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Introduction

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The papers and discussion presented in this volume are the result of a number of years of growing concern, on my part and that of the other seminar participants, over how to describe and explain change in what are variously referred to as social, cultural, or sociocultural systems (which I refer to here as "societal" systems, even though the term does not avoid all of the vague, loaded, and unoperational connotations of other terms). It is evident that while anthropologists and other social scientists have had a long-standing interest in explaining change (and have come up with some interesting results as well), we do not yet adequately understand either how to describe and measure change or how to explain it adequately in scientific terms. As scientists, we are not only interested in explaining varability within and among contemporaneous societal systems, but also we are interested in explaining both change and stability in such systems—or at least in those aspects of substantive societal systems that involve specific aspects of human behavior that we believe are in need of explanation.

The essential question is, Why and how did observed societal forms get to be the way they are (either today or in the past)? An extension of this question is, Can we predict future changes in these forms? The former question, at least, is precisely the one asked by Charles Darwin, Alfred Russell Wallace, and others with regard to biological forms; and it is the question that ultimately led to the modern synthetic theory of evolution. It is clear that we are in need of a coherent body of theory on *societal* evolution and we hope that this volume is a beginning in that direction.

A brief evaluation of past theoretical constructs (general explanations) of change is presented by Fred Plog (chapter 2). He tends to emphasize the elements of usefulness in these approaches, however, rather than focusing a great deal of attention on their inadequacies for our purposes; and this permits me the opportunity of focusing on the latter here, by way of introducing and justifying the seminar and its results. I make no attempt to cover all of the past approaches that might be relevant, but I do think it is important to point specifically to some of the fundamental inadequacies of at least a few of the most prominent published frameworks for explaining change—the ones that have been specifically labeled "evolutionism." The following statements, then, are viewed as supplementary to Plog's historical efforts; they do not include consideration of such explanatory models as "acculturation," "diffusionism," "behaviorism," and "growth" (see Plog, chapter 2).

I consider here only the paradigms of unilineal evolutionism, general evolutionism, multilinear evolutionism, and specific evolutionism ("neo-evolution" or "cultural ecology"). I do not intend, in doing this, to imply that the other paradigms Plog mentions are either more or less useful to us than these—they are not. I have chosen to review these four simply because they *do* claim evolution as their focus of attention.

Consider first the unilineal evolutionism of the nineteenth century (e.g., Morgan 1870, 1877; Tylor 1871, 1889, 1899). While it described general evolutionary stages, based on criteria of complexity, there was no satisfactory specification or detailing of the processes by which societies got from one stage to the next. "Progress" was, in a sense, inevitable for societies—or at least for some of them. In accounting for why some societies had evolved further than others (a process question), the answer was that some *races* had evolved further than others—they were more advanced on the evolutionary scale, and hence were more intelligent and capable of developing complex social institutions, technology, and so on. If this had been demonstrated to be the case, it would have been at least a useful partial general explanation of change. But since it was subsequently found not to be the case, the "explanation" is clearly inadequate.

In all fairness, of course, it should be pointed out that within the framework of then existing knowledge the explanation had to be considered reasonable—especially given the then believed "fact" that human biological evolution had occurred only very recently, and thus must have been very rapid. If the divergence of the races had occurred only in the last few thousand years, it stood to reason that some or all societal differences might be ascribed to racial differences. With the increased understanding of the mechanisms and great time involved in biological evolution, however, it became obvious that most societal differences had to be far more recent in origin than the divergence of racial types.

The more recent general evolutionism of Leslie White (1943, 1959) represents a major advance in societal evolutionary theory, yet it is also inadequate for our purposes. In fact, I would call it nonevolutionary, in the sense that it does not account for the *processes* through which evolutionary change occurs (as the biological theory of evolution clearly does). Essentially, his explanation for why some societies are more complex than others is that they are able to harness more energy per capita. So far so good; and the idea will probably be very useful to us. Although White explains why some societies are able to harness more energy than others (that is, because of more powerful and efficient technology), he does not specify the processes by which some societies obtain such technology in increasing amounts while others do not. The processes are simply unclear in his writings; and in the absence of processual mechanisms, it is inappropriate to label his ideas as a theory of evolution. To me, at least, the phrase "evolutionary theory" necessarily implies processual specification. While one can simply use the term evolution to refer to descriptions of evolutionary stages, there should in this case be no theoretical claims implied. Describing the course of evolution does not explain it—and the term "theory" always implies explanation.

