



Anthropological Theory, Cultural Pluralism, and the Study of Complex Societies [and
Comments and Reply]

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Anthropological Theory, Cultural Pluralism, and the Study of Complex Societies¹

by Leo A. Despres

I

JUDGING FROM RECENT DISCUSSION in the literature and at professional meetings and conferences, a problem has developed in anthropology, and it seems to be taking on the characteristics of a major crisis. The problem involves the impending extinction of the so-called primitive societies in which anthropologists have done most of their work and the growing realization that anthropology's future as a scientific discipline depends upon the ability of anthropologists to define and undertake meaningful research in the so-called complex societies.

That this problem is taking on the characteristics of a crisis for the discipline is evident by the anxiety it seems to have generated in certain quarters. To cite but one example, Lévi-Strauss has recently stated that anthropology will survive in a changing world only "by allowing itself to perish in order to be born again under a new guise" (1966:126). In elaborating upon this somewhat paradoxical statement, he suggests that wherever native cultures have remained to some extent morally intact, "anthropological research should be carried out along traditional lines." However, wherever native cultures rapidly veer toward our own, "anthropology, taken over by local scholars, should

adopt aims and methods similar to those which, from the Renaissance on, have proved fruitful for the study of our own culture." If I interpret this statement correctly, the study of culture from the "outside" is anthropology, but the study of culture from the "inside" is an altogether different kind of enterprise. In other words, anthropology cannot undertake the analysis of complex modern societies without undergoing a very fundamental transformation in its general perspective and approach.

To state the issue differently: it has been assumed that anthropology has a distinct contribution to make to the study of complex societies (e.g., Mitchell 1960; Eisenstadt 1961; Janowitz 1963; White 1965).² However, it is also suggested that anthropology is not equipped to undertake the analysis of complex societies without undergoing corrective surgery with respect to the theoretical and methodological "deformities" that have developed over the years as a result of anthropology's preoccupation with simple societies and cultures (e.g., Eisenstadt 1961; Gluckman 1961; Janowitz 1963; McEwen 1963; Gluckman and Eggan 1966).³ More specifically, it is argued by some sociologists (e.g., Janowitz 1963:139; Faris 1964:31-32), and even by some anthropologists (e.g., Voget 1960:957-60; Worsley 1961:218-19; Cohen 1965:58; Lévi-Strauss

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This article was submitted to CURRENT ANTHROPOLOGY 22 VII 66 and was sent for comment to 42 scholars, of whom the following responded with written comments: Michael Banton, John W. Bennett, Ronald Cohen, Munro S. Edmonson, Karel Fojík, Peter C. W. Gutkind, Marvin Harris, Henning Siverts, Julian H. Steward, and Norman E. Whitten, Jr. These comments are printed in full after the author's text and are followed by a reply from the author.

¹ An earlier draft of this paper was presented at the Washington University (St. Louis) Conference on Anthropology and the Study of Complex Societies, January, 1966. Although the manuscript has been revised for publication, it continues to address itself to the problem posed by the anthropologists who organized the Conference: "What can anthropology contribute to the study of complex societies?" I want to take this opportunity to thank John W. Bennett, Alvin W. Wolfe, Norman Whitten, all of Washington University, and Iwao Ishino, of Michigan State University, for the helpful suggestions they offered me during the course of the Conference. I also want to thank David Schneider, of the University of Chicago, and Sidney Peck, of Western Reserve University, for having provided me with many useful comments on subsequent drafts of the manuscript.

² Much of the recent discussion concerning the anthropology of complex societies has occurred at professional meetings. For example, see the abstracts of papers by Anderson (1966), Frucht (1966), Rodnick (1966), and Sjöberg (1966).

³ Frequently the call for corrective surgery, both with respect to theory and methodology, is expressed by anthropologists who have been working in urban settings. See, for example, the abstracts of papers by Fried (1965), Kenny (1965), Gutkind (1966), and Wax (1966).

1966:126), that anthropology no longer has justification for maintaining its boundaries as a special discipline. To the extent that the future of anthropology is perceived in terms of this problem, a crisis exists within the discipline.⁴

This paper is in the nature of a diagnostic essay on anthropological theory and the study of complex societies. Considering anthropology's well-known factual bias, I approach this task apprehensively but with the conviction that an attitude of mind needs to be expressed with respect to the anthropology of complex societies. I hasten to add that I have not arrived at this conviction for want of a generally agreed upon definition of what it is that constitutes a complex society. Although such a definition would be generally useful (Schneider 1961; Leach 1961), I do not consider the lack of one to be a critical issue in the present context. However, the implication that the study of complex societies (regardless of their definition) requires the development of a special kind of anthropology, or that it requires that anthropology be given up entirely, is a critical issue.⁵ Given its comparative perspective, it would be ironic, to say the least, if "the study of man," or "the study of culture," were so lacking in theoretical sophistication and methodological acuity that it could not subsume the study of different types of societies within one general epistemic framework. And this is precisely what is being implied in much of the current discussion on the anthropology of complex societies.

My clinical approach to the problem may be summarized as follows:

First, to assert that anthropology has a distinct contribution to make to the study of complex societies is, to my way of thinking, to put the cart before the horse. More to the point is the question, what does the study of complex societies have to contribute to anthropology? Instead of asking ourselves how anthropological theory and methodology can be adapted to the study of complex societies, perhaps we ought to be asking ourselves what kinds of research in such societies might contribute to the development of anthropological theory.

Second, while theoretical and methodological deformities resulting from anthropology's preoccupation with simple societies and cultures do, I believe, exist, they may prove less of a handicap if they are analyzed with reference to the kinds of data that anthropologists

⁴ This problem is posed most frequently with reference to social anthropology. For example, Faris (1964:31) recently concluded: "Social anthropology . . . is mainly sociology, and there is little distinction between the two fields other than that resulting from the accidental and scientifically irrelevant differences in the routes by which the two fields came to their present interests." Although Gluckman and Eggan (1966:xxviii) want to insist that "customs" (culture?) constitute a continuing focus of interest in social anthropology, they note that most of the contributors to the A.S.A. monographs do not seem to share this interest. In fact, they conclude that as social anthropologists tackle the problems of urban societies, problems shared with sociologists and political scientists, ". . . the *social* part of the title 'social anthropology' will begin to outweigh the *anthropology*." Perhaps sociology is what Lévi-Strauss had in mind when he suggested that if anthropology is to survive in a changing world it must allow itself to perish and be born again under a new guise.

⁵ The anthropology of complex societies reminds me somewhat of the situation in sociology where research in various settings has generated a proliferation of sub-disciplines. Some of these subdisciplines, medical sociology for example, appear to be quite removed from the central problems posed in general sociological theory.

might want to collect in complex societies in order to develop anthropological theory.

This approach derives from a point of view which includes: (1) the epistemic assumption that culture is a dimension of the human condition which may be analytically differentiated for the purpose of scientific investigation; (2) the maxim that culture, and not human behavior or social relationships, should be the substantive focus of anthropology; and (3) the axiomatic proposition that the comparative study of different cultures (e.g., simple, complex, tribal, peasant, rural, urban, etc.) is a *sine qua non* for the existence of anthropology as a science. It follows logically from this point of view that the comparative study of cultures cannot proceed without some general theory of culture. It also follows that any general theory which precludes from its purview the cultures of complex societies will severely impair the development of anthropological science.

In short, it appears to me that the most problematic issues raised by "the anthropology of complex societies" are issues of a theoretical nature. Has anthropology achieved a level of theoretical maturity which will permit general theoretical orientations to be related to specific propositions having to do with the cultures of complex societies? In other words, is it possible to delineate conceptual frameworks more or less consistent with some general theory of culture and capable of defining particular problems of theoretical significance with respect to the cultures of complex societies? In attempting to provide answers for these questions, it will serve the purpose of clarity to begin with a brief history of the problem under consideration.

II

The current and relatively widespread argument that anthropology no longer has justification for maintaining its boundaries as a special discipline is hardly new. It seems to have appeared just after the second World War, simultaneously with a cultural revolution in the underdeveloped world, the application of anthropology to the solution of social and economic problems, and the emergence of large governmental and private funding agencies.⁶

In the early 1950's, foundations began to seek out and support large projects involving cooperation between the several social science disciplines. In response to this trend, the Wenner-Gren Foundation provided funds with which Gillin, Hallowell, Murdock, Becker, Newcomb, Parsons, and Smith were "to explore the possibilities of interdisciplinary integration in the human or man sciences." While the anthropologists of this group were not completely prepared to give up the ghost of anthropological science, they were at least willing to consider the possibility, and their reasons were quite explicit (Gillin 1954:3-4):

The authors of this book, in common with many other specialists and scientific organizations, have faith in the application of science to human affairs, and they do not feel that either the alleged impossibility of a science of human social behavior or the supposedly inevitable delay

⁶ The same argument exists with respect to sociology (e.g., Homans 1964; Wrong 1961).

in its development is to be taken for granted without a close inspection of the situation and a careful consideration of alternatives and possibilities.

As a result of their careful consideration of alternatives and possibilities, Gillin proposed (p. 5) a "Federal Union" of the specialties dealing with human behavior.

If Gillin, Hallowell, and Murdock were somewhat apprehensive about the future of anthropology and its contribution to the study of human problems in complex societies, John Bennett was not. In a personal testament based on his experience in interdisciplinary research and teaching, Bennett (1954) rejected Gillin's notion of a "Federal Union" of the specialties and argued for the necessity of a unified science of man. He suggested (p. 173) that

The anthropologist who participates in any way in the new interdisciplinary movement described previously, is likely to reject this descriptive-holistic or "phenomenological" version of the cultural variable.... If he studies social relationships in modern society and its institutions, as he is likely to do currently, he soon discovers that he cannot assume that his subjects are simple bearers of culture who are learning and interacting in the face-to-face group atmosphere.

The argument concerning the existence of anthropology as a special discipline seems to derive its force from three principal criticisms: (1) the major source of anthropology's substantive data (i.e., preliterate societies and cultures) is rapidly drying up; (2) anthropology has not developed a precise empirical methodology capable of solving the problems of validation; and (3) anthropology has not produced any rigorously deductive theoretical frames of reference that are relevant to the analysis of complex societies.

These criticisms can be dealt with in one of three ways. They can be dismissed, as Janowitz dismisses them (1963:139), on the grounds that they carry all the overtones of a meaningless jurisdictional dispute. Among other things, the difficulty with Janowitz's solution is that the jurisdictional dispute exists, and it involves much more than a simple division of labor or the organization of academic departments (Bennett and Wolff 1956). Moreover, it is unlikely that the jurisdictional dispute will be solved by ignoring it.⁷ Another way to deal with these criticisms is to accept them and expand the discipline in the direction of fusion with related sciences. The difficulty with this solution is that it has not been taken seriously before, and there is no reason to believe that it will be taken seriously now. A final possibility is to face up to the criticisms, evaluate their validity, and determine whether or not surgery is possible without killing the patient.

The argument that anthropology's vineyard for the collection of substantive data is rapidly disappearing can be dealt with rather quickly. Preliterate societies and cultures are indeed being rapidly transformed into modern nation-states; but the conclusion drawn from

this fact is dependent upon a misconception of anthropological science as well as a myth which anthropologists themselves have helped to perpetuate. The misconception has to do with epistemology and theory. As I hope to demonstrate, there are no epistemological reasons why anthropology has to be practiced in simple societies, or why data gathered in complex modern societies cannot be made relevant to anthropological theory. As for the myth that anthropologists are exclusively interested in simple societies, this can be dispelled by the facts of history.

Anthropologists have long been interested in the data of complex societies. Sir Henry Maine's *Village Communities in the East and West* (1871) could be considered as the forerunner of a great number of anthropological studies of complex societies, many of which have become classics in the sense that they have contributed significantly to the modification of concepts or the formulation of research problems in anthropology as well as in related disciplines. Among the classics, one could probably include Peake's study of the English village (1922); the Lynds' study of a small American city (1929); Herskovits' research on village life in Haiti (1937); the work done among the French Canadians by Horace Miner (1939); Fei's research on Chinese peasants (1939); the study of peasant families in Ireland by Arensberg and Kimball (1940); Redfield's work in Yucatan (1941); the study of caste and class in the American South by Davis and the Gardners (1941); Whyte's work with street gangs (1943); and Embree's research in a Japanese village (1943).

It is not within the scope of this essay to trace the impact which these studies have had on subsequent research. However, lest it be concluded that this particular tradition of research has been confined mainly to the "backwashes" of complex societies, the work of W. Lloyd Warner and his associates ought to be mentioned. Conducted over a period of some 20 years, this work consists of numerous studies carried out in various American communities located in different regions of the country. Taken together, these studies represent an effort to delineate the social and cultural complexities of American society in terms of representative communities (Warner 1953). More recently, a similar effort was made with respect to Puerto Rican society by Julian Steward and his associates (1956).

Finally, to dispel the belief that anthropology developed primarily out of an interest in simple societies and cultures, still another collection of anthropological literature ought to be mentioned. It is that collection which has to do with civilizational studies, including studies of historic as well as contemporary civilizations. Inasmuch as civilization was conceptualized as the uppermost stage of cultural development in practically all of the early evolutionary schemes, the study of complex societies was implicit in much of 19th-century anthropological thought. Apart from the subsequent reaction to evolutionary theory, anthropologists have continued to be interested in the phenomena of civilization. This interest constitutes the major focus of classical archaeology and Old World cultural history. Moreover, as Mandelbaum has noted (1956:203-25), this interest is reflected in much of

⁷ Also, I doubt that many anthropologists would be completely satisfied with Janowitz's view that anthropology has integrity by virtue of its holistic approach to its subject matter and its qualitative methodology. This conception makes of anthropology a "methodology" and not a discipline with substantive and theoretical concerns.

