Systems Theory in Anthropology [and Comments and Reply]
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Systems Theory in Anthropology[1]

by Miriam Rodin, Karen Michaelson, and Gerald M. Britan

INTRODUCTION

Modern systems theory began having a significant impact on the social and behavioral sciences more than two decades ago. Since then, an entire generation of scholars has matured in an intellectual atmosphere which focuses on systemic relationships in social life. An understanding of the promise and problems of evolving systems approaches is now an important if implicit part of every anthropologist's training.

When the first *Anthropology Today* panel met in the early 1950s, modern systems theory barely glimmered on the horizon. In the fall of 1977, when a new World Anthropology Conference was convened, one of six panels was devoted to the topic.[2] This is a critical report of the views expressed by the panel members and discussants who gathered at that time to consider systems theory and anthropology. It does not review all of the relevant (and rapidly growing) literature, but tries to identify the major foci of current anthropological systems research, their common themes, and the crucial problems that remain to be solved.

The World Anthropology–1977 Conference was realized through the efforts of co-organizers Sol Tax and Demitri Shimkin. It gathered an international assortment of scholars to discuss important issues in anthropology's present and future. Each contributor prepared a detailed outline for a module focusing on a particular anthropological issue. These prospectuses and the discussions that ensued are expected to provide the basis for a larger conference and for a truly global dissemination of the findings and promise of anthropological research.

The systems-theory panel was organized by Fred Eggan (Chicago), Robert Miller (Wisconsin), and Demitri Shimkin (Illinois-Urbana). Modules were also prepared by Richard E. Blanton (Purdue), Gerald M. Britan (Northwestern), Marian Lundy Dobbert (Minnesota), Howard Harrison (Wisconsin), John Lowe (Illinois-Urbana), Karen L. Michaelson (SUNY-Binghamton), Beatrice Miller (Wisconsin), and Miriam Rodin (Illinois-Medical Center). These papers were the starting point for a four-hour discussion that also involved participants from the audience.[3] This discussion, along with the papers, provides the basis for our report. First, however, we must find our bearings by outlining the scope of systems theory as we see it.

THE SYSTEMS PERSPECTIVE AND ANTHROPOLOGY

Systems theories emerged from a variety of sources—cybernetics, engineering, industrial organization theory, epidemiology, and Gestalt psychology—and in Western and Marxist social contexts. Systems analysis, then, is a generic term for the application of formal, including mathematical, methods to the task of describing physical, biological, and social phenomena. When modern systems approaches first came to the attention of social scientists in the late 1950s, many researchers viewed them as a panacea. Systems theory could transcend the limitations of simple, functional cause-and-effect models and portray human adaptation in terms of well-specified webs of mutual causality. This method of analysis had been successfully applied to the complex flows of energy within biological organisms and communities, and there was every reason to believe it could be applied to the apparently more complex realities of social life. Indeed, systems theory seemed to offer precisely what the social and behavioral sciences lacked—the tools to model multivariate interactions that had previously escaped intuitive understanding. Our models might be more complicated than those of physical science, but they could still be firmly grounded in measured, empirical relationships.

After the initial flush of enthusiasm, it soon became apparent that systems theory was not a cure-all. Although researchers developed more and more sophisticated technology and clearer understandings of systems structure and development, social reality could never be simulated in all its complexity. As given by Gödel's theorem on axiomatic incompleteness and by Bremmerman's number (on maximum computability), even the most sophisticated of theories had limitations. Scientists still had to make choices, develop measurements, and test specific causal relationships. Systems theory provided a tem-

[1] This article reports the proceedings of the panel on Systems Analysis in Anthropology of the World Anthropology–1977 Conference, held in Houston, Texas, November 28–29, 1977. It represents a truly communal effort. The three designated authors, whose names have simply been listed in reverse alphabetical order, served primarily as reporters, editing, collating, and commenting on the panel proceedings. Michaelson prepared an initial rough draft of the article, which was then circulated to all of the panelists for comment. On the basis of these comments, Britan and Rodin substantially redrafted the manuscript and recirculated it before making final revisions. Thus, while the authors have tried to represent all participants' opinions, the final responsibility for what is said remains, of course, our own. We owe special thanks to Demitri Shimkin for his assistance in preparing comparative materials on Soviet and Marxist systems thinking and research and for his critical presentation of mathematical techniques in systems modeling.

[2] The other five panels were (1) The Lessons of Human Evolution and Prehistory, chaired by C. Owen Lovejoy and Gordon Willey; (2) The Bio-Social Interface, chaired by Estelle Fuchs and Solomon Katz; (3) Human Ecology—Models for Human Survival, chaired by Thayer Scudder and John Bennett; (4) Symbolic Anthropology and the Psycho-Social Interface, chaired by Margaret Mead and F. K. Lehman; and (5) Public Policy and Anthropology, chaired by Dorothy Willner, David Mandelbaum, and Sam Stanley.

[3] Among these were Cyril Belshaw (British Columbia), Jonathan Benthall (Royal Anthropological Institute), Ọladẹjọ Okediji (Lagos), Theodore Schwartz (University of California, San Diego), Thayer Scudder (California Institute of Technology), and K. S. Singh (Anthropological Survey of India).

plate for complex interactions, but it could never reveal which phenomena were worth studying and which were not. Static pictures of randomly selected empirical relationships often became reified statements of their authors' own biases.

In reaction to results that were less than promised, many Western social scientists, anthropologists among them, turned away from a systems approach. The new paradigm maintained influence, however, as systems concepts permeated discussions in the scientific literature. Some researchers continued to apply the systems approach, to refine it, and to learn what it could and could not do. It is these efforts that are finally yielding fruit.

Modern systems theory is neither a simple nor a unified body of knowledge. Rather, it is a compendium of approaches, theories, and methods. Whether a general and nontrivial theory is attainable still remains in doubt (Sadovskiy 1974). At base, systems theory is a general perspective, a way of looking at the relationships among variables that has much in common with traditional anthropological holism. Systems are sets of covariant entities, no subset of which is unrelated to any other subset. Systems analysis focuses on the meaningful interactions of the parts with one another and with the whole as they influence some process or outcome. No elemental part can be understood only in terms of itself; we must also study its interactions with the entire system, which is shaped by both internal and environmental processes and conditions over time. Systems "thinking" thus tends to be processual (time and space), conditional, and probabilistic.

In this sense, systems theory provides a broad framework for analyzing empirical reality. It is a metalanguage which allows various disciplines and subdisciplines, both within the social sciences and outside them, to communicate with a single terminology. It is a paradigm which comprehends relationships through a unified perspective of mutual cause and effect within the structural constraints of systemic interaction. The development of systems thinking has been greatest in the United States, Western Europe (e.g., Germany [Klaus 1964, Radnitzky 1974] and England [Clark 1968]), and the Soviet Union. American systems thinking has been empirically oriented, conceptually diversified, and closely linked to technological and application problems; Soviet work, in contrast, has been largely theoretical, with little substantive research, and has been presented mainly in journals devoted to mathematics and philosophy.

One way in which Western systems theory differs from the primitive "functionalism" of anthropological holism is in its specification of units, aggregates, and relationships. It has been concerned with measurable entities—flows of energy, information, or material between well-defined elements. The larger theoretical framework of modern systems approaches pointedly bares the assumptions, limitations, and definitional criteria of any specific theory that is being applied and provides an opportunity for testing this theory against empirical reality. A systems model demands a conceptual clarity and a specification of conceptual level that enable relationships among variables to be understood along consistent relational dimensions. By clearly defining the conceptual level at which discourse is taking place, systems theories provide a bridge among the levels examined by related disciplines. Such a fitting together promises a cross-fertilization of both specific analytical techniques and more general theoretical abstractions.

Klir (1972) has edited a compendium of leading Western viewpoints in systems theory. Particularly relevant to anthropologists is Weinberg's (1972:137) discussion of the unreality of social-cultural boundaries under conditions of change. Weinberg in part addresses Boguslaw's (1965) criticism of much systems analysis by Western social scientists, which bases social models on formal institutions without examining the less explicit but nevertheless important informal relationships which transcend formal organizational boundaries. To some extent, legislated requirements for impact assessments have directed studies to reach beyond the level of formal institutions (Lee and Hung 1976).

Systems approaches have developed a unique brand of insight by examining the characteristics of "systems qua systems" (Geertz 1973) and considering the effects of different kinds of system structure on system performance. Following the lead of earlier cyberneticists, social scientists first studied closed, "well-structured" systems. Through such analysis, the more primitive concept of "functional equilibrium" was translated into an understanding of the homeostatic maintenance of specified systems characteristics through structured interactions among related variables. However, a static view of system-maintaining (and seemingly purposively designed) negative feedbacks was quickly abandoned in face of a discordant reality. Constant system performance turned out to be the exception, not the rule. Positive feedback, thresholds, oscillation, nonlinear and discontinuous relationships, and growth were quickly incorporated into systems analysis. More recently, researchers have begun tackling the complexities of open systems, whose structures are defined only by the interaction of parts in the environment. This provides a basis for modeling the most complex human systems, not as they maintain equilibrium, but as they reflect the adaptive needs of purposive human actors (Buckley 1967). Equilibrium is not a given, but a plausible special case which must be explained. Thus, contemporary systems approaches seek to encompass dynamic process.

In this respect, Soviet theorists have perhaps moved ahead of their Western counterparts in identifying certain principles of systems analysis. Blauberg, Sadovskiy, and Yudin (1969:21–23) have proposed the concepts of (1) holism vis-à-vis the environment, on the one hand, and component elements, on the other; (2) system coherence through linkages of different function and type; (3) relative stability and order of elements and linkages (i.e., structure) over time; (4) structure as having both horizontal and vertical dimensions; (5) hierarchically ordered systems characterized by determinate and probabilistic control; (6) in some systems, control that is teleological in the cybernetic sense; and (7) in some systems with teleological control, directionality, so that synchronic and diachronic analyses are required for a full description. These principles have been applied to analyses of scientific innovation (Ignatyev and Yablonsky 1976; Marshakova 1977; Yablonsky 1976, 1977) and to experimental psychology (Zinchenko and Gordon 1976). In the West, Turner (1977) has taken a similar direction in analyzing mental phenomena. Interestingly, through the medium of information theories, Soviet anthropologists grounded in the primacy of economic determinism have been able to include aspects of Western structuralism in their analyses (Markarian 1969, 1972; Gretskiy 1974).

