonstrate, again and again, the fallacy of the Pompeii premise.

8. Perhaps the popularity of Schiffer's position with Cultural Resource Management archaeologists arises from its use as a basis for writing off most of the archaeological record as "insignificant," since it does not approach the idea of little prepackaged Pompeii. This approach renders it impossible even to begin to see something of the organization of past cultural systems.

9. See, for example, Schiffer 1975 and Sullivan and Schiffer 1978.

REFERENCES CITED


Anthropological Association, New Orleans.