Kroeber's work (e.g., 1952b:329-416). It is reflected also in studies of national character (Benedict 1946; Mead 1951) and in studies of national culture (Lowie 1945; Métraux and Mead 1954). To conclude, in the words of Mandelbaum (1956:221): "The notion that anthropological method was perfected on simple and static primitives and is best used in studying primitives is . . . a misleading one."

The argument that anthropology has not developed a methodology capable of solving the problems of validation is not fully supported by facts. In a recent and rather extensive review of the anthropological literature, Bennett and Thais (1964) have documented the use of a wide variety of methodologies by anthropologists working in simple as well as complex societies. They conclude, however, that anthropologists are not particularly interested in methodological polemics: rather, they are content to employ whatever methodologies that appear to be useful to them in their pursuit of problems.

The methodological argument cannot be dispelled by facts alone. To my way of thinking, methodology is fundamentally a matter of communications.⁸ It is comprised of techniques and procedures which are designed to enable the researcher to communicate something precise about what he observes. An empirical or "scientific" methodology is comprised of procedures which enable the researcher to communicate with such precision that what he has to say about what he observes is accepted as "truth" and, at the same time, can be checked by subsequent observers. Mathematics, sampling techniques, questionnaires, survey schedules, and computer technology facilitate scientific communication. However, their use is neither a prerequisite of science nor a substitute for the ability and the integrity of the researcher.

Viewed in this perspective, methodology is part art, part logic, and part technology. All three of these components are clearly evident in the intellectual craftsmanship of the scientist (Mills 1959; Hammond 1964). Problem formulation, particularly, involves elements of artistic expression. The linkage between theoretical insight and a logically deductive arrangement of propositions is a creative process stimulated by the observation of the empirical world. The verification of this linkage by the application of technology is primarily a problem of logic (i.e., techniques have to be logically consistent with or applicable to the problem under investigation). This too requires creative skills.

It seems to me that the argument as to methodology boils down to one or more of three possible charges: (1) anthropology is lacking in creativity; (2) anthropology is lacking in technology; or (3) anthropology is lacking in theory. To my knowledge, there is no evidence to suggest that anthropologists are any less imaginative or creative than any other group of social scientists. Similarly, there is relatively little evidence to suggest that anthropologists as a group lack technological skills; and if some anthropologists have not been trained in the technology of modern social science, this is a problem that can be rather easily remedied by the introduction of courses, training programs, and field laboratories. However, the charge that anthro-

pology is lacking in theory appears to have substance: it cannot be readily dismissed.

As Janowitz has noted (1963:151), the imbalance within anthropology between data collection and theory construction is so obvious that it has become a caricature of the discipline.⁹ This apparent lack of theory seems to me to derive from anthropology's failure to develop a *paradigmatic* theory capable of incorporating a coherent tradition of scientific research. This is not to suggest, however, that anthropology is without theories of an *explanatory* nature.

Explanatory theories comprise deductive systems of propositions which purport to explain, in terms of either stated or predicted relationships, the properties of phenomena which are conceptually delineated for scientific analysis (Merton 1957:95-99; Homans 1964a: 811-12; 1964b:951-55). Homans (1964b:961-73) distinguishes three main types of explanatory theories—structural, functional, and psychological—and suggests that historical explanations may constitute a fourth type. A fifth type, more frequently used by anthropologists than sociologists, is the evolutionary explanation (White 1945a, b; 1959; Steward 1949; 1953; 1955). Some theories of this kind are poorly constructed and inappropriately applied; some are open at the top (Homans claims that structural and historical explanations usually can be reduced to more general psychological propositions); and some (e.g., functional theories) tend to be teleological, as Homans (1964b:965) and Davis (1959) suggest. The fact remains that explanatory theories have been used extensively by anthropologists to explain a wide variety of phenomena.

In contrast to explanations of phenomena, paradigmatic theories delineate the substantive focus and intellectual approach of a scientific discipline and define what is to be explained by that discipline. Such theories comprise general orientations that provide models from which "... spring particular coherent traditions of scientific research (Kuhn 1962:10-11)." These models, Kuhn relates, "... serve for a time to define implicitly the legitimate problems and methods of a research field for succeeding generations of practitioners." They are able to do this because they represent achievements "... sufficiently unprecedented to attract an enduring group of adherents away from competing models of scientific activity ..." and also because they are sufficiently "... open-ended to leave all sorts of problems for the redefined group of practitioners to resolve." As Kuhn notes, the acquisition of a paradigmatic theory "... is a sign of maturity in the development of any given scientific field." More importantly, from our point of view, the study of paradigmatic theory is what mainly prepares the student for membership "... in the particular scientific community with which he will later practice." In short, paradigmatic theories are what make the development of scientific disciplines possible.

⁹ This charge is leveled at anthropology not only by sociologists but also by anthropologists. For example, in reviewing some of the criticisms that have been made of anthropological contributions to the study of complex civilizations, Mandelbaum (1956:218) confesses that anthropological theory is in such a rudimentary state that even such elementary concepts as that of diffusion have yet to be developed. Sociologists have been similarly critical of their own discipline (e.g., Williams 1958; Homans 1964a; Sorokin 1965).

⁸ This is essentially Redfield's point of view (1948 and 1953).

In the absence of paradigmatic theory, it is difficult for a coherent tradition of systematic scientific research to emerge. Thus, two courses of development tend to occur more or less simultaneously. On the one hand, as each individual is left to his own resources to define the legitimate problems and methods of his discipline, and thereby to focus attention on almost any phenomena that happen to stimulate his scientific curiosity, research activity is quite random. On the other hand, some individuals will attempt to synthesize a rapidly accumulating body of discursive, but not completely unrelated, data. However, until a sufficiently unprecedented paradigm emerges, individual efforts to synthesize will tend to resolve into a furious dialogue between adherents of paradigms largely borrowed from other more firmly established disciplines.

Both courses of development are clearly evident in the history of anthropology.¹⁰ The first, characterized by research that is lacking in paradigmatic focus, is especially exemplified by the Boasian period in American anthropology. During this period, American anthropology emerged as an ethnographic science. As such, it did not present a body of theory with which succeeding practitioners could orient their research systematically to problems of some theoretical significance. This is not to suggest that all those who labored under the Boasian influence failed to accomplish anything worthwhile, but rather that all too often they simply collected data with a very limited sense of problem and practically no theoretical perspective. Thus, the period is frequently described as one of naturalistic observation and description (Bennett 1944). Those who continue to labor under this influence, and they appear to be rapidly diminishing in number, usually adopt the view that anthropology is what anthropologists do. The obvious problem is that most anthropologists do just about anything they want to do by way of research.

The second course of development, characterized by a flurry of dialogue between adherents of paradigms largely borrowed from other disciplines, has been particularly evident within anthropology during the past 30 years. The literature of this period presents a relatively continuous round of discussion concerning anthropology's substantive focus, or lack of focus. Much of this discussion was stimulated by the emergence of social anthropology and of the culture and personality movement, both of which constituted a reaction to the Boasian influence and, in different ways, represented a threat to whatever independent status anthropology had achieved as the science of culture (see, e.g., Kroeber 1936; 1943; 1949; 1950; White 1945a; 1947). Should anthropology direct its

attention primarily to the study of cultural systems, as suggested by Malinowski (1944; 1945), White (1947), Evans-Pritchard (1950), Kroeber (1952a), Steward (1955:3-77), Kaplan (1965), and others? Should it focus mainly on the study of social systems (or social relationships), following the lead of Radcliffe-Brown (1957), Firth (1951), Nadel (1951; 1957), Fortes (1953), Beattie (1955), Devons and Gluckman (1964), and other social anthropologists?¹¹ Should anthropology be concerned primarily with the analysis of human behavior as reflected in the behavior of individuals (e.g., Mead 1937; Kluckhohn and Mowrer 1944; Linton 1945; Hallowell 1953; Barnett 1953:1-16; La Barre 1954; Wallace 1961)? Or, following Voget's (1960) advice, should anthropology give up whatever self-image it may claim, merge itself with the other social sciences, and direct its attention to the study and analysis of human events?

It goes without saying that the issue of anthropology's substantive focus is far from being resolved and that a paradigm capable of unifying the discipline by providing it with a characteristic theoretical orientation is not yet in sight. In the meantime, confronted with the necessity of explaining the perplexing variety of phenomena that they continue to observe, anthropologists have become increasingly dependent upon related disciplines for theory construction. In some instances this dependency involves little more than the assimilation of borrowed concepts without any concomitant change in basic theoretical orientation. A case in point, for example, is Aberle's (1962) use of the social psychological concept of "relative deprivation" to explain the nature and character of millenarian movements which seek supernatural help. This type of dependency is of little consequence: "relative deprivation" is one of those "coordinating" concepts which articulates well with general cultural analysis (Janowitz 1963:150). In other instances, however, the dependency of anthropologists on the theoretical constructions of other disciplines had entailed not only the assimilation of concepts but also the assimilation of related assumptions. As Janowitz notes (1963:150-51), this type of convergence between disciplines involves a theoretical transformation; that is, it involves a "... transposition of an idea or set of concepts from one discipline to another which has the consequence of fundamentally restructuring an intellectual approach."

A transformation of this order has been going on between anthropology and sociology for the past 30 years. It is characterized by a shift in focus from the

¹⁰ In developing his theory of scientific revolutions, Kuhn is primarily concerned with the physical sciences. He notes (1962:15), however, that it remains an open question as to what parts of social science, if any, have yet acquired paradigmatic theory. As I understand the concept, I would think that the stimulus-response model, from which several theories of personality have been derived, would qualify as a paradigmatic focus in psychology. Similarly, the model of the market-organized system has provided economics with a paradigmatic focus. I also think that a strong argument can be presented to the effect that the voluntaristic theory of action (Parsons 1937), derived from the work of Marshall, Pareto, Durkheim, and Weber, has provided sociology with a paradigmatic focus in the sense that most sociologists seem to be interested in some aspect of social system analysis.

¹¹ It is difficult to determine precisely where many social anthropologists stand in relation to this issue. A good example of this difficulty is the recent series of discussions between descent theorists and alliance theorists with respect to the study of kinship (see e.g., Schneider 1965). The paradigmatic focus of the descent theorists is social structure defined as a network of social relationships. That of the alliance theorists is social structure defined as the expression of cultural norms. Thus, the descent theorists deal primarily with social system variables while the alliance theorists appear to be moving in the direction of developing a rather systematic approach to the analysis of cultural systems. Gluckman is another example of this difficulty. While Gluckman has always displayed an interest in the analysis of social relationships and social processes, he also continues to insist that social anthropology is distinct by virtue of its preoccupation with the study of customs (1965:1-35).

description and analysis of cultures to the analysis of social systems and social relationships. This shift is most pronounced in the work of those social anthropologists who have rather systematically assimilated the concepts and assumptions of Durkheimian, Weberian, and Parsonian sociology. Perhaps one of the best examples of this convergence can be gleaned from the works of S. F. Nadel.

Nadel's contributions to anthropology span a period of more than 25 years. His monographic works (1942; 1947; 1954), based upon research among the Nupe and the Nuba, are very much in the tradition of cultural description and analysis. However, in his best-known effort to provide anthropology with a systematic theory, Nadel states (1951:21):

Actually, neither "social" nor "cultural" anthropology defines our subject matter satisfactorily; as I hope to show, it is essentially two-dimensional, being always both "cultural" and "social." But for this very reason I mistrust the exclusive concern with culture.... For it ignores the complexity of the process of scientific explanation, whether of human behavior or anything else. And if we took this precept at its face value we should be led into meaningless tautologies....

A few pages later (1951:30), following Talcott Parsons and Max Weber, Nadel specifically defines the subject matter of social anthropology as "aim-controlled" or purposeful action.

Nadel does not seem here to deny the existence of culture as a level of reality. In a subsequent context, however, he explicitly rejects this ontological assumption. Contrasting society and culture, he states (1951: 79-80):

Society, as I see it, means the totality of social facts projected on to the dimension of relationships and groupings; culture, the same totality in the dimension of action.

In other words, only behavior is "real." The difference between society and culture is an analytical difference predicated upon the epistemological view that a cultural analysis, while not very meaningful, is one which proceeds at a somewhat more abstract level of consideration than a social analysis.¹²

Nadel's assimilation of concepts from related disciplines is consistent with his view that purposeful action, and not culture, should be the focus of anthropological research. An example is his use of the concept "institution." Unlike Malinowski (Parsons 1957:59), Nadel does not consider institutions as the concrete isolates of organized behavior. In the tradition of Durkheim and Radcliffe-Brown, he explicitly differentiates the order of social relationships—i.e., social groups—from the order of "standardized" purposive action—i.e., institutions (1951:78).