The multilinear evolutionism of Julian Steward (1955a) also poses difficulties. It *does* become more specific in that it deals with accounting for variability in the adaptations of specific "culture types," or "levels of cultural integration," rather than dealing with general evolutionary stages. It is also more specific with regard to process in that it correlates specified "core" organizational attributes of the culture types with specified general characteristics of the different types of physical environment and technology associated with each culture type. Steward is definitely explanatory; he does, at least at a general level, account for why it is that his different culture types have different core organizational attributes—in terms of general differences in environment, technology, and exploitation of the environment. As an integral part of this, he emphasizes the fact that there are cross-cultural regularities in these processes of adaptation—hence the intraculture-type similarities in core attributes of organization.

Steward's contribution, like White's, was substantial. At the same time, however, it is inadequate in several respects. In the first place, it is still too general; it accounts for cross-cultural similarities in only a few very general attributes, and does not account for many other aspects of intra- and intersocietal variability that are of interest. Related to this, his emphasis on explaining similarities virtually ignores accounting for differences in societies within his culture types. Third, he is usually not very specific concerning the precise determinant variables and processes involved; and he says little or nothing about the causal efficacy of feedback relationships between cultural systems and their environments. Much of his work, and that of his followers, has involved more the correlation of societal attributes with techno-environmental characteristics than the specific explanation of such correlations.

Also important is the fact that Steward's multilineal evolutionism is largely nonevolutionary in nature. His *Theory of Culture Change* (1955a) is devoted largely to accounting for why it is that there is variability among kinds of societal organization—not why or how they change from one form into another (evolution). In that sense his work is more "functional" than evolutionary. To be sure, some of his works are indeed evolutionary (*cf.* especially Steward 1937, 1949), but even in these instances his explanations are too simplistic. He emphasizes univariate rather than systemic causal determinacy, subscribing to population growth and/or large-scale irrigation as prime movers.

In short, while he (and his followers) have contributed a great deal to our thinking concerning the processes of societal evolution, he has not presented a coherent theoretical construct that will adequately account for both stability and change—especially in terms of accounting for why certain aspects of *individual* societal systems may remain relatively stable, while others are changing. The specific processes of stability and change are missing. Moreover, he offers no theoretical paradigm by which one might be able to predict the nature, direction, and timing of change.

There are, of course, a number of anthropological studies (largely post–1960) that have overcome some of the difficulties of multilinear evolutionism. These can be included under the labels specific evolutionism, neo-evolutionism, or cultural ecology. This approach is associated with such names as Marshall Sahlins, Elman Service, Andrew Vayda, Anthony Leeds, Clifford Geertz, Roy Rappaport, Marvin Harris, and others. It focuses on individual societal systems and aspects of such systems, and accounts for their maintenance in terms of systemic relationships among sets of specific societal components or measurable variables. Rappaport's work (1967) is a notable example.

This approach is a form of structural-functionalism. The basic emphasis is to show in some detail how the specific systemic components of a society interrelate with one another and "function" to regulate the system in such a way that it can continue to operate in the face of environmental variability. In short, aspects of a societal system serve (or "function") as homeostats to maintain stability or equilibrium.

Many of the studies emanating from this general approach focus on describing the interrelationships among substantive components of the societal system (that is, social groups and institutions), and on pointing to the functions they perform in maintaining systemic equilibrium. Other studies are more refined in a processual sense, and describe societal systems in terms of sets of interacting *variables* rather than as sets of interacting components (Rappaport 1967). But in either case, this approach focuses on describing *how* societal systems operate as subsystems within an ecosystem.