Systems theory, then, provides a general theoretical framework for analyzing relationships within a bounded set of variables. It has generated a typology of systems structures—closed, open, hierarchical, decomposable, purposive—and has analyzed the implications of these structures for systems performance. It has grounded its understanding of larger systems characteristics—stability, flexibility, directionality—in an understanding of particular kinds of variable relationships. In conjunction with other, more specific theories, it has translated particular slices of empirical reality into abstract mathematical models that are multicausal but can still predict outcomes and be tested.

Modern systems theory can analyze the dynamics of a structurally static system, such as closed interactions in a stable environment, or the dynamics of an evolving structure, such as the transformations of revolutionary social change. Rather than ignoring the contradictions within human systems, systems analysis can model the contradictions, conflicting needs,

variable external stimuli, and internal tensions which are the paramount reality of social life and the special concern of Marxist anthropologists.

In this context, it is important to note that systems theory is not, as many of its critics claim, inherently conservative or static. Indeed, a systems approach attempts to transcend the conservatism of anthropological functionalism. Systems theory adds a means by which to understand not only continuous process, but discontinuous shifts as well. It need not, as Habermas (1973) and Kuenzlen (1972) suggest, ignore structural tensions or reduce social phenomena to input-output analysis and steering-mechanism problems.

Systems analysis, like Marxism and structuralism, has as its primary concern the examination of the underlying relationships which produce regularities in empirical phenomena—the internal logic through which the whole and its parts are related. It is a perspective about how relationships can be modeled, but it can only account for specific outcomes through reference to other theory—maximization, technoenvironmental determinism, learning theory, or the like. Any particular system analysis involves choices about what to observe and how to represent relationships, and these are only as good as the theory applied. Thus, it should be quite possible to use the language of systems—with its advantages of specificity and generalizability—to analyze not only static structures, but also Marxist relations of production and their transformation. Indeed, one could well argue that it is systems theory to which both Marxists and cultural idealists must turn if their specific theories about cause and effect are to be adequately tested.

That a given method has been used badly or that one disagrees with the findings of particular practitioners does not mean that it is useless. If Forrester's (1969) model of urban decay is conservative, it is because of his stated assumptions about the nature of significant variables and the structure of their interrelationship. Yet he was able to pinpoint the limits of common sense of intuitive models (Forrester 1971). Similarly, one may be critical of a Marxist "systems model" focusing on the economic origins of ethnic consciousness in the contemporary United States. In neither case is systems theory itself at fault.

Systems theory is a method of explanation through the modeling of multiple relationships, but it is an approach which, like most general models, can be applied in a variety of ways. In anthropology, its applications have varied widely, from the formal modeling of cognitive process (Leaf 1972) to the open analysis of adaptive coping, formal social organization, spatial relationships (Plog 1975), and environmental interaction. In some fields, such as regional analysis and ecology, empirically grounded and widely applicable theories of systems structure and performance have already been developed. In others, such as the dynamics of behavioral adaptation, we have discovered many of the questions and only some of the answers. Thus, this paper has two foci: To the extent possible, we will report on specific developments in the theoretically refined systems models that have enhanced our understanding of particular empirical phenomena. More generally, however, we will consider the broader issues that have evolved in the application of systems analysis to the modeling of characteristics of the human animal and his social groupings. Thus, while each panel participant provided his own insights in a particular substantive area, this report has been organized around the integrative topics which these contributions, and those of the discussants, suggest.

ISSUES IN ANTHROPOLOGICAL SYSTEMS THEORY

Our specific interests and methodological skills varied considerably, but we shared a common vocabulary and conceptual orientation. More importantly, we also agreed on the relationship between systems theory and anthropology, on the theory's limitations, and on what issues merit further investigation. Five questions surfaced throughout the session; this report edits the panelists' comments to present these themes in the following order:

1. How does systems theory relate to existing anthropological theory?
2. What determines appropriate system boundaries? Is systems theory best applied in local empirical analyses or at more general levels of theory building?
3. Can we devise models that comprehend individual variability and cope with problems of misplaced teleology?
4. What qualitative and quantitative methods are anthropologists using now?
5. How might systems approaches be applied in the future?

SYSTEMS THEORY AND ANTHROPOLOGICAL THEORY

Eggan's opening remarks on the history of anthropological theory suggested that early functionalism and structural-functionalism may well have met the theoretical needs of an earlier generation of ethnologists. Societies changed slowly during their colonial periods, and early ethnographers may have sampled an artificially stable universe. Therefore the static models of functional theorists may have simply reflected the political climate at the time. The rapid change experienced since World War II may represent not merely an adjustment to new conditions, but also a catching up on natural processes of social and cultural adaptation. The former colonies' striking rate of transformation since World War II, however, has sensitized us to the dynamism and variability of all human societies: the old static theories are found to be conceptually lacking. Recent theoretical innovations in anthropology have tried to explain the mechanisms of change.

Eggan noted that even the old static models contain a primitive concept of the systemic nature of society and culture. At least since Malinowski, we have ordered relationships among component social parts, focused on countervailing forces, related social and biological domains, and searched for heuristic models of culture. More recent applications of systems methods include the use of mathematical and logical models by Lévi-Strauss, cybernetics by Bateson, population dynamics by Vayda, Sahlins, Cancian, and others, physics by White, political control by Easton, and decision models by Parsons. The basic question, however, was the extent to which systems approaches offer a useful avenue for new theoretical development. To what extent, asked Jonathan Benthall, is systems theory a theory at all?

The panel reached a consensus that von Bertalanffy's (1967) coinage "general system theory" is slightly misleading. There is no single systems theory; there are many ways to examine the dynamic relationships among parts and the implications of these relationships for the whole. However, a specific theory must satisfy scientific and intellectual standards of adequacy: (*a*) It must be grounded in empirical data—either the qualitative observations of a trained ethnographer or the quantitative data of census statistics or social surveys. (*b*) It must have face validity; it must be appropriate to the research or development problem at hand. (*c*) It must be powerful enough to explain exceptions as well as the rules, but not so broad as to lose specificity. (*d*) It should motivate dependent hypotheses and generate new questions.

One aspect of a useful systems theory is a link to social applications, either substantive or methodological. In this vein, Michaelson offered the concept of a "critical theory," one which is directly related to application. Although "critical theorists" such as Habermas and Kuenzlen reject general system theory out of hand, their criticisms apply only to particular systems

models. Thus, when they claim that urban systems models lack human values, they merely underline the need for careful education in appropriate systems applications. As Harrison noted, social scientists, engineers, and planners must collaborate carefully if we are to deal at all humanely with phenomena that combine physical, biological, and social elements—what Harrison termed "people" and "semi-people" systems.

The panel finally agreed that systems theories could fulfill the requirements of scientific modeling. Moreover, participants felt that a systems perspective provides a promising way for social anthropologists to understand social and cultural change, rapid population growth, migration, institutional elaboration, and the relationship between cognitive and biological bases for human thought. Among the advantages this perspective offers is the possibility of transcending the ethnographic description of simultaneous events in linear language. Dobbert specifically recommended the use of flow diagrams as an aid in anthropological teaching and research.

Systems theories have already found wide application in many disciplines of the physical, biological, and social sciences, as well as in engineering and business. Yet, while traditionally anthropological problems have increasingly become the subject of interdisciplinary research (as Mead noted in her plenary address), anthropologists have themselves become increasingly isolated by subdisciplinary divisions. The unitary concepts and terminology of systems theory provide a lingua franca that can enable social, biological, and archaeological anthropologists to converse across specialties and work smoothly with engineers, managers, planners, geneticists, computer scientists, and others.

Many participants have already been able to work easily on common systems problems with colleagues in fields far removed from their own. As we compared our experiences in highway planning, agricultural extension, education, mental health, marketing, and the like, it became increasingly apparent that systems models are appropriate to a wide variety of theoretical and practical problems. Shimkin, Harrison, and Britan pointed out, however, that the selection of *which* model, or exactly how to apply it, requires intellectual rigor and honesty and a very careful examination of the limitations of the model's underlying theory. Merely relabeling acts and products as feedback, input, or output is not only atheoretical, but also terminologically sloppy and intellectually lazy.

A further advantage of systems analysis is its heuristic value in problem definition, whether or not other theoretical perspectives are finally applied. Given a researcher's assumptions about significant variables and dynamics, a systems model can offer a rough test of his hypotheses and may reveal important influences that are not intuitively evident and are difficult to conceptualize in other ways.

This final advantage returned our discussion to anthropology's need for dynamic theories. While it is true that systems perspectives are not the only approach to change over time, most forecasting models have incorporated at least rudimentary systems analysis. The primary difficulties in developing theories for social analysis are encountered in the early formulative stages, when terms are ill-defined and poorly understood. All research is founded on irreducible assumptions, and the value of theory depends not only on how well these assumptions emulate reality, but also on whether they are clearly understood by the scholar and his technical audience. The systems theorist, like any other researcher, must be sure his definitions and procedures are carefully and consistently wrought. While specific systems models may sometimes prove mistaken, their assumptions must at least be clear and their validity testable. In the professional exchange of publication, inadequate models can be winnowed out. As Cyril Belshaw indicated, the crafting of better theories is speeded when many researchers work within a common framework.

Systems Theory and Levels of Analysis

The problem of selecting an appropriate level for analysis is not unique to a systems orientation. Level, as we used it, refers largely to the kind of social aggregate to be studied. Should we focus on a single settlement, a social network, or the political and economic relationships within a city or between cities? Alternatively, "level" was used to distinguish among social, cultural, behavioral, and biological domains. The finding of an appropriate level of analysis is particularly important for those of us who are working with systems models, because the notion of interconnection and mutual influence among many variables can lead us to overspecify complex elements or, conversely, to elaborate models beyond our data-gathering abilities.

In any research, the first step is delimiting the range of phenomena to be measured or observed. Robert Miller, Shimkin, Lowe, and Britan all mentioned that anthropologists tend to select their levels of analysis arbitrarily. Anthropologists concerned with "cultural groups" can define them in terms of language, ethnicity, or subsistence pattern. Various notions of "a city," a "social norm," a "transformational rule," or a native taxonomy are treated as if they were comparable, yet such concepts often reflect the peculiar opinions of each researcher. Dobbert noted that native categories can be employed only to produce culturally specific descriptions of perceived interrelations (see Dobbert 1977). Ethnographic data must be classified, and this creates problems of definition; the choice of an appropriate level of analysis is inescapably related to the nature of the problem and theory motivating research.