If institutions are conceptualized as patterns of social relationships, rather than as sets of organized activities, it follows logically that their investigation necessitates the development of models suitable to the analysis of social relationships. Nadel has provided social anthropologists with one of the most systematic

of such models in his treatise on *The Theory of Social Structure* (1957). Here he rigorously moves the social action focus of social anthropology into its logical framework—the paradigmatic framework of comparative sociology. Briefly, he demonstrates that one can deduce all that is needed for a systematic theory of social structure from two relatively simple propositions or "criteria of relevance" for the analysis of social roles: the differential command that one actor has over the actions of others, and the differential command that actors have over existing benefits or resources (1957:114-24). In short, Nadel concludes that power relationships are the essence of social structure in formal terms. As Janowitz has noted (1963: 152), Nadel's model for structural analysis parallels the classical concerns of sociology and political science. Thus, having assimilated the concepts and assumptions of the theory of action, Nadel quite logically assimilated the paradigmatic focus of classical sociology.¹³

In view of these considerations, what are we to make of the charge that anthropology has not produced any rigorously deductive theoretical frames of reference that are relevant to the analysis of complex modern societies? Unfortunately, this charge is generally supported by the facts. On the one hand, anthropology has been conceptualized as the science of culture. As such, the discipline is characterized by a progression of paradigmatic arguments, most of them revolving around the culture concept and the relationship between anthropology and history. As a consequence, the paradigmatic focus of anthropology has been in a state of flux, and the science of culture seems not to have developed much beyond the stage of naturalistic description and observation. The cultures of complex modern societies seem much too intricate and too unwieldy to lend themselves to such an approach. On the other hand, in the absence of a paradigmatic focus, many anthropologists have developed problem orientations. In doing so, they have borrowed extensively from related disciplines, particularly sociology and psychology. To the extent that social anthropologists have converged upon problems of social structure and organization, they also have systematically assimilated the concepts and assumptions of sociology. Thus, anthropology has been in the process of converging with sociology.

What are the implications of this predicament with respect to anthropology and the study of complex societies? To the extent that anthropology maintains its identity as the science of culture, it would appear that its major theoretical function is comparison for the purpose of identification and classification. However, if we can accept the findings of the folk-urban studies (Freeman and Winch 1957), complex societies have been identified and suitably classified. Therefore,

¹² One might raise the issue here as to whether or not behavior, or purposeful action, is any more or less "real" than culture. Such an issue, however, would involve us in what appears to be an endless philosophical discussion. See, for example, Bidney (1953) and Kaplan (1965).

¹³ If I interpret Nadel's structural theory correctly, then the rich and informative character of social structure studies does not derive from a rigorous structural analysis carried to its logical conclusion, but rather from their qualitative and integrated descriptions of social relationships in terms of the cultural fiber (e.g., values, norms, rituals, etc.) in which these relationships are embedded. If Nadel's frame of reference is strictly employed, it is not possible to apply the concept "structure" to the analysis of society. In Nadel's terms, when one refers to the structure of society one usually has in mind the polity. Society has no single structure, and the many structures which it contains are neither logically nor empirically articulated (Nadel 1957:153-59).

in its present state of theoretical development, the science of culture seems to have very little to contribute as a separate and independent discipline to the study of complex societies.

Thus, as I suggested earlier, we seem to put the cart before the horse. We ask ourselves what anthropology can contribute to the study of complex societies because we have no coherent tradition of systematic research with which we can ascertain precisely what the study of complex societies can contribute to anthropological science. As a consequence, we are more or less compelled to occupy ourselves with problems defined by others, either human problems resulting from social and economic change or problems developing out of the theoretical constructions of other disciplines. The logical outcome of this predicament is not the development of anthropological science but rather the transposition of Western social technology to newly emerging nations or the convergence of anthropology with related disciplines.¹⁴

Having presented the case history of the problem, it now seems appropriate to take up its etiology. Why has cultural anthropology been unable to develop a coherent tradition of systematic research? More specifically, why has it been unable to develop beyond its phase of naturalistic observation and description? In order to answer this question, it is necessary to consider the epistemology of the science of culture.

III

During the years that the Boasian tradition dominated American anthropology, "culture" comprised the central datum of anthropological science, and cultural analysis was firmly embedded in the framework of naturalistic observation and description. It is frequently suggested that Boasian naturalism represented a reaction to the armchair ethnology associated with 19th-century Tylorian-Frazerian evolutionism (White 1945b; Steward 1949; Herskovits 1953; Wax 1956; Marian Smith 1959). While the validity of this interpretation is beyond doubt, it tends to overlook the fact that the period of Boasian naturalism represented a continuation of the Tylorian-Frazerian tradition in at least two very important respects. First, while rejecting the methodology of armchair speculation, most Boasian anthropologists did not reject or seriously modify the Tylorian concept of culture.¹⁵ Second, while rejecting the foibles of 19th-century evolutionism, most Boasian anthropologists did not reject the Tylorian preoccupation with human origins and universal cultural history.¹⁶

It is not difficult to demonstrate this continuity in tradition with respect to the culture concept. Although numerous definitions of culture were found in the literature surveyed by Kroeber and Kluckhohn (1952), Tylor's definition or some slight modification of it seems to be the one most widely used in the writings

of anthropologists. Similarly, the two generations of anthropologists who were directly influenced by Boas were very much concerned with the problem of human origins. If they confined their research to tracing the origin and development of American Indian cultures, it is only because these cultures were readily at hand. If many of their studies seem microscopic, it is only because their methods were based upon the close-range empirical techniques developed by Boas in his studies of art forms and mythology. However, behind the detailed diffusionist studies of the period loomed the search for cultural process (Bennett 1944), and behind the search for cultural process the problem of human origins and universal cultural history.

As a matter of fact, one could argue that Boasian naturalism represented an epistemological adjustment to, rather than a rejection of, the Tylorian-Frazerian problem of human origins. Such an adjustment was implicit in the tacit acceptance of the Tylorian concept of culture. As long as that concept was used to identify the central datum of anthropology, then (1) the study of culture had to be naturalistic; (2) the problem of human origins had to loom large among the seemingly diffuse interests of anthropologists; and (3) the idea that man and culture comprise continuously evolving phenomena had to exist as an implicit frame of reference from which no anthropologist could completely divorce himself.

The Tylorian concept of culture imposes these conditions for two reasons: first, it was developed in the context of the debate between the degenerationists and the progressionists (Stocking 1963), and was designed to deal with the major questions concerning the relation of race, language, and the ethnological conditions of human variation. Thus, the concept is physicalist or organismic in its ontological derivation. Second, the concept is applied to a class of phenomena that is residually defined. According to Tylor, culture or "civilization" is everything about man that is *not* biological: that is to say, it includes all those capabilities acquired by man as a member of society.

Ontologically, this conception of culture postulates the existence of two separate (but presumably related) realms of phenomena: the biological and the cultural. At the same time, however, it establishes the parameters of the latter by reference to the former. Now, if the parameters of biological phenomena are not known with respect to man (they were not known in Tylor's day and are not yet completely known), how is it possible for the anthropologist to delineate the parameters of his central datum in a systematic fashion? There appears to be only one solution to this problem: the parameters of culture have to be delineated by empirical observation. In an epistemological sense, the task is unmanageable. It necessitates a naturalistic methodology which treats every observable aspect of human behavior as significant provided that it is empirically established. The overwhelming problem that emerges is to delineate the full range of variation in order to determine what is humanly possible from what is biologically necessary. Thus, in the naturalistic tradition, bones, stones, languages, and traits are collected, mapped, and classified in terms of geological ages, archaeological stages, and

¹⁴ It might also be suggested that the logical outcome of this predicament could be more projects like "Camelot" (Horowitz 1965).

¹⁵ For Boas' modification of the Tylorian concept of culture, see Stocking (1963; 1966).

¹⁶ For a history of this problem and its relationship to the emergence of anthropology as a discipline, see Hodgen (1964), particularly the concluding chapter.

cultural areas. Implicitly, it was thought that by unraveling the origins of man and culture, and by charting the course of universal history, the laws regulating biological and cultural processes would become evident and then, perhaps, the mystery of nature versus nurture would be clarified also.¹⁷

The Tylorian paradigm entangled practically every 19th-century anthropologist in the nature-nurture problem. Because of this problem many of them—e.g., Lubbock, Frazer, Marett, Hahn, Rivers—became embroiled in anthropological psychologizing. The same intellectual concern is evident in Boas' *The Mind of Primitive Man* (1911). Kroeber's classic article on "The Superorganic" (1917) may be viewed as an effort to free anthropology from the morass. However, as Leslie White (1947) has indicated, many anthropologists (including Boas, Sapir, Herskovits, Goldenweiser, Benedict, Hooton, and Bidney) rejected the "superorganic" as mystical and unnecessary. Thus, even as late as the 1930's, Margaret Mead (1935) continued the search for the parameters of culture in the jungles of New Guinea.

In short, from a paradigmatic point of view, the period of naturalistic anthropology represented a search for parameters—more directly, a search for the substantive focus of anthropological science. If the period is theoretically barren, it is only because the Tylorian paradigm precluded the development of theory. A science that cannot delineate its central datum also cannot construct systematic theory.¹⁸

In spite of the progression of paradigmatic arguments that emerged during the Boasian period, the epistemic legacy of naturalistic anthropology remained in force well into the 1940's. However, even before that time, some of Boas's students were becoming impatient with its limitations. This impatience is reflected in Margaret Mead's efforts to develop a problem orientation first with respect to culture and personality formation (1930) and subsequently with respect to the analysis of cultural change (1932). This impatience is reflected also in Ruth Benedict's classic statement on cultural patterns (1934). Nevertheless, the Tylorian concept of culture continued to vex anthropologists interested in the development of systemic theory (e.g., Kluckhohn and Kelley 1945; Kroeber and Kluckhohn 1952; Bennett 1954; Kroeber and Parsons 1958).

Anthropologists will undoubtedly continue to disagree about the precise definition of culture as well as the ontological status of the phenomena to which the concept is applied. However, it is clear that if cultural anthropology is to move out of the epistemological context of naturalistic description and observation, and if it is to maintain an identity as a separate disci-

pline, cultural phenomena cannot be delineated residually with reference to unknown biological parameters. Furthermore, by this time it has been fully established that the nature of man is neither biological nor cultural but biocultural and that various dimensions of the human condition may be analytically differentiated for the purpose of scientific analysis. Thus, while definitions of culture vary somewhat in terminology, there appears to be general agreement on the following propositions (e.g., MacIver 1942:269-90; Kluckhohn and Kelley 1945; Parsons 1951:10-11; Kroeber and Parsons 1958:582-83): (1) culture is historically derived; (2) culture is essentially ideational; (3) culture is comprised of cognitive orientations and values; and (4) culture is patterned. In these terms culture is positively defined: it is a historically derived system of ideas and values delineated by and expressed through the institutional structures of society.¹⁹

Epistemologically, this paradigm makes possible the development of systematic cultural research. It also places the anthropological study of "complex" societies in an entirely different perspective. By focusing attention on the structure of ideational systems, we can construct analytical models which reduce the complexity of historically different cultures to comparable dimensions. By comparing analytical models rather than historical cultures, we can eliminate the old problem of comparing incomparables. In addition, since analytical models are constructed with reference to particular theoretical problems, it follows that although the distinction between "simple" and "complex" societies may have heuristic value for certain kinds of research, it does not have paradigmatic implications. In short, the distinction between "simple" and "complex" societies may be useful for investigating certain problems of theoretical significance: however, it is not epistemologically necessary to develop a *special kind* of anthropology to undertake the study of these problems.

The question remains: is it possible to delineate conceptual frameworks more or less consistent with this paradigmatic view of culture and capable of defining particular problems of theoretical significance with respect to the study of complex societies? By way of responding to this question, it is instructive to consider some of the recent discussions concerning the idea of "cultural pluralism" and its heuristic value for the study of newly emerging nations. These discussions are instructive in the present context because they give emphasis to some of the essential differences between cultural and sociological approaches to the study of

¹⁷ Given the magnitude of this task, it is not surprising that Boas became increasingly pessimistic with age. In 1888, he was convinced that the discovery of laws was the most important aim of anthropology; by 1932, he felt doubtful that such laws could be discovered (Kluckhohn and Prufert 1959:24).

¹⁸ I hasten to add that the period of naturalistic anthropology cannot be dismissed as having been scientifically unproductive even if it was unproductive of theory. If anthropologists had not completely unraveled the nature-nurture problem by the 1950's, at least they knew from their accumulated work that the problem had been poorly conceived by their predecessors. And, on the positive side, as a result of the vast amount of data collected, the problem of human origins, as Darwin had conceived it, had been circumscribed and virtually solved (Teilhard De Chardin 1953).

¹⁹ It follows from this paradigm that culture is neither a system of behavior nor a system of social relationships: it is an ideational system. This point needs to be emphasized because it has implications with respect to the problem of causation as it pertains to the analysis of behavior. Some theoreticians assign the primacy of causation to the cultural order (MacIver 1942:388-89) while others (Parsons 1951) prefer to consider the problem of causation in the context of functional analysis. The definition of culture, or the ontological status of cultural phenomena, ought not be confused with a theory of social causation. The question of what culture "causes" or does not "cause" has nothing to do with what culture comprises as a dimension of reality: rather, it has to do with whether or not this particular dimension of reality is being considered as a dependent or independent variable in a particular analytical context. In some analytical contexts culture may be considered as an independent variable. This does not imply that it need always be so considered.

complex societies and because they illustrate the types of questions that might be posed with respect to these societies with a view to the development of systemic cultural theories.

IV

The concept "cultural pluralism" was first introduced to the analysis of change in underdeveloped areas by J. S. Furnivall (1939:446-69) in his description of Netherlands India. Observing that the different cultural groups of that area lived side by side but without much mingling, Furnivall concluded (1939:468): "Nationalism within a plural society is itself a disruptive force, tending to shatter and not to consolidate its social order." Subsequent observers interested in colonial or ex-colonial areas have seized upon Furnivall's concept for purposes of analysis. Political scientists, particularly, have used it to explain the political convulsions which many new nations have experienced since their independence (e.g., Almond and Coleman 1960).