In this sense, such studies are clearly systemic, processual, and explanatory. They are systemic in that multivariate interdeterminacy is considered, including the importance of both internal and external feedback loops. They are processual in that they describe how specific regulatory mechanisms operate in the face of specific and measurable environmental variability. And they are explanatory in the sense that the behavior of the various components and variables of the system is accounted for (predicted) in terms of the behavior of other components and variables of the system and its environment.

The approach is clearly a useful one, and superior to the approaches previously considered—at least in its specification of process. It appears obvious that before we can explain change in societal systems, we must be able to describe the operation of our systems—and the cultural ecology approach is well on its way to being able to do this acceptably.

Unfortunately, however, the approach suffers from being non-evolutionary; it will not, by itself, explain change. It is, for the most part, focused on describing societies as they are, rather than on discovering the processes through which they arrived at their current states, or the processes that might be promoting change. In fact, it appears that most modern social theory, as well as practice, is concerned primarily with the processes of equilibrium (*cf.* Sahlins and Service 1960; Buckley 1967:1–40). The emphasis is on describing societal organizations, correlating these organizations with their environments, and pointing out the ways in which they seem to be well "adapted" to their environments.

It is clear that theoretical constructs emphasizing the maintenance of stability will not be very useful, by themselves, in accounting for change. However, I argue in my own contribution to this volume (chapter 3) that the processes of systemic stability are an integral *part* of the processes of change.

The primary point that should be reemphasized here is that apparently none of the general approaches that call themselves (or have been called) evolutionary actually explicate the processes of evolution—at least not sufficiently. While we have a number of descriptions of change in the literature, the precise processes by which it occurs, and the means by which it might be predicted, are not specified.

I emphasize that my intent is *not* to imply that none of these so-called evolutionary approaches have important elements of usefulness to us in our quest for an integrated theory of societal evolution. As Plog points out, these as well as other approaches do contain useful elements; part of the trick is to separate the wheat from the chaff, and to integrate these useful elements (together with other relevant elements) into a coherent theory. Even the rightfully maligned culture historical or diffusionist approach (*cf.* Binford 1962, 1968a, 1968b, 1972; Flannery 1967) has some usefulness, as I point out in my own chapter (3). Nonetheless, a completely satisfactory evolutionary theory has not yet been developed—here or anywhere else.

The need for further investigations into the processes of societal stability and change is evident. The purpose of the seminar was not to sit down together and devise an a priori body of theory which we could claim to be sufficient and useful as is, although general theoretical propositions are certainly proposed in the following chapters. At least of equal importance was the goal of doing what we could toward learning how to go about building an appropriate body of theory. In this sense, much of the material that follows is methodological rather than theoretical. Nonetheless, my feeling is that the following chapters, taken as a whole (and including the discussion), represent a remarkably coherent theoretical and methodological framework for explaining stability and change, its insufficiencies and lack of refinement notwithstanding.

In essence, our general framework is based in general systems theory, although it also includes the tenets of biological ecology, the theory of evolution, locational analysis, and other elements as well. While the approach is general, it is also ultimately testable against empirical data. And while we have done what we could to carry out some limited testing in these papers, the primary focus thus far is not on testing. That, however, is certainly the next step—and the idea that the testing will involve computer simulation is agreed upon.

It is worth noting that while the title of the seminar was "Explanation of Prehistoric Organizational Change," I have deleted the term "organizational" from the title of this volume. The reason for doing this is important by way of introducing what follows. The difficulty is that this term would perhaps have implied that we are concerned only with explaining change in things that are commonly understood as "organizational" (such as organization of residences, tasks, sodalities, statuses, and so on), and not in nonorganizational entities (for example, projectile points or house structures). Actually, this is not the case; our general approach is designed to explain change in any aspect of societal systems, whether it be a system or subsystem, or simply a physical and nonsystemically organized component of such a system.

The initial idea that we were dealing with "organizational" change as opposed to some other kind of change (to be defined) would have been a false distinction. The reason for this is, of course, that system components (entities) that are not themselves internally systemically articulated and regulated (as systems or subsystems) are nevertheless parts of such systems. As such, variability and change in them can only be explained by reference to variability and change in the systems of which they are a part. To give an example, changes in the forms of projectile points cannot be explained by reference to the projectile points themselves—they cannot cause themselves to change. They change only when activities surrounding their manufacture or use change (as when the nature of the hunting or warfare subsystems changes).