Harrison contributed the insights of a working engineer through several examples of systems problem-solving and design. He argued that many alternative solutions may exist for any one problem, and the investigator must choose the most useful option: "Thus the appropriate division of subject matter is heavily dependent on purpose." After identifying an appropriate domain, "one will invariably study a system larger than the target." The conceptual problem of identifying the boundary between a system and its environment—defining internal and external variables—is largely a matter of judgment. As a rule of thumb, Harrison suggested that system boundaries be placed where system output does not significantly influence system input. For example, if hoe agriculture does not affect the natural environment's carrying capacity, the anthropologist can limit his investigation of a primitive economy to a consideration of domestic resource management. If, however, environmental characteristics are sensitive to human action, the system boundaries must be expanded to include those aspects of the environment (such as water table or soil fertility) that are affected. As Blanton indicated, it was precisely such a consideration that motivated his use of regional analysis to study the development of local markets, since the development of individual markets varied with trade among many settlements and settlement clusters. A dramatic application of this method of delimiting system boundaries was offered by Lowe in an analysis of the collapse of the Classic Maya. Britan noted that errors in boundary placement and uncertainty about the interactions among parts have sometimes kept anthropologists from developing adequate models. For example, while Barth's systemic approach to social change is in some ways overly general, its combination of environmental, behavioral, and cultural variables has stimulated an impressive and valuable series of investigations.

The discussion culminated in a more detailed examination of the particular systems models that participants have used in their own research. Rodin, for example, described how she applied ecological models of competition and predator-prey relationships to a study of urban neighborhood succession with comparisons to the dynamics of expansion and warfare among African segmentary societies (see Rodin 1977). Dobbert de-

scribed her use of conditional-probability matrices in analyzing context-specific decision making by individual actors. Beatrice Miller outlined the uses of dynamic systems theories in family therapy. Shimkin and Lowe described the application of Markov chains and other stochastic models to urban systems growth and decline. Britan discussed the nature of boundaries in formal bureaucracies considered as informal sociocultural systems. Theodore Schwartz discussed the applications of cybernetics to the understanding of cognition and social process. His research in Melanesia identified situations in which social structure placed severe limits on the feasibility of alternative adaptations. His comments were the basis for further discussion on future applications of systems models to problems of development.

At the conclusion of this round robin, the panelists generally agreed that the choice of a particular level of analysis—individual cognition, behavioral process, social patterns, cultural rules, or ecological interactions—was dependent on the specific problem at hand. Beyond this, Shimkin noted that analyses of larger social entities or longer time periods must necessarily be less specific. Small, well-defined, or local situations may be modeled fairly exactly within the limits of human predictability; the resulting theory can be applied to similar situations but may not be widely generalizable. Less precise models of large systems, such as national economies, may be capable of predicting large-scale shifts but probably not short-term or local fluctuations. Rodin suggested that the choice of an appropriate boundary depends on whether the researcher is interested in developing general or specific theories. Since most ethnographers describe and analyze particular local settings to enrich our inventory of human variety, specific theories of high precision present the most widely acceptable use of systems models in social anthropology today. Blanton and Lowe added that larger-scale models aimed at more general theories would be of greater use for archaeologists and prehistorians.

Thayer Scudder and Jonathan Benthall added a note of caution by pointing out that researchers often select models uncritically, without carefully examining their underlying assumptions. The easy confusion of competition and predation models and the loose borrowing of cybernetic terminology for cultural analyses were cited as examples. Both Scudder and Benthall emphasized that the credibility of systems analyses depended upon a careful reconceptualization so that assumptions and terminology developed in another substantive domain might be validly applied to social and cultural phenomena. Benthall was especially critical of statements mixing empirical data with value judgments, as in the phrase "systemic pathology." Blanton's and Harrison's use of continuous vs. discontinuous change, collapse models, and models of structural noncoherence and Lowe's concern for "forecasting the consequences" of a particular line of sociocultural development are instances of the maintenance of a more appropriate degree of cultural relativity.

Individual Variability and Misplaced Teleology in Systems Models

Early critics of systems analysis attacked the assumption that human systems are bound by mechanistic links so that an event must evoke a unique response. Such models, they argued, reduce human behavior to determinate output and ignore the variability of social behavior. While most participants felt that Harrison's designation of human systems as "ill-defined" simply begged the question, we recognized the conceptual and operational difficulties of modeling situationally plastic human action. Shimkin proposed that we start by thinking of behavioral process as "nonlinear, nonstationary, stochastic, and subject to structural change." To apply "system" characteristics to individual actors is an ecological fallacy. Harrison, Michaelson, and Shimkin noted that many economic forecasts assume that individuals exhibit only rational economic behavior. Yet, as Robert Miller noted in the case of sacred cows in India, when this logic is not grounded in native understandings it cannot predict either large-scale events or individual actions (see Miller 1971). Nonetheless, Rodin argued that such models have a heuristic value. Investigators can check their predictions against events to see how closely their assumptions reflect real cultural process. Shimkin noted, for example, that Lévi-Strauss's distinction between statistical and mechanical models is inconsistent with current understanding of cognition and behavior, in which memory and anticipation play a large role. Yet the computational capacity of the human mind does impose constraints, and mechanical categories may be viewed as coding devices that reduce environmental complexity. Thus, even if the assumptions are invalid, mechanical models can be tested against empirical indicators of social and cultural change.

While a stochastic systems model may account for present states, it cannot completely predict the future. Models may only be able to indicate the consequences of unchanging structural arrangements—and structures rarely remain unchanged. Human groupings are not mechanistic and perfectly specifiable systems. As Michaelson and Britan noted, human systems display real contradictions; all parts may not be equally related to all other parts. Tensions, moreover, are continuously generated in the process of life, and, as ecologists have discovered, equilibrium is only one of many possible states. Techniques for modeling changed structural arrangements need to be developed; Dobbert (1975) has suggested sequential generation as a method for handling this problem. Yet social theory is still imbued with the subtle teleological assumptions of early functionalism: "This practice exists in order to . . ." or "Social equilibrium is the goal of social process." Still, while the attribution of purpose to social *systems* is fallacious, the failure to attribute goals to individuals and corporate groups is also in error. Social systems analysis must encompass the dynamics of uneven distributions of beliefs, abilities, knowledge, and resources among people, as well as the genuinely differing goals among individuals and the corporate groups they form.

The State of the Art

As we have emphasized, there are many different systems theories and methods; it is thus difficult to draw a unified conclusion about where the field now stands. Indeed, systems theory appears to be increasingly pervasive not only in anthropology, but throughout the social and behavioral sciences. To date, we have been searching and experimenting with particular systems methods in each anthropological subfield, ranging from Bateson's use of cybernetics to understand schizophrenia to ecological models of cultural adaptation and to the "new" archaeology. The simple input-output models of the early 1960s have by and large been superseded by more complex ones.

The greatest technical advances in systems analysis have occurred not in anthropology, but in the natural sciences. Investigators there have applied finite mathematics—probability theory, vector and matrix models, linear programming, game theory (Von Neumann and Morgenstern 1947), and path analysis—to open and adaptive systems. The increasing availability of small high-speed computers has made it easier to apply these complex frameworks in a wide range of settings. Their use in anthropology, however, has been limited by their enormous demands for quantifiable and commensurable data. Even more importantly, these quantitative techniques have diverged widely from the methodological sophistication of

qualitative anthropology. Thus, we are still only beginning to apply the sophisticated mathematics emerging from physical-science models of discrete vs. continuous change, structural noncoherence, and topological transformation. What is needed for such analyses are theories that generate testable hypotheses. The panel suggested new approaches in social anthropology that would be more consonant with a quantitative systems framework.

Dobbert, Lowe, and Rodin, for example, suggested several ways to discriminate types of social systems on purely formal grounds. Beyond qualitative comparisons of kinship rules and terminology, they suggested the idea of mathematical isomorphism, which has had some currency among cognitive anthropologists. This would let researchers compare social systems on dimensions of dynamics, stability, and ranges of equilibrium solutions. Rodin further noted that biological systems often persist without achieving any stable equilibrium at all. Dobbert, on the other hand, outlined a strategy through which individual decision-making criteria could be represented with qualitative ethnographic data by estimating probabilities of particular decision contexts. The resulting probability matrices could be inductively grouped into classes for formal cross-cultural comparisons.

Blanton's (1976) work on regional analysis is another example. Using diachronic data from archaeological and archival sources, he and Kent Flannery and their associates documented cyclical processes of urban development in Oaxaca in terms of distorted central-place hierarchies that reflected the merging of administrative and market functions. The primate-city pattern developed to the limits of a region's agricultural potential but was followed by rapid collapse and decentralization. Blanton's work has many theoretical and methodological similarities to that of Lowe, who used systems analysis to identify the critical role of the Maya elite in pushing the lowland economy to collapse (Shimkin 1973).

Harrison broached the issue of systems whose components are of radically divergent orders, for example, man-machine systems. Beatrice Miller, Shimkin, and Lowe also mentioned biosocial systems, involving the interplay of biological and sociocultural factors in disease and epidemic cycles. Such models need a ready convertibility of measures, a condition which is and may remain unmet. Reliable measures of the relationship between social stress and cardiovascular pathology, for example, have not yet been devised, precisely because the cultural component in stress perception is difficult to quantify.

Human-ecological approaches, on the other hand, have consistently integrated physical, biological, and symbolic aspects of life. As Shimkin noted, game theory is a human-behavioral special case of Shelford's rule from biological ecology and can be applied to studies of human biocultural adaptation. This does not necessarily solve the problem, however. More than 50 years ago, Park (1969) borrowed biological ideas for his theories of urban ecology but mystified the analysis with unnamed forces and laws. Yet Rodin noted that biological ecology has coped with the problem of imputed teleology, and the concept of adaptation has been widely employed in urban ethnography.

Shimkin and Harrison outlined the promise for anthropology of theories of adaptive control in automated systems. Such machine systems grow more complex through feedback loops that have the capacity to modify system functioning. Britan and Michaelson noted Buckley's (1967) discussion of how feedback loops can result in changes in systems structure. Rodin and Lowe further observed that the concept of control used so far on mechanistic systems has at least intuitive applicability to human systems, though problems remain in operationalizing the concept of control to account for cultural and social stability. Issues like the role of the Maya elite in economic management and the role of grass-roots political organizations in gaining funds and services for the inner city seem amenable to such an analysis. Britan provided another example for further inquiry by distinguishing between formal and informal structure in centralized bureaucracies and outlining the effect of this distinction on the diffusion of innovations.