Among anthropologists, especially those who have been working with Caribbean and African societies, the concept of cultural pluralism has stimulated considerable theoretical discussion.²⁰ A fundamental issue is whether or not the plural model, when compared to the structural-functional model of the unitary society, offers any particular advantage for examining the problem of unity and diversity in society and its relationship to the process of cultural change. (I have briefly dealt with the discussion of this issue elsewhere [1964].)²¹

M. G. Smith has recently revised Furnivall's conception of pluralism and suggested that a rigorous analysis of certain types of societies is difficult, if not impossible, without making use of the plural model (1955; 1960). In developing his model, Smith takes the Malinowskian position that social institutions are the concrete isolates of organized behavior. Each institution comprises a mutually supportive set of values, rules, activities, and social relations. Institutions having to do with the same phases of life (e.g., marriage, family, extended kinship, etc.) form clusters or subsystems. These subsystems, in turn, combine to form the total institutional system. Because institutions function to define and sanction the persistent forms of social behavior, the institutions of a people's culture make up the matrix of their social structure. It follows from this that the core of a culture is its institutional system. In other words, according to Smith, institutional differences serve to differentiate cultures and social units.

Not all institutional differences are of equal importance for differentiating cultures and social units (M. G. Smith 1960:766). Following Linton (1936:272-74) and Nadel (1951:121), Smith (1960:769) distin-

guishes between "compulsory," "alternative," and "exclusive institutions. Compulsory institutions are those in which all members of a social unit must participate (among them kinship, recreation, education, religion, property, economy, and certain types of sodalities). Alternative institutions are those shared by certain individuals as a matter of choice (e.g., social class membership or community membership, etc.). Exclusive institutions are those shared by individuals who belong to certain socially recognized categories (e.g., an occupational group). A population that shares a system of compulsory or basic institutions tends to form a relatively closed sociocultural unit. It is socially and culturally homogeneous.

Building on these conceptualizations, Smith distinguishes three types of societies: (1) homogeneous societies, in which all the groups of a political unit share the same total institutional system (the best examples of this type are found among the preliterate societies studied by anthropologists); (2) heterogeneous societies in which the groups within a political unit share the same system of basic institutions but participate in different systems of alternative and exclusive institutions (most modern societies, like the United States, display this heterogeneous character); and (3) plural societies, in which groups living within a political unit have very different systems of compulsory or basic institutions. In the plural society, the cultural plurality of the society corresponds to its social plurality. The culturally distinct units of the plural society are its "cultural sections." Generally, these cultural sections are highly exclusive in the sense that each displays an area of common life beyond which relationships are specific, segmental, and governed by economic and political structures. The best examples of this type of society are found among the newly emerging nations (Shils 1963).

Some anthropologists have taken exception to the theory of cultural pluralism. For example, Raymond Smith (1961:155) has suggested that the concept "cultural pluralism" has little value other than summarizing in two words some very complex problems. One aspect of the theory that has been criticized is the view that cultural homogeneity is a condition of societal homogeneity. This view, Vera Rubin (1960: 784) suggests, is utopian. When it is recognized that nearly all national societies display significant regional, rural-urban, ethnic, and social class differences, from an historical point of view the homogeneous society appears to be the exception rather than the rule (Braithwaite 1960:824).

Another disturbing feature of the theory of cultural pluralism, according to its critics, involves the concept "institution." If institutions are reified in Malinowskian fashion rather than defined in terms of activities analyzed in structural-functional terms, they are reduced to clusters of cultural traits about which only differences and similarities may be noted. It follows from this that society, as a social system, is defined in terms of culture. Braithwaite (1960:823) argues that such an approach is fundamentally at variance with that which stresses the viewpoint of social action and concludes that the theory of pluralism is of little value for the analysis of social behavior.

²⁰ For reference to this discussion, see M. G. Smith (1965:vii-xvii). Perhaps the best and most extensive review of the issues involved in this discussion may be obtained in a forthcoming article by M. G. Smith under the title "Some Developments in the Analytic Framework of Pluralism," scheduled to appear in a volume on *African Pluralism* edited by Leo Kuper and M. G. Smith.

²¹ For a critique of this article, see McKenzie (1966); for an expansion of the analysis presented in it, see my forthcoming book (1967).

The greatest amount of criticism of the plural model has been directed at Smith's view that there is no necessary functional integration of institutional systems or cultural sections at the societal level. In the critics' view, the plural model overlooks the fact that there must be a minimum core of shared values if the integration of a society is to be maintained other than by force. If force is essential to the political structure, then the application of the plural model would seem to be limited to the analysis of colonial societies, and even here the model would emphasize the differences between groups at the expense of those factors which serve to integrate them into the total system.

Critics of the plural model suggest an alternative approach to the study of newly emerging nations. They hold, following Radcliffe-Brown, that the unit of study must be the whole society (e.g., Raymond Smith 1956:255). Culture, as a variable, is relevant only to the extent that it represents a system of "shared symbolic meanings which makes communication possible in an ordered social life" (Raymond Smith 1956:253). The differentiation of cultural sections is considered to be a structural problem that may be dealt with in terms of stratification theory. Social class divisions not only distribute the members of different cultural groups with reference to social statuses, different degrees of political power, and different social functions, but they also serve to integrate different cultural groups within the overall social structure of the society. In other words, in terms of this unitary theory of society, cultural sections are structurally integrated in the total system, and to consider them as separate entities is misleading.

In order to evaluate these two analytical models—the plural model and the unitary or reticulated model—it is necessary to return to some of the paradigmatic issues discussed previously. The reticulated model makes the assumption that society is a unitary system of social action; i.e., it has a *structure*. In addition, this model assumes that the integration of society is possible because of a system of shared symbolic meanings and values or *a culture*. If this second assumption is systematically pursued, the reticulated model reduces culture to a constant. As a consequence, the theoretically meaningful variables in the model are those which have to do with the structure of social relationships.

One point that needs to be made with respect to the reticulated model has to do with its systemic features. As Nadel rigorously demonstrates (1957), the structure of society cannot be systematically delineated as a unitary entity either in terms of logic or empirical evidence. To state the matter differently, society is comprised of numerous structures, and not all of them reticulate in terms of the criteria of differential command over the actions of others and differential command over resources and benefits.

A second point that needs to be made with respect to the reticulated model concerns the assumptions upon which it is predicated (i.e., that society has a *structure* and is integrated by virtue of *a culture*). It would appear that these assumptions are most valid in reference to preliterate societies and cultures. In other words, the interlocking of culture and structure is most evident in preliterate societies where shared meanings

and values are homogeneous and a wide range of different activities are carried on in terms of a uniform structure such as kinship. Implicitly, then, the reticulated model is predicated upon a conception of society which is historically derived, even though, explicitly, it rejects such a conception in favor of the systemic analysis of social relationships apart from their historical context.²²

The final point I wish to make concerning the reticulated model has to do with paradigmatic theory. In terms of paradigmatic theory, the reticulated model represents a form of sociological analysis. This is most evident from the fact that it rejects the theoretical significance of historically deep-rooted subcultures in favor of the assumption that the total system of social relationships has so modified these subcultures that they are no longer meaningful from the point of view of system integration. Thus, the reticulated model would seem to preclude the cultural analysis of complex societies.

To summarize, the unitary or reticulated model: (1) lacks systemic development; (2) is predicated upon assumptions which appear to be primarily relevant to the study of preliterate or simple societies; and (3) precludes the cultural analysis of complex societies. For these reasons, it would seem to have little analytic value for the anthropological study of complex societies.

Turning to the plural model, it should be noted that attempts to conceptualize social and cultural pluralism and to define precisely what it is that constitutes a cultural section have involved numerous ambiguities. In terms of M. G. Smith's view (1960:768), society and culture are not coterminous. If they were, the plural model would be a theoretical impossibility. Accordingly, M. G. Smith defines society as a political unit rather than a sociocultural system. It is a territorial area with its own governmental institutions. In these terms, the plural society becomes a political unit of a specific type: one that contains populations that are culturally distinct.

However, this conception raises still another difficulty: how are we to differentiate the plural society from the heterogeneous society? The use of the distinction between "compulsory," "alternative," and "exclusive" institutions, it seems to me, does not provide a very satisfactory solution to this problem. If a culture is understood to be an integrated pattern of standardized social usages, then which of these usages can be considered basic or compulsory? Might not all of them be considered basic to the cultural pattern? Or, assuming we could isolate the "compulsory" institutional system for analytical purposes, at which point do variations become great enough to warrant the identification of separate cultural sections? These issues cannot be easily ignored if cultural theory is to be systemic.

I have suggested previously that cultural facts consist of ideas, values, symbols, and meanings existing

²² Although I have not had the opportunity to explore this issue systematically in the literature, it is interesting to note that Max Weber (1947), according to Parsons' translation, avoids using the concept "society." I suspect that it is difficult, if not impossible, to define this concept apart from an historical conception. I also suspect that many of the social system theorists avoid the concept for this reason.

through time (i.e., they are historically derived). As such, culture has no clear-cut boundaries. By definition, however, a unit (e.g., *a culture*) must be bounded. It follows that culture can exist as a unit only when it is bounded by structural dimensions existing in time. Following Malinowski, social institutions are the structures through which cultural ideas and values are expressed as traditional norms regulating the affairs of individuals and groups. Thus, I consider sound M. G. Smith's position that institutional differences serve to distinguish differing cultures and social units: how else are we able to make such distinctions? Insofar as this is the case, however, all institutions must be considered equally functional in the expression of culture. Therefore, the analytical distinction between basic, alternative, and exclusive institutions, as far as the expression of culture is concerned, is spurious and serves no theoretical purpose.

Society, in contrast to culture, is a structural unit of the most encompassing type. A society has many levels of institutional structuring, and some are more interdependent than others. For example, human activities may be institutionalized locally, regionally, or nationally. Some activities may involve horizontal structures such as castes, classes, occupational groups, professional groups, and the like. Culture is expressed through all of these different kinds of institutional structures, and the activities structured at one level need not be integrated with those structured at another level in the sense that they are interdependent.

The most inclusive structures of a society are those that have to do with its political or governmental institutions. Regardless of whether or not lower-level structures articulate with higher-level structures, governmental institutions have as their function the integration of the social system (through force or value consensus or both). Thus, I would also agree with M. G. Smith that society and culture are not coterminous. However, this should not be interpreted to mean that society and culture can be entirely independent of one another (even force has to be recognized to be effective). There can be no societal structure (including the governmental) which does not mobilize some definite expression of cultural values. What, then, is a plural society, and how is it to be conceptualized?

In keeping with the systems approach, I would suggest that the definition of the plural society must take into account two related sets of facts: (1) the extent to which specified groups are culturally differentiated in terms of specific institutional activities; and (2) the level at which institutional activities serve to maintain cultural differentiation as the basis for sociocultural integration. When institutional activities serve to integrate culturally differentiated groups primarily at local levels, such groups might be referred to as "minimal" or local cultural sections. On the other hand, when institutional activities serve to integrate culturally differentiated groups at the societal level, such groups might be identified as "maximal" or national cultural sections. Short of a society being homogeneous, the existence of "minimal" or local cultural sections expresses the smallest amount of cultural variation a society can have. A society that contains local cultural sections is socially and culturally heterogeneous. Short

of a society being bifurcated, the existence of "maximal" or national cultural sections is the greatest amount of cultural variation a society can have. A society that contains national cultural sections is a plural society.

The distinction between national and local cultural sections (and between the plural and the heterogeneous society) may be understood by way of the following illustration: The United States is a heterogeneous society. It contains many cultural groups that are integrated in varying degrees in local communities (e.g., the Irish, the Polish, the French Canadians, etc.). We usually think of these as ethnic groups. There are practically no institutional structures that serve to integrate each of these groups, separately, at the national level. Uganda, on the other hand, is a plural society. It contains a proliferation of culturally differentiated groups—Baganda, Acholi, Langi, Iteso, Turkana, Karamojong, Bankonjo, Bwamba, and Batoro, to name but a few. Many of these groups, the Baganda for example, not only are culturally differentiated and locally integrated, but participate in institutional structures which serve to maintain their cultural differentiation at the national level.

In these terms, then, the heterogeneous society and the plural society are two ideal types on a continuum of sociocultural integration. This particular conceptualization permits the construction of analytical models which specify the criteria to be used to determine empirically the degree to which a society is pluralistic. In order to construct such models it is not necessary to assume that some institutions are more "basic" than others. It is useful, however, to distinguish between two broad classes of institutions: (1) those which serve to structure activities and express cultural values within the context of local communities; and (2) those which function to link local activities to the wider spheres of societal activity. The first of these types may be conveniently identified as "local institutions." The second type may be called "broker institutions" in order to emphasize their functional character.²³ The kinds of activities falling into each of these categories may be expected to vary empirically from one society to another. Generally, however, the following activities tend to be structured in terms of local institutional patterns: language (dialects), family and kinship, work, socialization (and possibly education), recreational activities, associational activities, and communal activities (e.g., local government). All of these may generate institutional structures through which cultural differences may be expressed between groups in the hamlet, the village, or the neighborhood.

The question remains: by what criteria are we to judge institutional differences in order to determine the degree to which local cultural sections are apparent and viable? Three empirical questions, I would suggest,

²³ The distinction between "local" and "broker" institutions was suggested to me by a review of Julian Steward's conception of levels of sociocultural integration. Steward notes, for example, that national culture is divisible into two general kinds of features: (1) those that function at the national level and (2) those that pertain to sociocultural segments or subgroups of the population. Broker institutions, it would seem, function to integrate these two levels in various ways. In this sense, they are distinguishable from local institutions. See Steward (1955:43-63).