In this sense, then, everything we study is either a component or subsystem of some other system—a point that Arthur Saxe makes quite forcefully (chapter 4). Thus, explaining change is *always* explaining *organizational* change—systems, by definition, are organized. Plog and Wright (chapters 2 and 7) reinforce this idea by pointing out that the explanation of any given phenomenon lies in placing it within a context of interacting variables, and that within such a context the behavior of the phenomenon can be predicted by the behavior of these other variables.

Thus it is not surprising that at the seminar there was very little concern expressed for the definition of the term *organization*. Since we took a systems point of view from the outset, the discussion (and concern) was phrased in terms of What is a system? and What is systemic change? rather than What is organization? and What is organizational change? The term *organization* is understood once an understanding of the nature of systems is achieved. And that is indeed a major topic of consideration in this volume.

I might also point out that while the seminar title implied a concern for explaining change in aspects of cultures, social organizations, and so on, there was no discussion about what a culture or social organization might be, definitionally or otherwise. It was simply understood that definitions would constitute irrelevant academics—these concepts are not in need of definition. If we can describe and explain both stability and change in measurable aspects of societal systems, that is enough. Whether or not we call the results studies of "culture" or something else is unimportant—and this is reflected in the varied usages of such general labels throughout the volume.

Finally, I almost decided to delete the term *prehistoric* from the title, since it is in some sense irrelevant. Change is change, and understanding its processes is independent of the temporal loci of our data. Even though the intent of the seminar was to lay a groundwork for explaining societal changes that occurred in prehistoric times, our discussion was by no means restricted to prehistoric data and the relevance of our work should not be so restricted. At the same time, to delete the word *prehistoric* would have been misleading because most of our examples make use of prehistoric data. And we do indeed share the belief that archaeologists are in a very special position with regard to explaining change.

The seminar papers are presented as chapters in this volume (chapters 2 through 8). The primary rationale underlying their order of presentation is based on the relative degree to which each emphasizes either method-theory statements or substantive contributions. While virtually all of the chapters deal in one way or another with important methodological or theoretical concerns, I have tried to place the ones focusing exclusively on method and theory first, following these with those dealing with both kinds of contributions. Related to this, the ordering represents an attempt to lead the reader through the fundamental concepts and principles of our general approach before exposing him to substantive examples with the hope that the substantive cases can be better understood in their appropriate contexts. I have not been a slave to such an ordering, however. Even though Wright's contribution is primarily methodological, I have placed it near the end, immediately before Sanders's work, simply because both are concerned specifically with explaining the evolution of the state.

Plog's paper (chapter 2) deals, first of all, with the nature of acceptable scientific explanation. He proposes that in order to have a good explanation we must be concerned with the nature of our formal, logical model of explanation, the nature of our substantive explanation, and the operational procedures required for an acceptable explanation. He then defines "change," and discusses four general approaches to explaining change that have been employed by anthropologists in the past. He argues that all of these "paradigms" have been and are useful, but none are adequate in themselves, and none of them are mutually exclusive. Finally, he presents his own model of the nature, description, and explanation of change, which in turn is composed of three non–mutually exclusive models of change which he believes should all be used in explaining any given situation of change, whether in societal systems, small groups, or individuals.

It is perhaps noteworthy that Plog's contribution is the only one emphasizing the importance of employing several explanatory models concurrently. At the same time, I see nothing in his work that conflicts significantly with the views presented by the other authors—his concern is simply somewhat more broadly gauged, and it provides a good context for what follows.

My own contribution (chapter 3) attempts to describe and develop an integrated general systems approach to explaining both stability and change. I provide a discussion of fundamental systems concepts, including the nature of regulation and change in living systems. I then evaluate the previously published systems approaches of James G. Miller, Walter Buckley, and Magoroh Maruyama, concluding that there are probably no published systems approaches suitable for our purposes. Following this, I present a modified approach that I believe *is* useful, providing hypothetical examples to illustrate my points. I then specify my view of the operational requirements necessary in explaining change, including the place of systems simulation. I also consider the problems of measurement faced by archaeologists attempting to explain change; and I conclude by evaluating the usefulness of general systems theory for our purposes.