Techniques for quantifying cultural beliefs and values have not yet been perfected, and consequently qualitative models seem for the moment to provide better conceptualizations. Yet the combination of social, cultural, biological, and physical variables in qualitative analysis raises again the problem of comparable measurements (Shimkin, Hyland, and Rodin 1976). For example, when cognitive, demographic, and economic phenomena are combined in a single qualitative model, how can we determine the relative contributions of the various elements? This problem can possibly be solved through the use of weighting factors assigned through ethnographic observations of importance and/or through summing repetitions (Dobbert 1975). Harrison suggested that, in any case, qualitative modeling permits an investigator to identify those aspects of social and cultural organization which are more or less sensitive to external pressures and to examine the interrelationships among variables. Qualitative modeling and testing of theoretical assumptions can then be grounded in detailed quantitative analyses of selected subsets of the system.

Systems Application in the Future

In the final part of our discussion, several members of the audience took the initiative. Cyril Belshaw related our examination of systems theory to current directions in economic anthropology, observing that isolated investigators can never accomplish as much as a group that shares ideas and results. He suggested that a critical mass of anthropological systems theorists would stimulate further interdisciplinary collaboration. He noted that some promising systems applications have already been undertaken by the International Social Science Council in the area of world modeling and that more participation by anthropologists would be desirable. He also pointed to the need for anthropologists to be present in development agencies and mentioned several interdisciplinary projects already under way in Asia and Latin America. In Argentina, for example, there is a strong interest in systems analyses, and researchers there have developed relatively sophisticated mathematical applications.

Ọladẹjọ Okediji spoke of development in Africa and particularly of the need for studying urbanism and urbanization. He noted that in-country technical skills are less easily available in nonindustrialized nations and emphasized that we would have to communicate the immediate practical value of a systems perspective before it would be accepted. One goal of major importance to him and his colleagues is the discovery of social means for promoting political stability in rapidly urbanizing societies. He expressed reservations about the future acceptability of pure theoretical research: the focus should be on immediate applicability. This suggests a deemphasis on mathematical modeling and an increased concern for qualitative approaches.

K. S. Singh reemphasized the points made by Okediji and by Theodore Schwartz. He noted that Western social science has sometimes served the ends of recolonization and suggested that we make greater efforts to develop useful Marxist analyses. To that end, he felt that systems research might focus on the types of cultural groupings which are more and less amenable to change. He said that analysis of systems dynamics in East Asian and Indian societies would be of particular interest, given the variety and great stability of these societies.

Harrison responded with enthusiasm to the request for non mathematical formulations, arguing that systems concepts could be mastered quickly and used as a basis for qualitative models of social process Specific applications were proposed by

several participants. Michaelson said that social-impact studies of large public construction projects (such as dams, highways, railways, and housing) are technically feasible and immediately useful. Britan emphasized the utility of systems approaches in analyzing organizational dynamics and conducting program evaluations. Robert Miller focused on the systemic effects of proposed economic changes, citing conflicts on issues of taxation between local and national interests in both developed and developing nations. Dobbert mentioned the immediate application of stochastic systems models to the cultural transmission of knowledge and the structuring of educational situations.

Rodin and Shimkin described how game theories and theories of competition have been applied to the political dynamics of urban residential environments. In sum, the panelists suggested a number of local development problems that might be attacked with systems models and agreed on the necessary steps to begin such applications on a wide scale.

The need is not for global systems models, but rather for perspectives that are precisely based on local data. Many of us felt that anthropologists can play a useful role by helping to set up local record-keeping plans for in-country planning of health, agricultural development, and urban service systems. Panelists also held a general belief that quantitative systems analyses are best grounded in local studies. In such cases, reasonably complete data can be obtained for a significant time period, and, with careful conceptualization, the ethnographer can maintain direct control over the quality and validity of data generated by surveys, census, or record summaries.

CONCLUSIONS

The World Anthropology–1977 Conference was planned as an international forum to take stock of our discipline. Free from the confines of narrow academic discourse, participants considered how far we have come since the original *Anthropology Today* conference in 1952 and where we should now be going.

The systems-analysis panel, in keeping with this intent, ignored specific research in favor of a more general assessment of the systems revolution that has affected the natural and social sciences. Crowded into a small room, colleagues formerly known to each other only through abstracts and papers found a group of like-minded people sharing a familiar frame of reference. In this setting, we were able to isolate continuing issues and common concerns about the place of systems theory in anthropological research.

This report has been organized around the five themes that we jointly identified and discussed: the relationship of systems theories to anthropological theory, the problem of levels of analysis, the place of variable human behavior, the state of the art, and promising avenues for future investigation. By identifying these themes, we hope that we have helped to elucidate a theoretical framework in progress. By addressing issues of both theoretical adequacy and practical application, we have tried to meet the standards of science while remembering the needs of the human beings who are our subjects. In so doing, we are pursuing the tradition that Sol Tax has pioneered.

Now that we have isolated the general issues of action and theory, we feel that the major task still remains. That task is to stimulate substantive research incorporating the systems perspectives and methods we have described. It is our hope that this report will spread a growing awareness of the promise of systems perspectives in anthropology. We expect that publication of the substantive reports summarized in each panelist's prospectus will build the foundation for a more unified approach by scholars throughout the world. The World Anthropology–1977 Conference provided an initial forum. The next phase of growth should now begin.

Comments

by A. DE RUIJTER
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Rodin, Michaelson, and Britan's article reflects very well a characteristic of many large-scale world conferences, namely, superficiality. This characteristic is strengthened by the extensiveness of the topic: systems theory is "a compendium of approaches, theories, and methods." The result is that the article contains many uninformative vague generalities. An example is the conclusion of the panel that "the choice of a particular level of analysis—individual cognition, behavioral process, social patterns, cultural rules, or ecological interactions—[is] dependent on the specific problem at hand." In brief, five issues pass in review. One need not be astonished that the discussion of these issues is defective; many questions remain.

In this regard, I wonder why the panelists refer so little to Lévi-Strauss. His structuralism strongly emphasizes relationships between phenomena; it is preeminently a systems approach. His "Social Structure" was a breakthrough to modern systems thinking in its emphasis on underlying relationships and its search for the internal logic of a system. (In this context, it is piquant that his exposition was presented at the original *Anthropology Today* conference in 1952.) The seven principles identified by Soviet theorists can—to a large extent—also be discovered in the works of Lévi-Strauss. What reference is made here to his work suffers because of the briefness of the discussion. For example, Shimkin says that Lévi-Strauss's distinction between mechanical and statistical models is inconsistent with current understanding of cognition and behavior, in which memory and anticipation play a large role. Lévi-Strauss's works, however, give rise to three possible interpretations of the distinction between mechanical and statistical models: (*a*) a connection with the difference between ideal and real behavior; (*b*) a connection with the presence or absence of an explicitly formulated system of rules; and (*c*) a connection with a different kind of—discipline-tied—approach (Lévi-Strauss 1958:303–53). My questions are (1) Which interpretation is Shimkin's starting-point? (2) On what grounds has this interpretation been chosen? and (3) Why is the distinction between mechanical and statistical models inconsistent with current understanding of cognition and behavior? Perhaps these questions will seem out of order, but to me they illustrate the core problem I have in evaluating this article: Does it offer anything new, or does it only repeat more or less accepted general statements and conclusions?

by JAMES DOW
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I agree wholeheartedly with the authors' positive presentation of systems theory as capable of dealing with social and cultural change, productive of new theories, and leading to accuracy in theory. It is unfortunate that "systems theory" has an alien connotation in anthropology. What is being discussed here is a type of anthropological theory that has insight into social and cultural dynamics. Systems perspectives have been part of anthropology for a long time. The problem for the future is not to bring systems theory into anthropology, but to make anthropologists aware of the extent to which systems concepts already exist, to help them to find techniques to prove or disprove their theories, and, thus, to move anthropology ahead as a science. The struggle is really between atheoretical historicism and modern functionalism.

What seem to be needed at this point are guidelines for the creation of systems models that allow researchers to work with theory that has understandable, predictable, and verifiable logical consequences. The logic and mathematics of systems analysis can be taken from other fields, but the general and specific guidelines for model building in anthropology have to be developed in this field. Specific guidelines are being worked out in many subdisciplines, among them cultural ecology, social structure, and anthropological economics. General guidelines are difficult to discuss but appear in such work as that of Odum (1971) on energy, Bennett (1976) on cultural ecology, and White (1975) on culture. I have discussed the problem of integrating energy and symbolic information in systems models (Dow 1976). For example, a general guideline is that the complexity of real systems can be reduced in models by taking advantage of the hierarchical nature of the total system. Short-term equilibria can be treated as variables in a higher-order system. Thus we already should know that, proceeding from the modeling of short-term dynamics to long-term dynamics, we go from consideration of individual decisions to social norms to institutional organization to cultural-ecological adaptation to basic human ethology. The important general job is, as always, to find ways of reducing complexity while retaining validity in the models.

by JULIO CÉSAR ESPÍNOLA
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En términos generales, uno de los fenómenos más ilustrativos de la hora presente en las disciplinas y dominios especializados de la ciencia del hombre, lo constituye el *renversement des méthodes,* cuyo rasgo más característico es el paso del empleo de los procedimientos *cualitativos* a un creciente uso de los procedimientos *cuantitativos*. La comprensión de una regularidad de datos empíricos, la formulación de un modelo matemático, la demostración de su confiabilidad y la aplicación del mismo con fines descriptivos, analíticos y de predicción, constituye una fuente de interés y de fascinación para un grupo de creciente prestigio entre los antropólogos sociales.

Obsérvese que no cabe postular un retorno al monismo materialista y biológico del siglo XIX, conforme al cual Quetelet emprendió la tarea de delinear un esqueleto matemático del ser humano y Comte se propuso transformar el cerebro humano en un espejo del orden exterior. Hoy sabemos que la expresión matemática de los fenómenos socioculturales es una función auxiliar, pero nunca una meta. Tampoco se debe suponer el retorno a esa suerte de extraterritorialidad ontológica que caracterizó al ser humano antes de la aparición del método científico, según el cual el uso de procedimientos inductivos y experimentales es sólo aplicable al orden físico.