TABLE 1
CULTURAL PLURALISM: FIRST-ORDER TRANSFORMATION

Local Institutional Activities	INVARIANT POINTS OF REFERENCE		
	Structure Valid for Particular Groups	Structure Valid for All Groups but Modified by Some	Structure Uniformly Valid for All Groups
Language (Dialects)	x		x
Family and Kinship	x		x
Local Economy (Work)	x		x
Religious Activities	x		x
Socialization and Education	x		x
Recreational Activities	x		x
Associational Activities	x		x
Communal Activities	x		x
	Plural model relevant		Plural model not relevant

serve to direct our attention to the data in terms of which the integration of local cultural sections can be ascertained. With respect to each sphere of institutional activity we may ask: is the structure of the institutional activity valid only for particular groups? Is it valid for all groups but modified by some? Or, is the structure of the institutional activity uniformly valid for all groups?

To the extent that the structure of institutional activities is valid only for particular groups, the structures involved express different cultural values, and the groups under investigation constitute local cultural sections. If the structure of institutional activities is uniformly valid for all groups, then the groups under investigation share a common culture, and the plural model is not relevant to the analysis of the society in question. The first-order transformation of the plural model is illustrated in Table 1.

The integration of local cultural sections at the societal level is a function of the structure of broker institutions. Again, the kinds of broker institutions will vary empirically from one society to another. They may include, among other things, markets (labor and consumption), corporations, cooperatives, religious associations, public and/or private school associations, social and civic associations, labor unions, ethnic associations, political parties, and various governmental agencies. With respect to each of these broker institutions we may ask: does the structure of the institutional activity reinforce the separate integration of

similar local cultural sections? Or, does it serve to mediate relationships between members of different local cultural sections and thereby modify the expression of their different cultural values? Or, does the structure of the institutional activity serve to integrate the members of different local cultural sections by generating the expression of new cultural values? For example, in Uganda, do Baganda, Acholi, Langi, Iteso, Turkana, etc., belong to different labor unions? Do their economic transactions take place in different markets? Do they belong to different political parties? Or do these kinds of institutional structures bring the members of these different groups together in the expression of common cultural values?

Maximal or national cultural sections will exist when broker institutions serve to integrate, separately, similar local cultural sections and thereby allow for the expression of their characteristic cultural values in national spheres of social activity. When these conditions exist, the plural model fits the society under investigation. This second-order transformation is illustrated in Table 2.

In terms of this model, the plural society is a socio-cultural system with specified empirical dimensions. However, it is not unitary in the sense that all of its component elements are functionally integrated by

TABLE 2
CULTURAL PLURALISM: SECOND-ORDER TRANSFORMATION

Broker Institutional Activities	INVARIANT POINTS OF REFERENCE		
	Structure Integrates Similar Local Cultural Sections	Structure Mediates between Different Local Cultural Sections	Structure Integrates Different Local Cultural Sections
Markets	x		x
Corporations	x		x
Labor Unions	x		x
Governmental Agencies	x		x
Political Parties	x		x
School Associations	x		x
Religious Associations	x		x
Social and Civic Associations	x		x
Ethnic Associations	x		x
	Society is culturally pluralistic		Society is culturally heterogeneous

virtue of a common system of values. The interdependent units of the plural society tend to comprise culturally differentiated groups rather than socially differentiated persons. These groups, or cultural sections, are part of a common body politic. They may or may not participate in a common economy. While relationships between these groups may be segmental and instrumental, depending upon the degree of their exclusive incorporation, their integration into the society as a whole is ultimately a function of governmental structures and the relative absence of conflict.

From the point of view of paradigmatic theory, the contrast between the plural and reticulated models is extremely revealing. The plural model implements what is essentially a cultural approach to the study of complex societies (1) by avoiding assumptions which reduce the phenomena of culture to the analytical level of a constant, with the result that the meaningful variables are not social relationships; but rather the relatively exclusive cultural traditions that persist due to the structure of social relationships; and (2) by focusing attention on the institutional expression of cultural values, thereby providing a framework within which the cultural patterns of complex societies may be systematically and empirically differentiated.

The contrast between the two models is also revealing from the point of view of theory construction. The logical and systemic development of the reticulated model culminates in theories of power, that of the plural model in theories of culture. The empirical differentiation of cultural patterns enables the anthropo-

gist to raise a whole series of questions which are significant for the development of cultural theory. How are these patterns affected by the relationships which exist between different groups in society? Will cooperation between groups in a complex society result in the modification of existing cultural patterns, or will it contribute to their persistence? How does conflict and the resolution of conflict affect existing cultural patterns? How are different cultural patterns within a society affected by economic and political changes? What impact does urbanization have on the integration of cultural sections? How does institution-building affect existing cultural sections and their characteristic cultural patterns?

Questions of this type, it seems to me, place the issue under consideration in a new light. It is not necessary for anthropologists to ask what anthropology can contribute to the study of complex societies. The fact is that the study of such societies can make a truly significant contribution to anthropology *if* questions are formulated in terms of a clearly recognized paradigmatic focus. We do not need a special kind of anthropology to undertake the study of complex societies. What we need is a paradigmatic focus in terms of which we can develop analytical models with a view to the construction of cultural theory. Only when such a focus is established will anthropology, as a discipline, make a unique contribution to our understanding of complex societies.

Abstract

In this essay I have taken issue with the view that a special kind of anthropology is needed if anthropologists are to make a contribution to the study of complex societies. This point of view appears to represent a response to conditions which have prevailed more or less since the end of the World War II: instability in the newly emerging nations and the availability of funds for the application of social science to the solution of human problems. If this attitude persists, it would appear that anthropology will become either a new kind of social technology or a comparative sociology. It is argued by some social scientists that anthropology can no longer exist as an independent intellectual discipline. This argument is predicated upon the following propositions: (1) anthropology's traditional subject matter (i.e., preliterate societies and cultures) is disappearing; (2) anthropology has not developed a sophisticated empirical methodology; and (3) anthropology is lacking in systematic theory. The first of these propositions is erroneous: it is based on a misconception of anthropological science, and it ignores the fact that anthropology has never been exclusively preoccupied with preliterate societies and cultures. The second proposition, pertaining to research methodology, involves considerations of epistemological disagreement between anthropology and related sciences and of the uses of modern research technology. In view of anthropology's propensity for configura-

tional analysis and its tendency to reject operationalism, issues of epistemological disagreement between anthropology and related sciences are not likely to be resolved. As far as modern research technology is concerned, there is no reason why anthropologists cannot be easily trained in its uses. The third proposition, that anthropology is lacking in systematic theory, cannot be so easily dismissed.

In evaluating the criticism that anthropology has no theory, I have suggested that it is necessary to differentiate between two types of theory: explanatory theory and paradigmatic theory. Explanatory theories are comprised of deductive systems of propositions which purport to explain, either in terms of stated or predicted relationships, the properties of phenomena that are conceptually delineated for scientific analysis. Paradigmatic theories are comprised of general orientations which serve to define the substantive focus and intellectual approach of particular disciplines. While anthropologists do in fact use all of the various types of explanatory theory that have been employed by the other sciences of man, they seriously lack a paradigmatic focus.

Conceptualized as the science of culture, anthropology has labored primarily with the Tylorian paradigm of culture or some modification of it. This paradigm imposed an epistemological framework upon the discipline which precluded the development of systematic cultural theory. Thus, in the absence of a coherent tradition of scientific research, anthropology's substantive focus continues to be an issue of considerable

debate, and individual anthropologists have been left largely to their own resources in defining the legitimate problems of anthropological research. As a result, anthropologists have researched an almost incomprehensible range of human phenomena. This characteristic of the discipline is often presented as its forte by those who conceptualize anthropology as "the study of man"; but I consider it its principal weakness.

In pursuit of problems having to do with almost everything that is conceivably human, anthropologists have become increasingly dependent upon related disciplines for the construction of theory. This often has led individual anthropologists to assimilate the paradigmatic focus of those disciplines from which they have had to borrow concepts and assumptions. As in the case of Nadel, such a process of assimilation is clearly evident among social anthropologists who have borrowed concepts and assumptions from classical sociology in order to analyze problems that have to do with the structure of social relationships. In short, to the extent that anthropologists do not share a paradigmatic focus, anthropology is in the process of converging with other disciplines.

These circumstances have combined to make the anthropology of complex societies a problematic issue with implications for the continued existence of the discipline. Anthropology's paradigmatic focus must be the study of culture if anthropology is not to converge with other disciplines; but the science of culture, still in the stage of naturalistic description, has not yet produced a body of theory with which problems of signifi-

cance can be delineated with respect to the cultures of complex societies. The simple solution to this dilemma is to accept the problems defined by funding agencies and by other disciplines and to put anthropologists to work on these problems with the observational skills they have acquired from their studies of pre-industrial societies.

To my way of thinking, a more desirable solution is to undertake the arduous task of extracting cultural anthropology from the morass of naturalistic description and observation. Anthropology has always concerned itself with the analysis of cultural systems. Such systems are clearly relevant to the process of human evolution, to the organization of societies, and to the development of individual personalities. Without scientific knowledge of these systems neither anthropologists nor the other scientists of man can fully comprehend, or appreciate, the human condition. In short, anthropology's paradigmatic focus should be the scientific study of cultural systems.

Thus, I have attempted to show how the construction of models for the analysis of cultural systems might contribute to the formulation of questions that are relevant to the development of cultural theory. To illustrate how this process of theory development might proceed, I have used as an example the model of cultural pluralism, pointing out its advantages over the unitary or reticulated model for the study of complex societies. It needs to be emphasized, however, that this particular model is by no means the only one with which anthropologists can develop theories of culture.

Comments

by MICHAEL BANTON*

Bristol, England. 20 v 67

To discuss whether anthropology can make a contribution to the study of complex societies one should be informed both about anthropology and about the study of complex societies. Despres' essay shows relatively little acquaintance with the existing state of research concerning complex societies on the part of sociologists, political scientists, and others. The individual anthropologist cannot be expected to master so extensive a field, but this may be a characteristic weakness (cf. Bennett and Wolff 1956:336) and if anthropologists are to move into this field they will have to find ways of rectifying it. My own experience suggests that if they do, they will find that the problem of a possible anthropological contribution dissolves. Even in a relatively homogeneous society such as England there are institutional areas in which ideas of cultural pluralism could be most fruitfully deployed (MacIntyre 1967). My study of the role of the police patrolman in Scot-

land and the United States and continuing my research in the field of police-public relations make me believe that there must be many areas which are not being sufficiently studied by sociologists and in which ideas from anthropology could be most valuable (Banton 1964a, b).

Anthropology may be lacking in paradigmatic focus, but so, I would have thought, are sociology, economics, political science, etc. Despres wants to recruit us to a collective endeavour to remedy a dubious malady; he rejects the "simple solution" which would attach little importance to disciplinary foci. But if anthropologists concentrate their work in the areas where they can get maximum intellectual return upon the time and trouble invested, this should be to the benefit both of anthropology and of scholarship, for we are nearer to a perfect market in the world of learning than in any other complex society. Whether this would weaken or strengthen the integration of anthropology no one can tell, but this issue is less important than the advancement of learning. Janowitz's dismissal of the "jurisdictional dispute" is not overturned by Despres' reference to Bennett and Wolff's persuasive account of the differences in sociologists'

and anthropologists' "images of man," for these differences are associated with the study of different kinds of society. Whether they would persist if sociologists and anthropologists studied the same kinds of society no one knows.

by JOHN W. BENNETT*

St. Louis, Mo., U.S.A. 18 v 67

I take it that Despres wants to (1) define culture as the valuational component of human existence and (2) define anthropology's task as the study of this phenomenon. He deplores the tendency of anthropologists to wander off into societal constructs and analyses, although he acknowledges the distinctive value of societal approaches (would he prefer that these be left to sociologists?). His plea for a more rigorous and focused sociocultural anthropology is eloquent and well-taken, but he appears to ignore a number of serious obstacles inherent in the subject matter with which anthropologists deal and a number of semantic problems as well.

With respect to semantics, much difficulty with the culture concept arises out of the fact that there is only one

word in European languages for two quite different levels of phenomena: human culture and specific human group cultures. The former term refers indiscriminately to the whole of human existence, as Despres is of course aware in his criticism of the Tylorian definition. The second refers to specific historically limited ways of life, with knowable behavioral parameters. The tendency to confuse one with another leads to the misapplication of the Tylorian concept to the behavior of specific times and places, and since the Tylorian concept is descriptive only, explanation of the how and why of specific cultural behaviors cannot be achieved without paradox and confusion. Despres' explication of this difficulty is detailed and almost definitive, but I believe he has not solved the basic problem. One reason he has not is that the professional credo of anthropology has developed along dualistic lines, following this double usage of the term culture. At the most general level, anthropologists seek to know the whole of human phenomena, the biological as well as the "cultural."

This sweeping view forces them to continue using the Tylorian conception of human reality, and this prevents single-minded concentration on a specific aspect of that reality. The difficulty of distinguishing the societal from the valuational arises not only from this holistic focus, but also from the nature of human existence itself: it is multidimensional, and as soon as a scholar of the whole begins to inquire into any particular phase, he is almost duty-bound to inquire into other phases as well.