Saxe's chapter (4) begins with a consideration of the nature and behavior of living systems, emphasizing the processes of both stability and change (morphostasis and morphogenesis), and how they operate together as evolutionary processes. His focus, however, is on the *origin* of evolutionary processes; and he argues that change is never initiated by factors internal to a given system, but rather is initiated (caused) by external factors—namely, long-term matter-energy interrelationships with at least one other system. He concludes that "a system in adaptive equilibrium will remain in adaptive equilibrium unless the equilibrium is disturbed by some extrasystemic force." He makes a good case for the idea that "an evolutionist explanation involves a functionalist explanation at a superordinate systemic level."

Saxe then provides a demonstration of the usefulness of his model of the change process using protohistoric and early historic data from the Hawaiian Islands. In fact, he rather convincingly explains the origin of the state in Hawaii following the arrival of Captain Cook in 1778 (or, the evolution from "chiefdom" to "state"). His explanation accounts for why it was that this organizational change was inevitable, given the extrasystemic inputs that occurred—and why it could not have happened in the absence of such inputs. His explanation is, in a general sense, relevant to explaining the origin of the state anywhere—past, present, or future; and it explains the many such transformations that occurred elsewhere in the world following European contact. This, in my view, is one of the very few cases in which an anthropologist has offered and partially tested an acceptable processual explanation of change. Saxe's two appendices provide many of the data necessary to document his case.

Ford's contribution (chapter 5) also considers briefly the nature and processes of living systems. But he then focuses on the *ecosystem* as his system of interest, and argues the importance of considering societal evolution within the context of the evolution of the ecosystem (the latter can be equated with Saxe's "superordinate system"). He presents a processual model of both ecosystemic regulation and evolution, emphasizing successional change in such systems. In short, his model attempts to explain the evolution of ecosystems.

He then shows how man, at all levels of sociocultural complexity, is an integral part of his ecosystem; and he presents a series of hypotheses on how human organization (at different levels of complexity) should articulate with and respond to the ecosystem and its evolution as well as how human organization can serve to help regulate and partially determine the evolution of the ecosystem. He then applies his model to the prehistoric American Middle West, and attempts both to predict the nature of ecosystemic and societal evolution that ought to have occurred from Paleo-Indian to historic times, and to test his explanatory hypotheses against currently available data.

It is a most provocative presentation. Ford offers not only a general explanatory model for organizational change in the Midwest, but also one that is applicable to numerous other areas of the world as well—particularly during immediate post-Pleistocene times. He courageously makes predictions about sociocultural evolution that can be tested, and from which further, more specific propositions can be generated.

Glassow (chapter 6) relates the principles of systems theory (and locational analysis) to the specific problem of explaining variability and change in the spatial patterning of population aggregations—notably households. The basic idea is that the spatial distributions of households (and other system components) can be explained because they tend to be located optimally in terms of the frequency and costs of matter-energy and information flows among them, and between them and their environments.

After considering some of the kinds of statistics required for describing spatial distributions, and developing his own modifications of them, he turns to his own prehistoric data from northwestern New Mexico to test two hypotheses designed to account for changes that occurred in the spatial locations and clustering of households during the Basket Maker II–III periods. He then turns to data from the entire northern Southwest, and tests a series of hypotheses designed to explain the rapid increase in site size and household aggregation that occurred during the same time period.

Given that his test results are interesting, though not as satisfying as he wished, he concludes that a systemic explanatory model, involving a number of articulated hypotheses, is far superior to testing a series of individual hypotheses one by one. He then develops and partially tests a most interesting general systemic model which may account for a whole variety of Basket Maker II–III changes, including the shift to dependence on agriculture, changes in site location and aggregation, and the evolution of aspects of societal organization and integration.