La teoría de los sistemas refleja de manera singular los cambios sufridos recientemente en la concepción general de las ciencias de la naturaleza y el desarrollo de nuevos instrumentos intelectuales aplicables al conocimiento del hombre, que permite avisorar una nueva conversión epistemológica que reconcilie el pensamiento científico clásico (ampliado por este camino) con el pensamiento de las ciencias humanas, que indagan su propia imagen. Al respecto, parece singularmente significativo y de gran alcance para el futuro que eminentes físicos y biólogos hayan venido observando con gran claridad desde hace algún tiempo que tales disciplinas son también ciencias del hombre y no sólo ciencias de la naturaleza. Como dice Gusdorf (1957:121), «Las ciencias humanas, hasta hace poco agregadas a las ciencias de la naturaleza, tienden a tomarse su desquite. En matemática, física, la crísis contemporánea de los fundamentos ha destacado el hecho de que las disciplinas positivas son un espejo del hombre; jalonan una toma de conciencia del hombre en el tiempo».

La teoría de los sistemas y otros desarrollos menos generales, pero que dan evidentemente respuestas, aunque parciales estimulantes y constructivas, sobre como el método científico puede servir para el conocimiento del hombre (vg. Auger 1952, Kaufmann 1975–77, Vendryés 1942). Contribuyen, aunque sea de manera restringuida a un sector del conocimiento, a transformar a un tiempo nuestra noción actual del determinismo y nuestra noción actual de la vida, proporcionando ese acercamiento entre las ciencias de la naturaleza y las ciencias del espíritu que es una de las tareas más urgentes de nuestro tiempo. Esta dirección de la investigación que se va imponiendo a un número cada vez mayor de estudiosos, sea cuales fueren sus convicciones filosóficas, parece ser la más válida científicamente y la más susceptible de aportar contribuciones sustantivas en el dominio de la ciencia del hombre y, por ende, en el dominio de la antropología social.

[Generally speaking, one of the most illustrative phenomena of the present moment in the specialized disciplines and domains of the science of man is the *renversement des méthodes,* the most characteristic feature of which is the change from *qualitative* to *quantitative* procedures. The recognition of a regularity in empirical data, the formulation of a mathematical model, the demonstration of its reliability, and its application to descriptive, analytical, and predictive ends are a source of interest and fascination for an increasingly prestigious group of social anthropologists.

One cannot postulate a return to the 19th-century materialist, biological monism according to which Quetelet attempted to sketch a mathematical skeleton of the human being and Comte essayed to transform the human brain into a mirror of the external order. We know today that the mathematical expression of sociocultural phenomena is an auxiliary function, never a goal. Nor can one postulate a return to the kind of ontological extraterritoriality that characterized the human being before the emergence of the scientific method, according to which inductive and experimental procedures were applicable only to the physical order.

Systems theory reflects in a singular way recent changes in the general conception of the natural sciences and the development of new intellectual tools applicable to the understanding of man. It allows us to visualize a new epistemological conversion that would reconcile classical scientific thought (thus amplified) with the thought of the human sciences, in search of their own image. In this connection, it seems uniquely meaningful and important for the future that eminent physicists and biologists have been observing with great clarity in recent times that their disciplines are also sciences of man and not only natural sciences. As Gusdorf (1957:121, my translation) says, "The human sciences, until recently annexed to the natural sciences, are beginning to get even. In mathematics, physics, the contemporary crisis in basic principles has underlined the fact that the positive disciplines are a mirror of man; they highlight man's acquisition of consciousness in time."

Systems theory and other less general developments obviously provide answers, stimulating and constructive although partial, as to how the scientific method can serve the understanding of man (e.g., Auger 1952, Kaufman 1975–77, Vendryés 1942). They contribute, although in a manner restricted to one area of knowledge, to the transformation both of our present notion of determinism and of our present notion of life, bringing about that rapprochement between the natural sciences and the sciences of the spirit which is one of the most pressing tasks of our time. This research direction, which is coming to prevail among an ever increasing number of scholars, irrespective of their philosophical outlooks, seems the most scientifically valid and the most likely to provide substantive contributions in the domain of the science of man and, therefore, in the domain of social anthropology.]

by SUE-ELLEN JACOBS
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Rodin, Michaelson, and Britan have provided an interesting and valuable perspective on systems theory in anthropology. Their summary of the four-hour discussion of the papers prepared for the World Anthropology–1977 Conference indicates the earnest effort of the participants to reach a consensus concerning the five themes "jointly identified and discussed." A sixth theme that might well have been discussed is the variations in epistemological or cultural uses of systems theory. While some attention was paid to Soviet, German, and other Western attempts to refine systems theories, none was paid to Eastern scholarship devoted to these problems.

A systems theory *might* well develop which can be applied to the solution of specific human and environmental problems. However, as the authors note, this has yet to be accomplished. Perhaps it is because theories about and ways of using systems analysis have not yet incorporated non-Western concepts of human and environmental relationships. Maruyama (e.g., 1961, 1975, 1978*a*) has attempted to do this, explaining that it is very difficult to communicate with clarity cross-epistemologically and even more difficult to achieve an integrated model or theory of a given system (or subsystem) applicable to and understandable by people from diverse cultures. I offer the following as an example of these difficulties:

Tewa concepts of relationships in the environment are typically nonlinear (Ortiz 1969), yet their model of the world as a system is so explicit that everyday life can be explained in relational terms as part of the systemic order by most Tewa adults. Intrusions into the system (which systems theorists might view as causing deviations, then as deviation-amplifying mechanisms), whether by humans or nonhumans, may cause temporary changes in certain everyday behaviors, but they will not change the nature of the overall relationships in the long run; i.e., the culture will not be destroyed by these intrusions. Hispanos and Anglos who share the same physical environment with Rio Grande Tewa Puebloans seldom share this view of life and the world, and they also seldom share a common view of these with one another. Systems approaches used in development projects in this area more often than not fail to take into account the various "harmonious" relationships perceived by the people concerned.

When multiple epistemologies truly influence systems theories and methods for analysis, then perhaps the models and theories will prove useful for problem solving in multiethnic environments. Until then, as the authors note, the potential of "systems perspectives in anthropology" remains an unfulfilled promise.

by BEATRICE DIAMOND MILLER
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As a member of the Systems Analysis panel, I well appreciate the difficulty of the task assigned to Rodin, Michaelson, and Britan. Their article presents an excellent overview of our very wide-ranging discussions and opinions. Therefore what follows is not a critique, but an expansion of some of the points they have touched upon.

No list of anthropologists who have applied systems theory to the description and analysis of various aspects of anthropological concern is complete without Count (1973), for his systems analysis of human biobehavioral evolution, or Wallace (1961). Wallace's attention to the "mazeway" and to "equivalent behaviors" broke the mold of the "replication-of-uniformity" approach that has so bedeviled the field of culture and personality. Wallace stressed the concept of individual processings, perceptions, and cognition rather than the automatic and inherently static transmission of a society's "culture" (and "personality") to its members. His contributions can be seen as running counter to the conservative approaches of systems analysts who have been so caught up with homeostasis and "closed systems" that, like many functionalists before them, they view "*the* system" and overlook what I have called the system's "component systems" (1974).

Anthropologists have been burned so badly by the fear of teleology that we tend to deny automatically any imputation of purpose or design to any social system. Yet, if we look closely we can, in fact, discover that there is an overt *purpose* to an "educational system," i.e., to educate, or to a "political system," i.e., to control. The *goals* of the people involved as components in any such systems will vary, however, according to multiple factors, including (1) where in the system individuals perceive themselves and are perceived by others; (2) with which other components they recognize similar interests, which may—or *may not*—conform to those the system "assigns" to them; and (3) what access they have to means for influencing the system's purpose and/or upsetting its equilibrium. Disconformities between the purpose of the social system and the goals of its human components (Miller 1975) are the very stuff of both gradual and revolutionary transformations of social systems.

In terms of application, recognition of such disconformities calls not simply for acknowledging the need for "perspectives that are precisely based on local data," but for reemphasizing the individuals who are the targets or "clients" of various formal systems (Miller 1977). Focussing on the individuals—and on the formal and informal systems of which they are *component-members,* with some voice in decision making (Miller 1974)—can provide a "bottom-up" corrective to the customary approaches which work *down* from some formal system through its so-called subsystems. Neglect of this corrective is partly responsible for the most dismal failures of well-intentioned "service systems." Habermas (1973) and Kuenzlen (1972) had valid criticism of urban systems models which lacked "upward" and "horizontal" flows that originated from *individuals* and *groups of individuals* with similar and conflicting goals and values.

One of the advantages of general systems theory is that the analyst defines the system and its level on the basis of the problem(s) to be studied. A cell biologist views a single cell as a system, while an astronomer so examines our "solar system." For their widely disparate purposes, both define their systems. Within the systems' context, they have no difficulty in communicating their systemic characteristics. For the cell biologist's purpose, the solar-system *level* may be inappropriate, but the same *approach* is valid. Thus, although Rodin et al. state that "to apply 'system' characteristics to individual actors is an ecological fallacy," to apply "system" characteristics to individuals can be a physical, social, or mental health *necessity.* The nature of the problem the analyst is examining may well require considering individuals as systems, or as component-members of more inclusive systems, or as target components of still other types of systems (Miller 1972). The intimate relationships between *problem, level,* and *system* for analysis still remain largely unexplored.

by PHILIP C. MILLER
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This article will, I hope, stimulate anthropologists to inquire more deeply into systems analysis. While the authors are restricted by their nonmathematical exposition, many substantial areas are referred to. I shall comment on two of these:

1. As a methodology, the systems approach can be useful in coordinating interdisciplinary research. Disciplinary paradigms

often restrict the opportunities for dialogue. Through systems analysis it may be possible to uncover a common ground of shared interests. The research of specialists can then proceed within the framework of a comprehensive research program. The coordinating function of systems analysis is particularly useful in dealing with environmental and natural-resource issues. The limits to this structured eclecticism stem from a lack of unitary organization or purposeful design in the object of study.

2. Mathematical systems theory may aid the development of quantitative and ecological anthropology. Mathematical models of dynamic processes offer a succinct presentation of concepts where description is too voluminous and diffuse and theory is as yet incipient and unwieldy. The price that is paid is the loss of the richness of ordinary language.

Models are often useful tools for developing analytical approaches to broad-ranged problems. Models in theory also play an important role by phrasing alternative explanations and suggesting avenues for empirical tests. In modelling, several allied fields may be drawn upon. The data requirements suggest the use of quasi-experimental designs in field research and multivariate statistics. Computer simulation is a very useful approach for understanding multiequation systems. Analytical fields such as optimal-control theory and theories of stability and discontinuous change may stimulate new ideas for research.