These difficulties emerge in Despres' discussion of the Caribbean case. He discards the reticulated model of Caribbean society because it is based on a societal analysis alone; he accepts the plural model because it is based on the cultural. It is possible that the fact that the plural model does a better job of explaining Caribbean society is not because it is a cultural model or because it is a superior theoretical concept, but simply because of the nature of Caribbean society. There may be other societies in which the situation is reversed. A case can be made for an approach which holds that "culture" and "society" are variables, not constants; i.e., some societies have more "society" than "culture," and the reverse (Redfield once believed, following German sources, that folk people had more "culture" than urban).

In any case, the anthropologist, seeking comprehensive understandings and explanations of living human realities, is forced to move toward sociological or societal as well as cultural analysis, because these realities are societal. Many anthropologists must therefore become sociologists as well.

The danger of accepting assumptions along with concepts, which Despres expertly analyzes, is a real one, and I am skeptical as to whether it can be avoided. Despres seeks a rigor and narrowness of focus which anthropology, as a general discipline based on informal and intimate participative research, with much free creative intellectualizing, seems almost incapable of achieving. And I am not at all sure it should try overly hard, because much of the distinctiveness of the field may be lost in the process.

Despres is separatistic, analytic. He has a point: people studying culture should sharpen up their tools and be content with their limited focus, and if they move to another dimension or level, they should know what they are doing. I have the feeling that he sometimes confuses this quite legitimate point with his very strong desire to make a case for acultural *vs.* a societal approach. I am not entirely sure on which side Despres wants to throw the emphasis.

Anthropologists who have worked with the valuational concept of culture have experienced difficulties (see, for example, the candid analysis of these difficulties by Vogt and Albert [1966]). A study of culture as values becomes a study of values, not culture. That is, a particular dimensional definition of culture generally leaves anthropologists with a virtually unsolvable causal dilemma: if values are the major component of culture, then do they cause other phenomena, or are they caused by it? Are values indeed the major component of culture in every case? Despres might answer by saying that to determine this is precisely the objective of studies of culture, and I cannot argue this point. He is entirely justified in saying that we need more and better conceived studies of culture as values. However, when he recommends that we make values the central topic of cultural anthropological effort, I believe he confuses a desirable and important segmental effort with a much larger and more varied undertaking.

There is a further difficulty in the argument. Despres insists (again following Kroeber) that culture is a topic capable of becoming a "paradigmatic focus"—as society is for sociology, behavior for psychology, the cell for biology. There has been no lack of effort along these lines since the superorganic notion was developed, but somehow the objective has remained unclear and the results ambiguous. Is this failure due to the epistemological confusion which Despres describes, or to some other cause? Certainly anthropologists have been confused on the issue; but there remains a persistent

question as to whether culture really does occupy a distinct analytical or ontological position. In any usage aside from the Tylorian conception of the totality of human phenomena, culture is simply equivalent to the *content* of any human social situation, and this content is multidimensional. Hence the analytical schemes used by anthropologists must include more than the cultural. This was precisely what I was attempting to say in the 1954 paper which Despres quotes as witnessing the opposite position. Kroeber spotted this in a letter he wrote me after publication of the paper: quite correctly, he noted that I seemed to want a "small anthropology in a large social science," whereas he favored a large, independent anthropology, based on the study of culture, alongside other and cognate sciences.

I would now modify my own position, however, and agree with Despres that more careful and analytically conceived studies of culture are needed. Anthropologists have identified culture with the tribal version at the great cost of becoming unable to see culture in other contexts. Their failure to study cultural phenomena in modern industrial society as intensively as they have studied it in tribal and peasant is a result of this special preoccupation. But even here there are difficulties in using the cultural frame, for it is evident that an explanation of phenomena in peasant and other complex societies requires considerations of other phenomena than cultural. This situation has given rise to the enormously varied effort of contemporary anthropology in the post-World War II period. Laura Thompson's recent suggestion to distinguish between micro- and macro-culture has some merit here: it takes care of the need to recognize that cultural phenomena in complex societies have some peculiar dimensions not found in the isolated community. Despres is also correct in pointing out that the failure to define culture adequately on a cross-cultural basis has stultified theoretical progress in anthropology.

Thus I agree with Despres' eloquent plea for more and better cultural studies; but I doubt that it is either possible or wise for anthropology to restrict its effort to culture in the face of its evident multidimensional aims and the basic ambiguity of the culture concept itself.

by RONALD COHEN*

Evanston, Ill., U.S.A. 24 v 67
Since I am one of the persons referred to by Despres who sees a "crisis" in contemporary anthropology, and since

I take a somewhat different view of it than he does, let me explain briefly what my position is. Despres mentions Kuhn's work on *The Nature of Scientific Revolutions*, and he is right to do so; but he has missed the main thrust of Kuhn's thesis and has therefore misapplied it. Basically, Kuhn suggests that research traditions evolve in cycles of "normal" and "crisis" periods. Normal research builds rather incrementally within some general theoretical orientation; but the very act of research (and, I would add, the conditions of research) ultimately produce a crisis by bringing to light new facts and/or new conditions which cannot be incorporated without progressively greater modification of the general theory. Eventually, the crisis produces new theories, new methods, and, often, a completely new orientation to research.

Anthropology has, I believe, seen two major crises of this kind. The first was a fieldwork revolution that had its roots in natural-history method but provided a new thrust away from the somewhat simplistic evolutionism of the late 19th century. How the fieldwork tradition played itself out as "normal" research in England, the United States, France, and elsewhere was a function of local academic conditions and of conditions in the particular areas of the world where each national group did most of its fieldwork. Out of this work came the holistic approach, social structural studies, psychological anthropology, acculturation studies, and, above all, a unit, the tribe or ethnic group. By knowing about tribes, we achieved both an academic role and a role in the overseas colonies of the imperial powers. As long as the groups we studied remained isolated or semi-isolated, they retained their integrity as wholes, and the study of the interrelationship of their parts and the comparison of these relationships across a number of such units were methodologically appropriate, defensible, and enriching to the general social science community.

The second "revolution" began with the end of World War II. Since then, tribal societies have come more and more to play a role in modern nation-states in which rapid change for the country as a unit is a basic assumption of the leadership. Suddenly, the units of anthropological study have lost much of their wholeness, because one of their most significant features is now the fact that they are parts of larger nation-states. Despres goes to great lengths to show that we in anthropology have dealt with such problems before. We have theorized that social structure is not really a unified whole but a series of networks or segments that cannot be tied together (Nadel 1957); we have theorized about

the implications of cultural pluralism, through the work of M. G. Smith on the Caribbean (1960) and others (Steward et al. 1956). Despres thus tries to carry our previous theorizing forward, amending it and allowing it to grow; certainly he sees no such sharp change as might be demanded by a crisis in Kuhn's sense of the word.

Here is where his position seems to me shortsighted and unrealistic. The concepts of "plural" societies or "broker" institutions cannot bridge the gap between simple, homogenous communities, or ethnic units, and the kinds of units with which we have to deal today. Certainly, in some cases where local ethnic groups have maintained an organizational integrity, the concept of a plural society (involving, as it does, the maintenance of our old units as entities to be studied) may have some bearing on what is happening. Surely, however, such concepts cannot tell us much about trade unions, neighborhood groups in the new cities, political parties at the local level, university students with multi-ethnic backgrounds, attitudes to modern versus traditional views of life among multi-ethnic populations, and a host of other problems, within which ethnicity or community or tribe (i.e., our old units) becomes only one variable in the new research situation. This is our crisis. It provides us with new research orientations; indeed, it forces these orientations upon us. From them, new kinds of data emerge. Recently, I have found that statements by secondary school students in Northern Nigeria on "My Future Marriage" reveal a strong, positive correlation between dependency statements and tribal size: members of traditional state-type societies make many more statements in which Ego's actions are conditioned by deference to other persons than do members of traditional non-state societies. This finding involved written materials by the subjects being investigated, content analysis of the material, and multiple correlations of quantified variables derived from the material. Was this anthropology or sociology or social psychology? Frankly, I am uninterested in the answer. What concerns me is the problems I wish to solve, not whether or not I am maintaining some continuity within the hallowed traditions of my discipline.

Twenty years ago the anthropologist was almost alone in his interest in the social, cultural, and psychological behavior of non-Western man; today he has been joined by many others. The anthropologist has to choose which particular aspect of the behavior of non-Western people he wishes to study. He is no longer the expert on everything. Besides this, the people themselves are changing, and the rapidly developing bureaucracies that

review research (especially in Africa, where I have worked) want to know rather explicitly what it is that the researcher is coming to ask questions about. Furthermore, change, urbanization, Western education, modern nationalism, and other forces are producing wide variations in attitudes and responses to questions. Such situations require some quantitative measure of the variations involved, and this particular methodology involves expanding one's research efforts while restricting their scope. These restrictions, imposed by changing field conditions, must at the same time be informed by some theoretical schema. Where a theory comes from is irrelevant as long as it helps us to understand a particular problem. The rapid development and change of the non-Western world is forcing many of us, especially the younger anthropologists and graduate students, to look at fieldwork in terms of general social science theory, methodology, and techniques.

This does not mean that older problems and directions are necessarily dying out; however, as in England, much of this traditional anthropology may turn towards ethnohistorical studies. In the United States, there is growing interest among those concerned with traditional problems in cross-cultural survey work using ethnography as data and in the reworking of already published ethnographic work. Nevertheless, I suspect that fieldwork still has the strongest attraction for those coming into social, and cultural anthropology, and for these I would predict a new style of research, one that reflects the fact that most of the non-Western world is significantly affected by outside forces. Out of the present "crisis" (in Kuhn's sense) must eventually come new theories, methods, and research direction—all in a tumble, at first, but slowly working themselves out into a "normal" period of consolidation and testing. In the end, anthropology may be, and in all likelihood will be, a very different sort of enterprise than that which Boas started at the beginning of this century, but one that will surely still attract those who wish to understand the human experience.

by MUNRO S. EDMONSON*

New Orleans, La., U.S.A. 16 vi 67

I am one of those, dismissed towards the close of Despres' discussion, who sees anthropology as the science of man. I am not alarmed or cowed by the range of implication of such a definition precisely because I see it (as Despres does not) as a theoretical assertion. To see man as the central concept of our field means to me that the study of fossilized crania, Thai verse, alco-

holism, microliths, and plural societies are relevant to one another only insofar as they illuminate the question of what a man is. Such illumination is offered by only the most partial and occasional results of the study of these disparate phenomena, but it is in these partial and occasional discoveries that I discern the progress of anthropological theory. It follows that there are peculiarities to the anthropological approach to them.

Our only professionally shared conclusion about the nature of man which is both generally accepted and consequential is that man is a cultural animal, indeed (as a matter of degree anyway) *the* cultural animal. This again I see not as a "naturalistic" or naively empirical assertion, but as a theoretical proposition of some considerable power, epistemologically, methodologically, and substantively. The assertion has consequences; it generates corollaries; it can easily be rendered paradigmatic (if that is desirable). So far as I am aware, no one in the lengthy history of the use of this central concept in our field has ever seriously confined it to primitive people.

Perhaps the simplest phrasing of the status of culture in current anthropological theory would be a definition equating culture with complex communication. What is peculiar to man is a complex communication system rarely approached and never equalled in other animals. I hazard the guess that such a phrasing will find more general acceptance among modern anthropologists than the assertion that we are still laboring under limitations imposed upon us by Tylor and Boas. In any case, there is nothing in this view of culture which would confine us to the study of simple societies, nor do I believe that we have been so confined.

Anthropologists with this view of culture must of necessity concern themselves with the channels in which the communications take place, in short with social structure. A theory of such structure is essential to an adequate theory of culture. We have borrowed theories of society wherever we found them, but I do not believe such borrowing to have been quite the act of desperation which Despres implies, nor do I believe we have contributed so negligibly to the improvement of the general understanding of society in and out of anthropology.

I do not agree that we face urgent problems of survival as a discipline requiring us to rush to the task of finding a niche for ourselves in the study of complex cultures, nor do I even see the need for the maintenance of boundaries, whether of anthropology, taxonomies of societies, or of societies themselves. Even when labelled as a

paradigm, a Linnaean taxonomy of the world's societies is not a timely nor even a very useful task. For the distinctive services that anthropological methods can perform in a complex society, I would refer not to the community studies, but to such monuments as Sahagún's *Relación*, Lane's *Modern Egyptians*, Granet's *La Chine antique*, or Freyre's *Casa grande e senzala*.

The idea that a social taxonomy can restore to us the manageable unit "peoples" that we used to perceive as natural isolates of our study (and that it can do so in the study of complex cultures) seems to me untenable. To judge the "pluralism" of cultures and societies we must have a clear and unequivocal basis for counting them. We do not, and it is precisely this lack which has forced us increasingly to abandon functionalistic theories of both societies and cultures in favor of formulations which can better fit the facts. Complex societies confront us with plural and overlapping cultural and social relationships. They have structure, but they do not have boundaries. Judgments about the singularity or pluralism (or homo- or heterogeneity) of over-all systems strike me as far too crude to help.

The adoption of contrast definitions of society and culture can generate models of the kind here proposed, but they must remain ideal types, and they give rise to artificial dilemmas. Society and culture are not empirical isolates but differentiated conceptualizations. Once we abandon the idea of boundaries, the structures of society and culture may be seen to correspond. The isomorphism of culture (e.g., language) and society (e.g., marriage arrangements) in Leach's study of northern Burma is a striking example, even though it eludes Leach.

The central dilemmas of anthropology today seem to me to result not from a lack of theory but from some pretty difficult and pretty central problems—problems we have discovered and explored but not solved. Among these the central one in my view is the paradox of cultural relativism. Retreats to the theories which led us to this impasse, whether evolutionary, functional, or historical, do not meet the challenge of already extant anthropological theory. Model-building only temporizes. In the fashion of most sciences confronting hard problems, we are sidling up to this one, but it will require a massive revision of our fundamental epistemology, and as a profession we confront this with reluctance.