It is interesting that while Glassow does not use Ford's terminology, his explanation of Basket Maker II–III social organizational changes is fundamentally the same as Ford's explanation of social organizational changes in the Midwest during Mississippian times. In both cases, the idea is that as groups of people become increasingly dependent upon agriculture, they become specialized agriculturalists without the possibility of reverting to substantial hunting and gathering, even in times of crop failure. Thus, because their crop success is dependent on their monitoring and responding to the vagaries of the environment, they are forced to undergo fundamental organizational changes that will permit them effectively to monitor both predictable and unpredictable environmental variation; and they are forced to develop adequate organizational and technological means for damping these environmental effects. In short, the societal system must undergo irreversible change—evolution. The essential nature of this kind of explanation is presumably widely applicable to accounting for a variety of changes in societal systems.

Wright's contribution (chapter 7), like those of Sanders and Saxe, is concerned with the systemic processes accounting for the general kind of societal complexity known as the state. He focuses specifically on the nature of research strategies that might be useful in this regard, given the probability that an acceptable explanation must necessarily be multivariate and systemic. The paper deals with the practical problems encountered in building an explanatory theory.

He first presents a four-category typology of past theories of the evolution of the state, and derives from this a series of four generalizations concerning the similarities among all four types of explanations. These are then used to isolate the major determinants (multiple variables) that can be presumed to have some determining efficacy with regard to the evolution of the state. He proposes that a good explanation must take account of these multiple variables, and must account for both growth and stability.

In presenting his views on how one might develop an appropriate theoretical model to account for the state, he discusses the nature of systems, the nature of operational explanation, the nature of a useful theoretical model, and the usefulness of simulation modeling. He then turns to his past and current work in southwestern Iran to exemplify his own strategies. He concludes, as Glassow also does, that it is much more desirable and useful to develop an a priori theoretical model to explain change, and deduce related hypotheses from it (to test) than it is to try to test the effects of various possible determinants one at a time in the absence of their theoretical articulation. In fact, hypothesis testing and theory development must go hand in hand.

While Wright's contribution is concerned with methods (strategy) for explaining the state, Sanders's work (chapter 8) is a substantive example of it. He describes and explains the evolution of the city and state of Teotihuacan in highland Mexico, attempting to account for why it developed where and when it did. After describing the area and its resources, and presenting an account of the successional changes that occurred in the area during prehistoric times (Early Formative through Historic), he offers and defends an explanatory model he feels is very similar to Karl Wittfogel's explanation of the origin of states. In essence, he explains it as a result of increasing population in the Valley, which resulted in increasing intensification of agricultural techniques and competition for critical resources (especially spring-fed irrigation water). An increasing population placed a premium on centralized managerial tasks for coordinating and managing the water resources, external competition, and trade networks. Eventually the city became large and powerful enough to control outlying areas, and to exploit them for critical resources necessary to support increasing craft specialization and class stratification within the city.

While Sanders phrases his explanatory model in ecological-successional terms, it is clearly a systemic model as well. It includes the interrelationships of several major variables, including population size and growth rates, intensification of agriculture, increasing centralized control and power monopoly, increasing population, and expansion of trade and control over hinterlands.

It is interesting to note that Sanders appears to rely heavily in his explanation on population growth as a prime mover. In comparison, the reader will note Wright's explicit emphasis on the importance of multiple variable determinacy. In spite of this apparent difference in viewpoint, it is important to point out that all of the seminar participants recognize the importance of multiple variable feedback determinacy—but at the same time, some tend to regard certain variables as more "determinant" than others. In fact, most of the papers (especially those of Hill, Ford, and Glassow) appear to regard population growth as, in some sense, a prime mover—even though rationally most of us would admit, I think, that systemic explanations do not necessarily (or even often) involve prime movers. The determination of the relative importance of specific kinds of variables in an explanation is a matter for empirical demonstration in the cases at hand.