Systems analysis should be of particular interest to ecological anthropologists. An integrated socio-ecological approach is in demand for impact assessment of large-scale public works and the study of primary resource-based communities. These fields would benefit from the integrated study of the cultural and ecological context of human populations.

by EMILIO MORAN
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The authors must be congratulated for summarizing the disparate ideas of a symposium that appears to have involved numerous participants. It is perhaps their conscientious effort to represent all the ideas raised at the conference that presents problems for a clear understanding of the contribution of systems theory to anthropology. While they touch upon practically every possible idea that is relevant to systems in anthropology, they gloss over most of them. I would like to pick up on two ideas that I was particularly disappointed to see glossed: the inherent differences between functionalist and conflict models of social systems and the relative value of quantitative vis-à-vis qualitative modeling.

The authors correctly point out that contemporary functionalist theory has gotten away from the static equilibrium-oriented models of yesteryear. They do not point out, however, that even today the functionalist approach dominant in most systems theory emphasizes the role of consensus in the maintenance of system stability. Such a view came to us via Comte and Durkheim and has permeated British and American intellectual life. A consensus-oriented approach to system stability and evolution tends to overlook the inherent contradictions in society and nature. It is probably for this reason that Forrester (1969) has described complex systems as "counterintuitive" (i.e., inadequately perceived by the human mind and leading to counterproductive actions). When one works under the assumption that consensus is the chief means to stability, one simplifies the behavior of systems beyond recognition and is unable to explain the evolution of social structures. One need not go very far in the historical record to point out that social systems can remain maladjusted for long periods and even totally disintegrate in the end. The cases of social change in the record have not always resulted from consensus but, rather, from conflict between societal segments vying for power and from the sometimes violent takeover of one such group. It is surely inadequate to say that if a group is in power it expresses the consensus of the populace. Yet that is precisely the position in which consensus-functionalism puts its defenders.

Conflict theory needs to be blended with functionalist theory if systems theory is to live up to the expectations of the authors and the rest of the intellectual community. By conflict theory I mean not a naive Marxism but, rather, the awareness of and accounting for the differentially shared values and aspirations in society that generate a constant tug-of-war between its segments. Out of this inherent conflict arise new solutions or adjustments to changing circumstances (i.e., internal change). Even response to external change is facilitated as the various segments propose alternative ways of coping with external stress. It is because of this variability that systems may perhaps be much less counterintuitive than functionalists might think. Conflict theory also accounts for the fact that societies may evolve in the direction of instability. In the tug-of-war over power in society there is no guarantee that those who gain it will have the interests of society at large in mind and that their acts will not be destructive of the social fabric. The conflict model permits one to deal with that possibility in the behavior of systems.

The other disturbing gloss in the article is a passing remark about a forthcoming trend marked by a "deemphasis on mathematical modeling and an increased concern for qualitative approaches." If anything, all that we have had in the social sciences are qualitative models. Even in the more biological branches of anthropology, practitioners of quantitative modeling and simulation have been few indeed. If systems approaches are to live up to their potential, what is needed are some serious efforts to put numbers to all those boxes-and-arrows of qualitative models. The difficulty has been that systems-oriented anthropologists have taken holism too literally and have tried to model systems too large to be practicably handled by any exact means. The result is that they have had to settle for more qualitative, and often impressionistic, remarks about the behavior of systems. The authors correctly point out that the only practicable and accurate applications of systems analysis will be in micro-situations, carefully bounded by exact criteria, with awareness of the theoretical assumptions behind such a choice and the limitations of the results for understanding large-scale systems.

One difficulty with anthropological participation in systems-oriented research that cuts across disciplines is a tendency toward complacency on our part. We tend to make an issue of the fact that our discipline has been since its inception holistic and, so the argument goes, essentially systems-oriented. While this has been a goal of anthropology, it has seldom been accomplished. The Boasian reaction to environmental determinism produced a generation of scholars that gave us minute descriptions of societies, but not of the environments which formed the context of their behavior. Specialization within anthropology since that time has increasingly produced sophisticated studies of parts of total systems. Anthropology, no less than the other sciences, needs to approach the study of systems with humility and a commitment to a task which is far more taxing of our expertise than our specialized tasks and of which the returns are, at best, uncertain.

by XTO. G. OKOJIE
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The identification and discussion of these five themes by the systems-analysis panel is scholarly and stimulating, but highly theoretical. I must agree with Belshaw that as in other anthropological subfields, isolated investigators can never accomplish as much as a group that shares ideas and results. Furthermore, qualitative anthropology is getting complex, but need we for merely academic reasons further complicate a subject that is

the whole science of man? We are all agreed, firstly, that in this science quantifiable and commensurate data are hard to come by and, secondly, that since human systems display so many contradictions their study must of necessity be multidisciplinary; but is this any excuse for turning human groups into mechanistic and perfectly specifiable systems? How, for instance, would even relatively sophisticated mathematical applications make anthropology easier for teachers, let alone students? I am in full agreement with Okediji in his reservations about the future acceptability of pure theoretical research. For us here in developing countries grappling with myriads of human problems, the focus should be on functional anthropology.

On the other hand, I agree with Miller, Shimkin, and Lowe that it is not without reason that human-ecological approaches have consistently integrated physical, biological, and symbolic aspects of life. Examples of biosocial systems involving an interplay of biological and sociocultural factors in disease abound in medicine. My work amongst the simple rural people of Ishan, Bendel State, Nigeria, gives some measure of the relationship between social stress and cardiovascular pathology. Twenty years ago, they were simple farmers engaged in subsistence agriculture. All a man needed to be recognized in his village was a thatched house and a wife and children; no one had any cash savings. Today many men aspire to leadership. They own cars, and their main occupation is as contractors engaged in government projects in three or more distant villages, rushing from one project to the other from morning to night. They own several houses, keep multiple families scattered all over the place, and come to the hospital begging for medicine to be able to sleep. Next to malaria as a local killer now comes hypertension, an affliction I never saw in my first ten years in the area.

by M. ESTELLIE SMITH
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The authors, as well as a number of the participants (e.g., Okediji, Belshaw, and Singh) in the symposium, stress the utility of systems models in the analysis of systems dynamics, particularly since these are "the special concern of Marxist anthropologists" and since such analyses would focus on "the immediate practical value of a systems perspective." One almost senses a collective cry of "mea culpa" as Harrison ("with enthusiasm"), Michaelson, R. Miller, and others assure their more pragmatic colleagues that there are, indeed, applied aspects in the work of systems scholars.

Despite this, one is also struck by the fact—though some may think this trivial—that whereas Soviet Marxists are lauded as "ahead of their Western counterparts in identifying . . . *principles* of systems analysis" (italics mine), Marxists elsewhere seem essentially concerned with local applications and empirical formulations of a programmatic type—e.g., Okediji's emphasis on promoting political stability and Singh's call for understanding the dynamics of societies, such as India, which display "great stability." Given the rather abstract, theoretical emphasis in the earlier discussions, one might wonder at the rather unbecoming haste displayed as various participants apparently scurry to suggest socially relevant applications such as social-impact studies of dams and issues of taxation conflicts.

My point is that, if Okediji, Singh, and Belshaw have legitimately chided the panel members (and I hope I have interpreted the tone of their remarks correctly), then one wonders why Soviet Marxists—who surely should be expected to be utilizing models within a Marxist mode par excellence—stress research on the theoretical questions and problems which, for them, still appear to exist in systems models, neglecting the application of the models to real systems. Perhaps, despite the enthusiasm and optimism expressed in the closing session, it is because there is still much fundamental developmental work that must be done before systems analysis can genuinely fulfill its promise. Either that, or the insights provided by the model are already so powerful that Soviet scientists are shy to apply them in situ.

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While this is a stimulating and welcome paper, it is less than satisfying. Ranging across a number of important issues, the discussion seems to avoid closure on any. Perhaps this is unavoidable in a cursory treatment of so large a subject area. My primary response is to urge careful and thorough exploration of the interface between systems concepts and anthropology. Systems theories and methods may offer a "new key" to nomothetic enterprise and perhaps provide us a means of integrating the many disciplines within anthropology. Effective development and use of this key is no small problem.

It is less than likely that publication of reports such as the present one will, of itself, "build the foundations for a more unified approach by scholars around the world." I would recall to mind a few of the more unfortunate results of the diffusion of "neutral" technologies among developing nations and make a plea for a more tempered optimism here.

My belief is that the next phase in the marriage of systems theory with anthropology should be characterized not by direct growth and rapid dissemination of information, but by a return to simpler constructs. Core concepts and techniques require standardization, correlation, and evaluation followed by gradual synthesis of a theoretical network. Models are being adapted from other biological and natural sciences with little consideration of their larger implications for our field. A rare opportunity exists for unification of anthropological disciplines through the critical assessment of the meaning and applications of systems concepts. It should be pursued before the proliferation of methods and measures has outpaced our ability to construct a guiding rationale for their use.

It is obvious from this report that anthropology as a science is now struggling with questions of just where and how systems concepts and models fit in. The present danger lies in the eager and disjointed nature of this pursuit. There is an enormous language problem. Terminologies are inconsistent, and usages range from the formal to the metaphorical. The term "system" itself has been used with a multiplicity of definition which must approach the record now held by "culture." The potential for confusion is hinted at in the present paper by the irritating shifting of reference between "systems theory" and "systems approach." A reader new to the field could not possibly come away from this discussion with a much clearer understanding of what exactly is useful about systems thinking or where to begin to get a handle on it (since the references cited in the paper are rather advanced).

While there is a need for cautious, disinterested study of the relative merits of various systems approaches and standardization of key definitions, a more important task is the articulation of infrastructure in general systems theory. Benthall is right to query, "Is systems theory a theory at all?" It is more an epistemology than either a science or a methodology (cf. von Bertalanffy 1967, Bateson 1972). We need to ascertain the scope and applicability of this body of theory to our field, and this requires an assessment of general systems theory as a discipline in its own right. Collaboration with the various guilds of systems theorists and scientists who are examining the same issues could prove most helpful for all parties.

Several major assets of systems thinking, alluded to obliquely by Rodin, Michaelson, and Britan but meriting much more consideration, are the following:

1. The systems approach accommodates change, paradox,

catastrophe, complexity—all the phenomena which prove to be the downfall of simpler or more specialized theories. Maruyama (1978*a*) has recently discussed the manner in which a new logical structure characterized by such phenomena is expanding its influence in all sciences.