I believe that this problem will be—in fact, is being—solved. I further believe that its solution will be of more moment to the study of man, society,

and culture than any possible change in the locus of our fieldwork.

by KAREL FOJtíK*

Brno, Czechoslovakia. 5 v 67

Despres' paper represents an interesting contribution to the already considerable number of papers dealing with the present state and future perspectives of ethnology. I agree with both of its author's main ideas: (1) it is not necessary to develop a new anthropological science in order to undertake research in so-called complex societies, and there is no reason to limit ethnological research to the "simple" societies; and (2) the restriction of ethnology to the study of so-called "simple" societies (including European village communities in the 19th century) in the past has contributed to the present one-sidedness of ethnological methodology, and the main task is to proceed from implicit to explicit or paradigmatic theory in ethnology. I may add only a few remarks based on my studies of several industrial cities and, on the other hand, of small towns and villages in Central Europe in the Middle Ages.

Some widespread ideas must be overcome: (a) the idea that the small town or village community was homogeneous and "simple" in the past; analysis of the primary sources of the Middle Ages demonstrates that those communities, although seemingly integrated, were in fact socially and culturally differentiated, composed of relatively closed groups and quasi-groups; (b) the idea of social and cultural isolation of those communities; and (c) the antinomy "Gemeinschaft"—"Gesellschaft"—still valued in European science, both ethnological and sociological—and the conviction that there is no room for friction in the village community, that it is *eo ipso* a "Gemeinschaft."

The one-sided orientation toward "simple" communities (and toward "cléár," unambiguous concepts) has led to a certain one-sidedness in the study of them and to the conviction that any more complex study of any more complex society requires interdisciplinary research. Our experience (see Fojtík and Sirovátká 1961; Skalníková *et al.* 1959; Mjartan *et al.* 1956) has been that what is needed is not the direct help of other social scientists, but rather a knowledge of their results as a starting-point for our own work. For a correct explication of cultural phenomena, not only horizontal, but also vertical (historical), analysis is indispensable: it is impossible to understand, e.g., the institutions of the family life, without it.

It seems to me that it would be useful to introduce, in addition to the concept "plural society," the concept "global society," one comprised of several socially and culturally differentiated groups which are united by some important common goals: whether a society is "plural" or "global" would depend upon the relative intensities of ethnocentrism and (the other side of the same concept) consciousness of belonging to a broader national society.

Finally, it is a pity that this paper is based on the literature in English only. Many treatises in other languages (German, Czech, Russian, etc.) might have been used in its preparation.

by P. C. W. GUTKIND*

Montreal, Canada. 31 v 67

I am afraid that the sudden discovery of "Complex Societies" might be one of those new intellectual gimmicks—perhaps a red herring—which will not take us very far. Why not? We used to say that anthropology was the Science of Man and that our objective was to generalize about man's cultural and social life. Ostensibly, we were interested in all aspects of the culture and society of the human group. We were soon driven, however—under pressure of theoretical straitjackets—to concentrate on allegedly "primitive" and "simple" cultures (simplicity being associated both with technological inferiority and with certain customs which reflected a low level of civilization. Still, our "simple" societies were complex enough to occupy anthropologists in concentrated study over a period of many years, and there is probably no tribal society about which we really know all that there is to know. In short, we have *always* studied complex societies, many of them with pluralistic characteristics, and any anthropologist who insists otherwise is either blind or a fool.

This view is more than a mere play on words or a sentimental romanticizing about tribal societies. Last summer, in the company of Nigerian social scientists at Ibadan University, I was repeatedly told that when anthropologists turn to "complex" societies (assumed to be Western societies or societies of the "Great Tradition"), they invariably contrast these with "primitive" African societies. Some of my colleagues hinted strongly that this new anthropological interest confirmed their suspicions that anthropologists, slowly being driven out of Africa, were now opportunistically adapting to the space age.

We ought to be very clear about what is complex and what is not. I suggest that nothing more is involved than scale. Populations are larger, per-

haps they are spread over a greater area, and perhaps there is a proliferation of groups with a more clearly defined specificity. There are no doubt differences in political organization and social control, but, again, these are a matter of degree and scale rather than the presence or absence, or the relative complexity, of institutional structures. True, there are no political parties (as far as I know) among Bushmen, but any society, however small or "primitive," draws on a complex body of ideas and actions in achieving its ends.

Anthropology has always been "comparative sociology" in the sense that our generalizations about society have drawn on a wide range of societies. Because few, if any, societies are homogeneous (if they were, they would have to be almost total social isolates) or wholly tradition-bound, our observations have always encompassed a wider social field. If anthropology must undergo "corrective surgery," it is because we have been trapped by the view that thus far we have studied allegedly simple and homogeneous groupings. Of course we need new models and new methodologies, but simply because societies around the world show a different type of complexity than they did 50 years ago.

I happen to be interested in Africa's urban areas, said to be very complex structures, but I am aware that their present complexity has its roots in the complexity of the previously rural nature of these societies. As these rural societies become progressively more geared to an urban-commercial-industrial way of life, they simply exhibit a new, and perhaps different, kind of organized complexity.

It is, of course, proper that anthropologists should now pay attention to these changes—and most of us have done just that for some years—but I cannot accept the idea that I am now studying an entity which is more complex than those studied by my colleagues of yesteryear. I might even suggest that this allegedly more complex society is easier to study than a "simple" one because I am forced to observe a smaller unit, the totality being too large to handle—again a matter of scale.

Perhaps we think the societies we are studying today are more complex because in the past we worked alone, within the confines of an often narrowly defined discipline, whereas in recent years cross-disciplinary approaches have given real insight into complexity. Years ago when we described a culture, we all too readily assumed that we were reporting all there was to report. Thus many an incomplete monograph on some "simple" society became a classic. It is true that anthropologists were interested in com-

plex societies long before the era of interdisciplinary research, but this is no evidence that they recognized their complexity. It merely points to the fact that we have always leaned in the direction of comparative sociology and would probably have made more rapid progress if all sorts of theoretical "dead ends" had not diverted us. Here Despres has helped to lead us out of the woods. The model he proposes must now be tested, but those of us who do so might think it useful to apply it to both Bushmen and New York apartment-dwellers. We can then contribute to generalizations about variations of societal complexity and perhaps at the same time avoid hidden value judgments which result in the view that complex societies are somehow superior. Africans, Asians, and Latin Americans feel that they are at least as complex as "Middle-towners." This view, I am sure, Despres will share. I am less sure that I would endorse his plea that "anthropology's paradigmatic focus should be the scientific study of *cultural systems*" (my italics), perhaps because of his use of the concept of culture. It is to this point that we need to address ourselves.

by MARVIN HARRIS*

New York, N.Y., U.S.A. 18 v 67

Despres' premises are spurious. His arguments are well-nigh incoherent. He says there is a crisis caused by the disappearance of primitives; we shall lose our identity as a discipline. But no, there is no crisis, since anthropologists have been dealing with non-primitives since the 19th century. But there is a crisis anyway, since the work done in complex societies by most anthropologists is not good enough or distinctive enough to save us from impending death and transfiguration.

We all know the inadequacies of the extant corpus of anthropological studies of complex societies. But only ignorance of regional institutes and foreign area training and research programs could lead to the conclusion that "the science of culture seems to have very little to contribute... to the study of complex societies" (p. 11). Even Despres seemed to be denying this proposition back on pages (pp. 7-10). One might imagine that Despres would at least make an attempt to back up this extraordinary claim by an analysis of the scientific worthlessness of a few examples of anthropological studies of complex societies. What he gives us instead is an harangue about the need for a theoretical model of cultural systems and for the development of cultural theory. And what is the "theory" which will rescue us? Here is the really insulting part of Despres'

paper. In order for us not to be mistaken for sociologists, we are supposed to adopt a third-hand version of Weberian and Parsonian schema (through Nadel). That is, we are to follow the very strategy which is responsible for the sinking ship of contemporary sociological theory! It was from the vogue of Weber and Parsons in sociology that M. G. Smith, Nadel, and Radcliffe-Brown derived their rationale for separating cultural from social facts. It was sociology that decreed that "cultural facts consist of ideas, values, symbols, and meanings" (p. 9). Most anthropologists, even when they have shared the crippling mentalist and idealist bias of Despres' approach, have always understood that culture also includes techno-environmental and techno-economic facts, energy budgets, work patterns, humanly caused physical and chemical transformations, and movement of things and people. I fail to see how our acceptance of a role set aside for us by sociologists would preserve our identity, supposing it was actually being threatened. The way to strengthen anthropology's claim to a distinct role in the social sciences actually lies in an entirely different direction from that taken by Despres. It lies in the comparative method (the real one, not just cross-cultural comparison) and above all in the unique relationship between cultural anthropology and archeological, biological, and linguistic research and theory.

Despres talks of "human behavior as reflected in the behavior of individuals" (p. 9). What conceivable difference can there be between human behavior and human behavior as "reflected in" human behavior? He argues that if we don't study this "reflection," we shall suffer extinction, for the only option left would be the analysis of "human events." What is he talking about? In view of acceptance of "anthropology's propensity for configurational analysis and its tendency to reject operationalism" (p. 17), we are not likely to find out.

by HENNING SIVERTS*

Bergen, Norway. 19 v 67

Major problems do face anthropologists who turn their attention to "large-scale societies" (cf. Mair 1963), but, as I suppose Despres would agree, other social scientists, notably sociologists, are probably not much better off methodologically or theoretically. They do the best they can with the means they have at their disposal, and they too, like the anthropologists, limit their field of inquiry, e.g., by concentrating on "the total institution" such as the prison or the asylum.

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I agree with Despres that anthropologists ought to be asking themselves what kind of research in complex societies "might make a meaningful contribution to the development of anthropological theory." I have the impression, however, that the growing number of anthropologists who do study peasantry, urban life, and other phenomena subsumed under the heading "heterogeneous society" are working with precisely this objective in mind. Indeed, I am convinced that the author would have gained additional insight into the present topic if he had given serious treatment to studies like *Village India* (Marriot 1955), *Aspects of Caste in India, Ceylon, and North West Pakistan* (Leach 1962), and *The Social Anthropology of Complex Societies* (Banton 1966), as well as studies by Geertz (1963), Bailey (1960), Mayer (1960), and Barth (1964, 1966).

The author argues for a paradigmatic focus in anthropology, but I fail to see how his own proposal provides a point of departure for the "scientific study of *cultural systems*" (my italics). His paradigms seem to represent just another typology of epiphenomena involving both cultural and social aspects. My main objection is to his view that "culture, and not human behavior or social relationships, should be the substantive focus of anthropology." The *substantive* focus of anthropology is human action, and our primary goal is to discover regularities of behaviour and to analyse how such regularities are maintained or generated. Since humans have ideas about right and wrong and pursue strategies that involve calculations of gains and losses with regard to values, anthropologists address themselves to the study of culture as well as society. Crucial to their task is to relate social behavior to the conceptual and valuational systems of the actors in different societies, whether they be classified as complex or not.

by JULIAN H. STEWARD*

Urbana, Ill., U.S.A. 12 v 67

Despres has given us a certain historical perspective on modern anthropology's involvement in studies of complex societies, but I offer additional facets to this history. The 1930's mark the period of crucial change, although I should not say crisis.

Through the first quarter of the present century, complex states, especially of antiquity, were viewed in terms of their cultural achievements, with only incidental glances at their class structures. North American Indians were ideally described by holistic ethnographies, and the concept of sub-

culture was not needed. Industrial developments during the 1920's, however, penetrated most Indian societies so strongly that the gleanings of aboriginal ethnography rapidly diminished. In the mid-'30's the late John Provinse cited a Hopi belief that their society would have vanished when a large, exfoliating boulder near their villages had disappeared. Will anthropology, he asked, cease to exist when aboriginal tribal cultures are gone?

Anthropology did adopt these changing cultures as part of its subject matter. A formal acknowledgment of this was the Redfield-Linton-Herskovits statement on acculturation. Indians were viewed not only as acculturated societies but as ethnic minorities within the United States. Interest was soon extended to other ethnic minorities, such as Italians, Greeks, and Chinese, who were distinctive because they were immigrants from other cultures, and to Negroes, who at first were also considered an ethnic minority, but "old Americans" were accorded very little attention. Because so many ethnic minorities lived in cities, especially after World War II, studies of urbanization became a special category.

The methodology for studying these ethnic minorities, however, was a transfer of that used for tribal societies; it became known as "community studies." In 1950, I pointed out in *Area Research: Theory and Practice* that this method was inadequate because it failed to take into account the larger state or national context of the community or subcultural group. In 1951, I offered the concept of levels of sociocultural integration as a means of distinguishing national-level economic, political, religious and other institutions from locally distinctive community-level features. In 1956, my colleagues and I applied this concept in *The People of Puerto Rico*, distinguishing the island-wide and even the international institutions from their specific effects on the different local subcultures.

During subsequent years it has seemed to me that this very fundamental though essentially simple concept is indispensable to filling in the larger context of any subcultural segment of society, but little use has been made of it. If I read Despres correctly, his "broker institutions" are my national-level institutions—and of course there are institutions at such intermediate levels as states, regions, cities and others—and his "cultural sections" are subcultural groups. It appears, moreover, that his Tables 1 and 2 are strikingly similar to my own comparative tabulation of national-level institutions and their local manifesta-

tions in Puerto Rico. Possibly I fail to grasp the language of the New Anthropology, but it is my intention to trace the continuity in anthropology.