Chapter 9, Plog's second contribution, represents a statement concerning the subject the seminar was taking up during the last day or so—computer simulation. As the seminar progressed, it became evident to us that computer simulation clearly has a major role to play in both developing and testing explanations of systemic change (and stability). While there was little we could do in the time available toward developing a simulation model of an empirical case involving change, we were nonetheless able to use Saxe's Hawaiian data (chapter 4) in an effort at setting forth the bare outlines of what a simulation model might look like.

Our view was that this was "icing on the cake," since we had already accomplished as much as we had set out to accomplish at the seminar—realistically, at least. And, indeed, this simulation attempt can be considered as no more than "icing," in that no claim is made to have produced more than the rudimentary outlines of a simulation. With more time, I'm sure we could have done more in this direction—and at this moment others (as well as ourselves) are actively pursuing this direction with some success. Nonetheless, we feel that it is important to present our tentative starts in the direction of computer simulation, since this effort may be found useful to those who are doing similar work. At the very least, we think we are pointing in the right direction. All of us are grateful to both Plog and Saxe for their contributions, and we look forward to a future seminar in which we can put more fully into practice the ideas that came forth at the conclusion of our meeting.

It is noteworthy that Plog's second contribution does not deal in detail with the usage of computer simulation in dealing with *change* in societal systems. Obviously, we must learn how to simulate equilibrium or steady-state situations before we can satisfactorily begin to use simulation in studying change. As far as I know, there have thus far been no successful attempts at explaining change using simulation modeling. While my own chapter (3) sets forth some of the procedures I believe will be necessary to do this, we have yet to see it done using empirical data. The problems such an endeavor presents, while not insurmountable, do indeed represent a challenge—as should be evident from a careful reading of this book.

Chapter 11, by Perlman, is a contribution of a somewhat different nature than the others. As one of the two discussants, his task was to evaluate the other papers, as well as the entire research effort represented by the seminar. Thus, even though he makes method-theory kinds of observations, his is the least formal paper in the volume.

The selection of Perlman as a discussant was indeed fortunate, since his views were in many respects quite different from those of the other participants; and he forced us to consider issues which we might not otherwise have discussed critically. This is clearly reflected in both the discussion (chapter 11) and his own contribution.

Since it was not possible for him to consider all of the papers and issues individually, he has chosen to comment on the two most important issues: (1) the nature of explanation, and (2) the nature of systemic stability and change. In both instances his views are clearly at odds with those of the other contributors. In considering the nature of explanation, for example, he contends that logico-deductive models are inappropriate for the study of complex systems, and that a "pattern" or "systems" type of explanation should be used instead. And in dealing with the nature of societal stability and change, he proposes that belief systems, ideologies, and system "goals" are extremely important. In fact, in his view the primary "process" involved is the "goal" of minimizing "disturbance" within a system. His notion of the nature of societal systems also differs from that of the other participants, though it is important to point out that he appears to be thinking primarily of substantive, on-the-ground systems rather than of systems as sets of variables related by equations.

In any event, many of Perlman's differences in viewpoint highlight major issues in the social sciences; and these issues inevitably arise whenever there is a confrontation between materialist and idealist philosophies (cf. Harris 1968). It is most unfortunate that the other discussant (Stuart) was not able to contribute his evaluative comments, as his views would have provided the contrasting strict materialist perspective.

Chapter 10 consists of edited portions of the taped seminar discussion. It is important because it deals directly with the pros and cons of many of the major issues surrounding the explanation of change. The other chapters do not, for the most part, do this; they instead provide coherent sets of ideas and data from the viewpoints of their respective authors. It is only in chapter 10 that one can really begin to see the force of the argumentation, the reasoning behind it, the degree of consensus, and so on.

It is my belief that, taken together, both the individual contributions and the discussion present many of the elements of an internally coherent approach to explaining stability and change, even though it may still be somewhat loosely articulated. It is, of course, largely a systems *theoretic* rather than a systems *analytic* approach—and we must certainly increase our sophistication in employing systems analysis techniques with our data. But the two "approaches" are not in opposition, for we must also continue to refine and modify the general theoretical framework within which we understand the nature and processes of change. And as archaeologists and others increase our capabilities for analyzing systems, the result should be a concomitant increase in our understanding of systemic processes—and hence advances in the development of theory.