2. The approach is totally flexible in its scope, domain, and range of application. The same formal concepts may be used reiteratively to construct quite sophisticated models from primitive elements. Birdwhistell (1972) has shaped the science of kinesics in this manner, adapting a few basic tools from the linguist's kit.

3. The fact that the essence of any system is behavioral or relational rather than material allows for a revolutionary manner of unitizing and measuring social and cultural phenomena. Metaconcepts such as "superorganic" and "ethos" are brought considerably closer to operational definition.

4. The approach generates not only testable hypotheses, but also operational paradigms and models within which any number of such hypotheses may be generated and ordered. The process of scientific inquiry can be much more thoroughly organized than has been true heretofore (cf. Churchman 1971).

I have found it very helpful in my own research to use an elementary form of systems theory, reduced to a set of simple, formal statements. These can be applied to the construction of a conceptual bridge across conventional anthropological methods (e.g., via isomorphic comparison). In this manner two or more methods can be taken as supplemental perspectives, and models previously considered unrelated can be viewed comparatively. The use of the term "system" is reserved for models in which analysis (preferably multi-method) has verified the presence of constraint or pattern in the data.

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The authors' hope that their paper "will spread a growing awareness of the promise of systems perspectives in anthropology" is not likely to be realized. The constraint of reporting the discussions at the World Anthropology–1977 Conference prevents the authors from performing a much greater service: writing a simple, clear, and balanced presentation of general systems theory aimed at the anthropological novice. In this respect, the paper also fails to meet a fundamental requirement of CURRENT ANTHROPOLOGY—to communicate with colleagues who are not members of an inner circle of initiates. Historical and bibliographic imbalance are evident in the overemphasis on Soviet writers (many of whose works, incidentally, have been translated into English—for references, see von Bertalanffy and Rapoport 1956–78) and the absence of references to certain seminal Western works (Boulding 1956, Ashby 1961).

A recent book of particular value to the beginning systems theorist who is also a social scientist is G. M. Weinberg's (1975) *An Introduction to General Systems Thinking*. The title highlights a major problem which Rodin et al. are aware of but do not resolve. General systems theory is not a theory at all. It is a way of thinking, an approach to dealing with the complexity of the real world, and a heuristic device for the definition and resolution of real-world problems. It is not a question of linear algebra or decision theory or Markov chains or predators. Rather, the key to understanding systems thinking is the idea of controlled isomorphism. General systems theory is the branch of science that specializes in the study of modelling. A systems thinker lives in a world of analogies. As Boulding once put it, he or she will step off the plane in Bangkok, glance around, and remark how like Pittsburgh it is. How many of our skeptical colleagues would be turned *on* instead of *off* if we could communicate to them the fundamental simplicity and playfulness of true systems thinking! Then, and only then, might they have enough conviction of the value of the approach to go on to computerized simulations and other such technological sophistications.

Rodin et al. point out that systems thinking and anthropology share a belief in holism. I will add two more articles of faith held in common: the comparative approach (seen in systems thinking as controlled-isomorphic model-building) and the processual-evolutionary approach by which both sciences express their concern with change. Systems thinking and anthropology are really metasciences. Isomorphism and the culture concept are their respective methodologies, and complexity is their common subject matter.

Weinberg, in exemplary systems-thinking style, has suggested the following analogy: the systems-theory movement is to science as ready-to-wear fashion is to haute couture. The chain-store shirt, like the stochastic model, offers the consumer a low-priced and serviceable product. A gentleman can safely send his shirt even to a European laundry, from which it will emerge boiled but unscathed. He has paid a price in style and quality for the dependability he has gained. Just so, good systems thinking must be open and comprehensible to the largely uninformed consumers who buy it for its usefulness alone. This is a requisite not only of systems models but also of papers on "Systems Theory in Anthropology." Unfortunately, the present paper lacks the common touch and thus does not fulfill the promise of its title.

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Although I subscribe to many of the ideas in Rodin, Michaelson, and Britan's paper, I shall employ these comments to describe developments in the "soft" systems methods which I feel they neglect.

In their discussion of "The State of the Art," Rodin et al. aver that "the greatest technical advances in systems analysis have occurred not in anthropology, but in the natural sciences." They ignore an important development in systems analysis, however, when they recommend that anthropologists should seek approaches "more consonant with a *quantitative* systems framework" (my emphasis), because increasing numbers of sophisticated systems researchers are turning away from rigorous quantitative methods and are developing or adopting techniques based on weak mathematical assumptions (van Gigch and Pipino 1977; Negoita and Ralescu 1975; West 1977*a*: chaps. 3, 7). An excellent point of departure for their paper would have been *Explanation in Social Science: A System Paradigm* (Meehan 1968). Meehan's perspective is compatible with anthropology as a nondeductive science and also leads one away from the deductive paradigm of science (and systems theory) which is too evident in the paper. For once weak is good, not bad, in that our often qualitative anthropological applications of this new wave of systems analysis would be unlikely to rape the assumptions underlying the tools used.

Examples of "soft" methods include the mathematics of graph theory (West 1977*b*) and fuzzy-set theory. Fuzzy-set theory was pioneered by Zadeh (1965), an internationally respected systems analyst and control theorist, in order systematically to handle imprecision (or vagueness) somewhat as probability theory helps one to confront uncertainty. In the 13 years since Zadeh's initial article, publications in fuzzy-set theory and its applications have come to number over 1,000 (Gaines and Kohout 1977). This emerging school of systems analysis now constitutes a fairly extensive, increasingly visible development (Zadeh et al. 1975, Negoita and Ralescu 1975, van Gigch and Pipino 1977, Gottinger 1973, Kaufmann 1975).

Zadeh now interprets fuzzy-set theory as a theory of possibility and observes that human decisions are based more upon

beliefs about what is feasible and the assumed meaning (e.g., value) of the alternatives than upon subjective estimates of the probabilities of events (1977*a*:1–2). Also, in appreciation of the virtues of natural languages as vague media for communication, Zadeh promotes use of the linguistic approach to fuzzy sets, "in which words rather than numbers are employed to characterize approximately the values of variables as well as the relations between them" (1977*b*:1–2). It makes great sense to interpret words, cognitive categories, and concepts as fuzzy sets (Kay 1975, Pierce 1977, West 1977*c*).

Although scientists too often presume that more precision is better than less precision, there is considerable evidence that certain fuzzy systems outperform sharp or precise ones. Mamdani and Assilian (1975), for example, have achieved excellent results by applying crude computerized fuzzy controllers to complex industrial processes. The rules in the controllers are stated using rather undiscriminating linguistic variables (e.g., the variable SPEED ERROR has seven possible values, including "positive big," "positive medium," and "positive small"), and experienced human operators formulated the rules comprising the control strategy. Mamdani and his co-workers have demonstrated that precise algorithmic controllers are too Procrustean to permit effective system control.

Dimitrov (1976) of the Bulgarian Academy of Sciences employs symbolic logic to prove that the optimal governance of social systems is fuzzy. His logic includes the following reasoning: According to Ashby's (1963:206) cybernetic law of requisite variety, any effective control of a given system must encompass at least as much variety as the system to be controlled. Hence, control of social systems which inherently contain enormous latent variety requires that the controller also be capable of manifesting immense variety. Ironically, it is the very imprecision of instructions stated in natural language and in other cultural rules which permits effective governance of social systems. The virtue of fuzzy rules and fuzzy instructions is that each can be given multiple interpretation, thereby admitting variety and permitting enough freedom to enable control to be flexibly attuned to compelling empirical conditions. Therefore, it is desirable for much of any culture to be fuzzy and for anthropology to reflect its subject of study by being equally fuzzy.

I am convinced that as anthropologists who employ systems analysis make increasing use of "soft" systems methods such as fuzzy-set theory, they will discover that the wedding is comfortable as well as expeditious. Rodin, Michaelson, and Britan's neglect of this subject is explained, though not justified, if the panel discussion on which they report largely avoided "soft" systems methods.

Reply

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For myself and my co-authors I would like to thank all those who commented on our report. I am especially pleased to see that several commentators expand on points we raised. Some, however, criticize us for not doing what we said we would not do. For their benefit I shall take this opportunity to restate our purpose.

Our report organized written and verbal materials around themes that ran through a professional discussion. In several places, we amplified the documents by reference to published sources or to examples from our own knowledge. Our aim at all times, however, was to stick to the substance of the conference. As we stated quite clearly, this report is neither a review of the literature on systems theory in anthropology nor a primer in systems theory for the novice ethnographer. A portion of the discussion may by-pass readers less familiar with the specifics of systems theories and modeling techniques. On the other hand, bound by the genre, we may not have satisfied the specialist. We interpret the extent to which the comments complement each other as a step towards fulfillment of our further intent to stimulate the exchange of ideas on systems theory in anthropology. Several particularly interesting points were raised to which I will respond.

The impact of systems thinking in the development of Western science constitutes, in the opinion of many, nothing less than a paradigmatic revolution. The earliest conception of systems grew out of empirical studies of simple homeostats conducted near the beginning of the 20th century. Anomalous observations, such as those of embryonic growth and differentiation or of cyclical oscillations in relative numbers of individuals in predator and prey species, led scientists to question the prevailing notion of the clockwork Newtonian universe. Closer to the social sciences, Freud's thinking, for example, was clearly proto-systemic, as he developed a model of the human psyche characterized by energy flows, differentiation, and conflict among subsystems of the mind. This progressive shift in epistemology has left few disciplines untouched.

We thank Espínola for his statement, which places these events in historical context, and for drawing out the implications. Not only have the humanistic sciences become "harder" as a result of the systems revolution, but the physical sciences are becoming humanized. Thus we do not see any inherent contradiction, as do several commentators, in urging the development of both quantitative and qualitative methods of model building. Furthermore, in elaborating the paradigm, inductive and deductive reasoning must proceed concurrently, each to the benefit of the other. As I infer from Espinola's discussion, the *renversement des méthodes* which has resulted in the increasing use of quantitative techniques need not imply or necessarily produce a dehumanizing trend towards sterile materialistic models. Properly understood, the use of quantitative and deductive techniques may well have the opposite effect.