In assessing the adequacy of established concepts, such as subculture, a word of caution is in order against ascribing precise, universal meaning to such concepts. Subculture means no more than a social segment which is somewhat distinctive in its culture. The nature and degree of this distinctiveness vary widely and are a matter of empirical determination in each case. Whether the existence of several subcultures within the context of similar national institutions shall be designated a plural society depends solely on the decision concerning how different their differences must be.

Anthropology's attention to ethnic minorities has meant choice of Indian societies and immigrants who retain an ethnicity different from that largely fictitious "old American" or Anglo-Protestant American culture. As the processes of change induced by national institutions and interactions with other subcultural groups eliminate or modify the alien ethnicity, distinctiveness follows lines of region, class, occupation, and other factors. The definition of plural societies might subsume these modified groups. More important than definitions, however, are problems that anthropology has yet to face. It has not dealt with the number of years or generations required for the loss of ethnic identity of minority groups or with the kinds of ethnic minorities that resist assimilation. One might test, for example, the hypothesis that individual families of immigrants would lose their alien features much faster than groups that introduced community-level institutions, such as Chinatowns. A problem on which we have neither studies nor hypotheses is what kinds of occupations entail a subcultural distinctiveness in off-the-job behavior.

I have assumed that anthropologists *qua* anthropologists deal primarily with subcultural segments of complex societies. Although this is not necessarily so, this delimited task poses methodological problems of recognizing, understanding, and utilizing data concerning the larger society that have been the subject matter of other disciplines. If we are to analyze national societies in a holistic manner, and include their many institutions, social segments, and international contexts, the task must fall to extraordinarily knowledgeable persons. This might be an ideal of area programs, but few problem formulations are interdisciplinary in any such heroic terms.

by NORMAN E. WHITTEN, JR.*

St. Louis, Mo., U.S.A. 13 VII 67

I applaud the emphasis placed on the question of what studies in "complex situations" can contribute to the development of cultural theory and theories of power. A preliminary understanding of our own development in methodology and theory should accompany a positive disciplinary contribution toward understanding complex societies.

I wish to supplement Dr. Despres' clarifying discussion by making a suggestion pertaining to the study of plural societies. My comment follows attempts to circumvent sterile arguments about organization vs. disorganization in the study of economically depressed ethnic categories of people such as those labeled "the New World Negro." Like Despres, I want to come to grips with the perplexing problem of cultural continuity and structural variation, on the one hand, and similarity of structural position and variety of forms and content of symbolic expression, on the other hand. These problems constantly define the paradoxes faced by scholars who focus on a discernible class-ethnic sector of a complex sociocultural system within a broad geographic area.

Despres states (p. 15), "what is needed [in anthropology] . . . is a paradigmatic focus in terms of which analytical models can be developed with a view to the construction of cultural theory." This can be rephrased to imply a need for clarity in understanding the functions of various segments in the plural model (which Despres says culminates in theories of culture) and the reticulated model (which he suggests eventuates in theories of power). I find a key to plurality to be the concept of *strategy* in the discussion of power, the concept of *style* in the study of culture.

The study of power may be regarded, simply, as the study of the tactical use of strategic resources (social, political, economic, and symbolic). Successful strategies lead to power inasmuch as they eventuate in control over some sector or domain based upon discernible resources. Cognition of resources and the style in which strategic resources are regularly manipulated defines the pattern which configures into a "subculture." When one proceeds from ideas such as these, certain critical questions must be asked of data gathered within a segment of a plural society, particularly when the segment under investigation is economically depressed, i.e., when most or all of the members are excluded from

full competition over the sources of economic advancement. I find the following series of questions useful in establishing a methodological base:

1) What strategies *must* be played for success and for survival given the economic, social, and political framework? What resources are available to the ethnic sector under investigation?

2) What is the probable developmental cycle of successful strategies worked out by the ethnographer? Other things being equal, or held constant, what is the probable developmental cycle of success that people's striving at any particular time seems to represent?

3) What are the symbolic boundaries defined by socially standardized and culturally acceptable coping mechanisms? What do people perceive and conceptualize as the ways in which the successful manipulate their milieux? What is the style of game-playing, and what are the people's concepts of "power"?

4) Is there a tendency toward synthesis of the strategies people are willing to use (their game style) and the success strategies which can be worked out by the ethnographer?

5) Assuming change to be a constant in any system, and assuming cognizance of strategic resources, both introspectively (folk-psychology and folk-sociology) and by observations of other ethnic coping sectors (folk-ethnography), what is the relation of "power" and "culture" to social structural transformation and cultural drift?

Since power can be examined in terms of the relationships between the analyst's derived developmental cycle of successful strategies and the culturally acceptable style of tactical coping, some hope for improved understanding of sociocultural change (both evolutionary and involutional) should eventuate from such formulations.

These suggestions are offered to supplement Despres' discussion of the need for a paradigmatic focus. They should be regarded as notes toward a productive methodology for understanding the power structure and cultural configuration of "plural societies" and for understanding the configurational style and strategic game-playing of coping sectors within a plural society. I do not think the notion of a choice between reticulated and plural models is the appropriate paradigmatic focus, and offer these methodological notes in the hope that synthesis of "theories of power" and "theories of culture" may be made.

Reply

by LEO A. DESPRES*

Ohio, U.S.A. 18 VII 67

In view of the foregoing comments, it is apparent that there are ambiguities in my paper that need clarification. One of these involves the central theme of my argument—i.e., that if anthropology is to make a *special* contribution to the study of complex societies it must have theories which are capable of informing anthropologists of significant research problems. The development of such theories necessitates at least some level of agreement among anthropologists as to the discipline's paradigmatic focus.

This argument is predicated upon the proposition that either anthropology claims a special status among the sciences of man or it does not. If the latter, as Cohen seems to suggest, then what passes for a scientific discipline can be little more than an organization of scientists who enjoy communicating with one another about their varied and exciting research experiences. I do not wish to imply that anthropology cannot be such an organization of individuals: obviously, it is this to a large extent. But, as both Bennett and Edmundson seem to suggest, is this all that anthropology must be? If so, then we ought to recognize that there are some serious difficulties enjoined by this view. Some of these difficulties are most clearly exhibited in Cohen's argument.

For example, contrary to Edmundson's plea for a "science of man," Cohen notes that the anthropologist is "no longer the expert on everything." (I am inclined to agree with Gutkind that the anthropologist never was the expert on everything.) However, if the anthropologist is not expert on everything, on what is he expert? Cohen fails to tell us. In fact, he indicates with reference to his own research in Africa that the question is irrelevant because what he is concerned with is the problems he wishes to solve. But where does Cohen get these problems? That too is unimportant. Clearly, they do not derive from any particular paradigmatic focus because "where a theory comes from is irrelevant as long as it helps us to understand a particular problem." Perhaps the problems come from the changing field situation (they simply exist for somebody and therefore they must be solved), or perhaps they are defined by "the rapidly developing bureaucracies that review research."

If it is unimportant where the research problems come from and unimportant who defines them, why should anthropologists—or any other social scientists for that matter—be

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concerned with their investigation? Cohen probably would respond by suggesting that such problems inform us about human behavior. Has he come around to Edmundson's point of view? Is Cohen ultimately making a plea for a "science of man?" Not quite. He notes, for example, the "anthropologist has to choose which particular aspect of behavior of non-Western people he wishes to study." (Why non-Western people?) But this is the thrust of my argument and this is precisely what Cohen seems to object to. In short, if I follow the logic of Cohen's position, the anthropologist who is no longer expert on everything does not seem to want to become expert on anything. Perhaps, for a time, such anthropologists can survive as general handymen for those social scientists who know what they are about but who have not yet made their way into the non-Western field?

It is precisely these kinds of considerations that prompted me to raise the issue of anthropology's paradigmatic focus. To assert, with Edmundson, that anthropology can continue to persist as "the science of man" is to lay claim to a status which cannot be validated. Moreover, as practically all of my colleagues have noted, anthropologists no longer need to be expert on everything: they no longer have to work alone in exotic and isolated world areas. However, to suggest that we can work with others without some serious effort to delimit the scope of our naïvety, is to disclaim altogether the status of scientists and claim the status of handymen. The question, then, of anthropology's paradigmatic focus is both legitimate and critical. However, as Bennett indicates, this question is not easily resolved.

It is apparent from the thrust of Harris' and Bennett's comments that my proposal to limit the focus of anthropological science to the cultural field of inquiry has not been advanced without some ambiguity either. Here, the problem seems to derive primarily from my failure to explore more thoroughly the ontological position of the culture concept and to consider more completely the epistemological difficulties associated with its uses. Without further apology, I should like to clarify some of the points at issue.

I believe there are a number of points on which Harris and I are in fundamental agreement. If I follow his argument in *The Nature of Cultural Things* (1964), we agree that the paradigmatic focus of cultural anthropology is the cultural field of inquiry. With specific reference to his argument, we also seem to agree on the following: (1) that human behavior constitutes the cultural field of in-

quiry (p. 20); (2) that human responses are not to be construed as culture or, to use Harris' words, as "cultural things" (p. 22); and (3) that cultural things are scientific concepts, models to use my terms, which the observer creates by interacting with and segmenting (classifying) the human behavior stream (pp. 23-35).

If human behavior constitutes the cultural field of inquiry, how is this field to be delimited? How is the anthropological observer to segment the human behavior stream and create theoretically meaningful and scientifically useful units (models)? There are fundamentally two ways of accomplishing this. It can be done by definition or it can be done by delineating the replicable operations with which the observer constructs the units he intends to use for analysis. I have elected the former of these two approaches and defined culture as a model construct based on the observation of the intergenerational continuity in human values and cognitive orientations. Harris, on the other hand, utilizes the operational approach. For him, culture is comprised of operationally constructed units of human action—actones, episodes, nodes, nodal chains, scenes, serials, nomoclones, permacrones, paragroups, nomoclonic types, permaclonic types, permaclonic systems, and permaclonic supersystems. It is also phonemes, morphemes, words, semantically equivalent utterances, behavior plans, "and many other emic things" (168).

Harris argues that the actonic or operational approach to the cultural field of inquiry more rigorously conforms to the canons of science. I am inclined to disagree. While the two approaches are mutually exclusive in logic, they rarely seem to be exclusive in practice. Perhaps the philosophers of science will have to decide the issue. However, in the event that their decision should be unfavorable to my point of view, there are at least three reasons why I am prepared to compromise the canons of science. First, the actonic approach excludes considerations of an historical nature with respect to the human behavior stream. Second, it excludes considerations pertaining to the reasons people give for doing the things they do. And third, apart from its scientific pretensions, the actonic approach is a closed system of thought by definition rather than by hypothesis-testing. For instance, Harris states (170): "...if there is any consistent relationship at all between etic and emic phenomena, it can only be one in which emic things are ultimately shaped by the conditions of the material world."

Apart from the difficulties of re-

solving these ontological and epistemological issues, Bennett seems to be particularly concerned with two other problems about which I have apparently failed to achieve clarity. He notes that a study of culture as values tends to become a study of values and not a study of culture. He also notes that a particular dimensional definition of culture generally leaves anthropologists with a virtually unsolvable causal dilemma—do values cause other phenomena or are they caused by it?

It seems to me that these two problems result from the failure to keep separate events in the empirical world (data units) and the data language (models) with which we organize these events for purposes of analysis. It is the old problem of reification. Values are particular kinds of models with which observers organize their observations on recurrent segments of the behavior streams of individuals. Strictly speaking, these models do not cause anything (not even when reified). With respect to these models, the issue of causality resolves itself into the following question: How much variation in the behavior streams of particular individuals can be predicted by a particular value model?

Models of values are not the same unit constructs as models of value systems. It is the latter models which I

consider to be of the cultural order. These models do not express variations in the behavior streams of individuals but rather variations in the values attached to the intergenerational structuring of groups and other types of societal units. Strictly speaking, these models also do not "cause" anything. However, with these models we can formulate research problems. For example, from particular cultural models we can seek to ascertain how much variation we can predict in the activities expressed by the structuring of groups, institutions, and other kinds of societal units.

It seems to me that if one sharply differentiates between data units and data language (models), still another problem that Bennett mentions tends to disappear. I refer to the problem of studying cultural phenomena to the exclusion of other "kinds" of phenomena—one dimension of which is the old society and culture bugaboo. I emphasize again, culture is not phenomena: it is a model with which phenomena are arranged for particular analytical objectives. The cultural, social, and psychological fields of inquiry deal with the same phenomena, but they utilize different kinds of theoretical problems. This stance does not preclude the consideration that with reference to a particular research

problem it may be extremely useful to draw upon the models of all three fields of inquiry. Without a paradigmatic focus, however, it is unlikely that sophisticated cultural models will be available.

A final point of clarification that seems in order with reference to my paper has to do with my discussion of the plural model. Several of my colleagues seem to have received the impression that I was putting forth this particular model as the only one suitable for a cultural analysis or as the one most appropriate for the cultural analysis of complex societies. I did not intend to create this impression. My discussion of the plural model was intended to illustrate a major difference which I perceive between one type of cultural analysis and one of the dominant versions of sociological analysis. It was also intended to illustrate how the uses of a cultural model is capable of generating anthropological problems of theoretical significance with respect to the study of complex societies. I quite agree with both Gutkind and Bennett that the heuristic value of this particular model remains to be established. For a discussion along these lines, I would recommend a series of papers to appear in a book edited by M. G. Smith and Leo Kuper on *Pluralism in Africa*.

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