Maruyama's (1978*a*, *b*) references to his own efforts to increase awareness of systems models of growth and differentiation and of self-organizing systems are a case in point. Although his neologisms can be baffling at times, the notion that a universe tending towards increasing entropy contains subsystems with the capacity for self-organization and for increasing levels of complexity (negentropy) is a significant shift in scientific epistemology predating his own work. Contemporary theories of evolution rest on this paradigm. Among others, Buckley (1967), von Bertalanffy (1967) and Rapoport (1966) have dealt with aspects of negentropy in social systems. Within this framework, Maruyama's observation of the importance of positive feedback, even of relatively short duration or low intensity, in stimulating profound changes in the state of a system is most useful. It is perhaps this type of phenomenon which will prove valuable in analyzing revolutionary social change of the order discussed by Moran. These are the types of problems with which mathematicians developing catastrophe theory are engaged, though their proofs remain controversial.

Dow and VanDeusen appear to be asking one of the questions which concerned the panel but was not dealt with in much detail. There is unquestionably a large number of attractive systems models being batted around. Specifically how to adapt appropriate models for use in anthropology presents problems. As Dow and VanDeusen suggest, it is largely a matter of careful scholarship, selectively choosing models appropriate to problems, thoroughly understanding both the properties of the model and the analytic techniques it entails. Dow's strategy of

reducing complexity, of which we would like to have seen more, seems to be similar to VanDeusen's strategy of building from simple elements. Because of the brevity of his comment, however, Dow's easy transition from individual decisions to social norms is troublesome. It implies a theoretical position on the relationship between cognition and behavior and glosses many difficult methodological problems.

The triangulation of methods and of systematic searching for structural isomorphism suggested by VanDeusen was mentioned in passing in the panel. Elsewhere I have advocated this strategy for urban ethnographers (Rodin 1977). I have found that the use of various statistical methods for survey and secondary data combined with more qualitative participant-observation, mapping, formal elicitation of categories from informants, and use of historical documents provides verification of intuitive models as well as identifying incongruities suggesting errors in the models. It is not entirely clear from the context, however, how VanDeusen is using the phrase "isomorphic comparison." If he is referring to the comparison of systems on the basis of mathematical homology, he is perhaps aware of the conservative position taken by Conant and Ashby (1970). They qualify the level of valuable comparison by stating that homology is acceptable provided that it is confined to a comparison of relations between the subset of regulatory elements and the system as a whole. That is, they regard as valid comparisons between systems on the basis of how they are controlled, rather than total-systems comparisons.

We must thank West for his exposition of the uses of mathematical models based on weak assumptions, such as fuzzy-set and graph theory. He usefully distinguishes between quantitative methods and mathematics per se, a distinction which was perhaps not well drawn in the report. His discussion of fuzzy control is especially relevant in view of our comments above and of Moran's discussion of the heterogeneity of beliefs and values and noncongruence of interests in large-scale, if not all, human societies.

The large technical literature to which West alludes would cause us to question Jacobs's interpretation of Tewa world view as completely explicit. Even though any Tewa adult can explain everyday life in relational terms, one is tempted to wonder whether this represents actual decision-making processes or ex post facto rationales. That the Tewa view the world as constructed of harmonious nonlinear relations does not to our mind constitute sufficient reason to label them non-Western systems theorists. In the course of the panel Dobbert, and in a previous volume of this journal Shweder (1977), among others, have suggested methods for modeling native cognitive categories and styles. The stability of such phenomena over time is also subject to investigation.

Systems theories do not assume that intrusions and disturbances necessarily destroy sociocultural systems. This is precisely the domain of control theory, one of which is discussed by West. The fact that Tewa, Anglos, and Hispanos can occupy the same space without significantly influencing one another suggests that each ethnic entity is strongly bounded, constituting a discrete subsystem. The very maintenance of such social boundaries implies a mechanism of control. Yet, we find it hard to believe that Tewa have devised or evolved no response to the presence of other cultural groups. Rather, Tewa relational propositions may serve to frame, control, and limit the inevitable intrusion of Anglo and Hispanic influences on their lifeways. As Moran implies, a system may persist apparently—and we emphasize *apparently*—unchanged until it suddenly disintegrates. On the other hand, the conditions for long-term stability without loss of adaptability remain important issues to be studied. The Tewa may provide an example.

Moran is quite correct in identifying the study of conflict as an important priority in anthropological research and in tweaking our complacency adhering to the ritual recitation about anthropology as a holistic science. There are a few points that require some expansion. The multivocality of symbols and their differential acceptance among parts of the same society are familiar to many ethnographers. Furthermore, ambiguity is a characteristic of human communication. These factors do indeed relate to underlying or even obvious stresses in societies leading in some cases to conflict. Within a single society, the presence of a variety of adaptive patterns may create strain at one point in time but supply useful alternatives when conditions change; that is, adaptive potential may increase through internal heterogeneity.

De Ruijter wonders why Lévi-Strauss was not singled out for attention. His own choice of words contains the germ of our answer. Lévi-Strauss's great contribution was in recognizing the way in which underlying relational principles order cognition across several domains, in directing anthropologists to distinguish between real and ideal, and in giving impetus to formal analysis of rule systems. His structuralism, however, lacks a means of accounting for conflict and for contingent behavior. It is significant that de Ruijter states that the work of Lévi-Strauss "gives rise to three possible interpretations of the distinctions between mechanical and statistical models." Lévi-Strauss himself does not offer these interpretations. We are not sure what de Ruijter means by the third, " a connection with a different kind of—discipline-tied—approach." With respect to his particular questions, Shimkin and Lowe have personally communicated the following:

> Shimkin and Lowe's use of mechanical versus statistical models derives from Lévi-Strauss's (1953) "Social Structure." The point they were making is that both types of models are intrinsic to both cognition and behavior and that one should not be regarded as more fundamental because each implies the other. Underlying statistical models is a sample space of probabilities (Feller 1968), and underlying that is a sharp conceptual typology. But in constructing mechanical models one selects from an indenumerably infinite number of possibilities and, as a result, can never be certain a particular model will be adequate. It is here that the computation limitations of human beings also come into play. Further, it is known from Gödel's proof that no mechanical model will completely capture reality. These factors of arbitrariness and incompleteness introduce a necessary uncertainty in the working of conceptual models in the real world—an uncertainty ineluctably statistical. (This is reflected in a theorem of game theory which states that in games of imperfect information mixed strategies, not pure strategies, are usually optimal.) The point is not that mechanical and/or statistical models of human activity cannot be created, or that they may not be useful in some cases, but rather to emphasize the interactive nature of both cognition and behavior.

Beatrice Miller raises several of the same points that interest Moran. In modeling a system, one must be careful not to confuse manifest purpose with latent function, not to attribute collective goals to component individuals. Political scientists have sought to characterize different kinds of consensus, but this need not deter us from recognizing aspects of conflict, competition, and cooperation among disparate cultural or interest groups. It is this point which Miller may have misunderstood in our reference to ecological fallacy. Not attributing the aggregate's purpose to individuals in no way denies or obviates attribution of purpose to individuals. The relatively little discussion given over to the study of individuals as systems simply reflects the composition of the panel, as Miller may remember. We were by and large social scientists trained to examine social groupings and their cultural doings. A group of psychologists and clinicians might have dwelt more on the level of interest to her. The importance, however, of social and cultural change in influencing not only observable behavior, but emotional and physiologic processes internal to individuals cannot be denied. Okojie has presented an immediate and pressing example of the need for applied research in psycho-physiological process under conditions of sociocultural change.

There seem, finally, to be two general categories of critique leveled at systems work in anthropology. These appear in one

or more forms in several of the comments and have appeared elsewhere in journal articles and exchanges. They seem to be more or less limited to anthropology; we see them less often in the sociological, psychological, and related fields, even though the substance of their concern is often similar to our own. The double-barreled charge of functionalism and deductive reasoning is often aimed as blanket criticism of systems analyses. We ought to reexamine the emotional and rational baggage associated with the place of functional analysis and of deductive vs. inductive reasoning in anthropology.

The tension between functionalism and structuralism certainly pre-dates the present context and may be traceable to the debate between Malinowskian and Radcliffe-Brownian schools. Few anthropologists today, even those avowedly concerned with producing functional accounts (e.g., Goldschmidt 1966), would maintain that all behaviors, institutional forms, or beliefs necessarily contribute to social cohesion or increased adaptive capacity. Much of what people do is either irrelevant to or destructive of such, if viewed functionally. Studies of conflict, competition, and change are addressed to this point. Functional analyses thus ought to be critically evaluated in terms of the ethnographers' dispassionate willingness to report things as they are. One can often detect a certain defensiveness, in ethnographic reports of beliefs or practices which harm health or close off the potential for adaptation, in the appeal to the implied "benefit" of promoting group cohesion. Thoughtfully conceived, a functional systems analysis is no more prone to this sort of naiveté than alliance, exchange, or other modes of analysis.

A further point to be raised is the ready identification by several commentators of systems and functional analyses as one and the same. While systems epistemology often does inform neo-functionalist analyses, it need not. Systems thinking occurs in many theoretical settings quite divorced from narrowly functional concerns. Critics who make the association mentioned expand the notion of functionalism to mean any examination of the relationship between beliefs or behaviors and environmental conditions or demographic factors. By that definition, nearly all social science is functionalist. This does not deny the importance of cognitive, symbolic, and semantic studies of myth, literature, and mental life per se. Rather, the two modes are properly complementary. Unless we are willing to take the extreme stance that causal investigations of complex phenomena are somehow impure, the co-development of functional and structural analyses, with or without systems epistemology, seems assured. Since systems methods (for historical reasons in Western societies) are readily adapted to broadly functional concerns, this area has received more attention. On the other hand, studies of symbolism, ritual, and cognition have benefitted greatly through infusions of cybernetic and information theory.

The second category of critique derives from the antiquantitative bias of some cultural anthropologists. Here again, the quibble is more apparent than real. Statistical methods and formal mathematical modeling may equally as well generate as test hypotheses. Seen holistically, the research process is both inductive and deductive. While purely deductive studies may not satisfy the creative appetites of many of us, if we do not pursue the hypotheses motivated by our theories, however elegantly induced, we will be saddled with untried speculations. Nor is deductive reasoning culture-bound. Having read, admittedly in translation, several Ming Dynasty philosophical tracts, I am convinced that deductive argument is not a uniquely Western mode of thought.

A more pragmatic point concerns the realities of academic life. Funding agencies increasingly prefer highly specific studies of a practical nature. Our non-Western colleagues are faced with immediate needs for useful information to guide development. Systems epistemology is an integral part of the planning process. Anthropologists did not participate early enough in this enterprise in the West, and we, especially our poor, are paying for it. Ideally, a balance can be struck between the requirements of applied research and the continuing need for theoretical advances. Without the latter, planning in non-Western contexts can only lead us to repeat our mistakes.

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