CHAPTER VII

Problems of Process: Results

Chairmen: Fred Eggan and Daryll Forde.

Papers discussed: Kluckhohn, "Universal Categories of Culture";* Hoijer, "The Relation of Language to Culture";* Martinet, "Structural Linguistics";* Lévi-Strauss, "Social Structure";* Boyd, "The Contributions of Genetics to Anthropology";* Steward, "Evolution and Process."

Speakers: Eggan, Boyd, Washburn, Kluckhohn, Murdock,† Henry, Hoijer, Kennard,† Martinet, Lévi-Strauss, Mead, Willey, Bennett, Kroeber, Nadel, Schapiro, Brew, Greenberg, Caso, Linton, White, Redfield, Forde, Steward, Lewis, Beals, Rowe.

INTRODUCTION

EGGAN: With this session, we leave the solid ground of archeological and ethnological fact and enter the world of abstraction. In a sense, we have already been in this world for several sessions. It is now time to become aware of that fact and to explore some of the consequences.

In the papers for this session the term "process" does not appear too often. This covert aspect of process may be due to the fact that, to quote Walter Goldschmidt, no process is really ever seen. It is only inferred from an examination of conditions. . . .

We might look first at the units to be compared. Do genes and chromosomes correspond in any meaningful way with concepts such as phonemes and morphemes or, in cultural anthropology, with items and traits or, in social structure, with social relations or subdivisions of them? Is it possible to compare genetic processes with the processes by which culture is acquired by the individual or with the processes of phonetic and morphological change?

A key question. . . . Does the concept of structure have a parallel significance in human biology, in culture, in society, and in language? Do such processes as adaption and selection operate in any or all these fields? Can we use concepts such as genetic drift, cultural drift, linguistic drift, in any but an analogic way? . . .

GENE FREQUENCES

BOYD: When Birdsell studied the Australian aborigines, he found that M and N frequencies were unique. Since then, Simmons and others in Australia have found that there is a gradient in M and N frequencies from Australia through the islands to the north. But . . . M and N frequencies in Australia and New Guinea are not very different, although the A and B frequencies are; this puzzled us. . . .

Recently, it has been discovered that the M and N system can be subdivided by a new series called anti-S. In the accompanying table of frequencies, s means the absence of reaction with the serum, S means the reaction. You note that there is no S at all in Australia, whereas in New Guinea there is some, though not a lot. The Maori differ somewhat but are more or less what you would expect. In a typical European population—the English—you have a lot of S reaction, which is obviously very low in the Pacific and zero in Australia.

	Ms	MS	Ns	NS
Australian aborigines	0.256	0	0.344	0
New Guinea, natives	0.04	0.01	0.075	0.28
Maori of New Zealand	0.500	0.14	0.458	0.048
English	0.285	0.255	0.386	0.074

WASHBURN: There is a paper by Miller, giving the racial incidence of the T factor, showing the variation.

BOYD: That is a much more difficult thing to work with serologically.

WASHBURN: It varied from a little over 20 to almost 95 in human races, with Duffy varying from a little over 20 to almost 100 per cent in different human races. Had we found variations of this magnitude in cephalic index, stature, or nose shape, anthropologists would have said, "Well, this is a new era in physical anthropology." But, since they are in blood groups, they do not seem to have made much impression. Actually, a variation of from 25 to almost 100 per cent is almost an ideal indicator of race, and to have this in two different independent known genes almost doubles the amount of useful blood-group information. . . .

CATEGORIES OF UNIVERSAL CULTURE

KLUCKHOHN: Julian Steward wrote me about my paper that "I have long wondered whether we might not approach universals and limitations from the outside, as it were, by writing an imaginary ethnology of wholly impossible practices and thus close in on what is possible. I am half facetious in this, but I think it has a point. [This would be amusing and not altogether unprofitable.]

"Second, I wonder whether it is logically and methodologically necessary to make universals the alternative to the relativistically unique. Isn't there a considerable range of recurrent forms and functions that have less than universal distribution? Thus, clans might be a significant, though not universal, category. I am not objecting to universals; I merely raise the question whether there is not this third class of categories which show parallels in special but limited circumstances."

I would agree that it is important to discover what categories have modal distributions, even though not universal. . . .

Professor Nadel said yesterday . . . that when all was said and done, we would still talk about material culture or technology, social organization, etc. In a certain sense this is true, just as we will always talk about youth, age, noses, etc. . . . But my point is that in some of them the vocabulary we use is loaded in terms of our own culture. Some of the nine categories in Wissler's universal pattern slice the pie in a way that corresponds to the traditions of our culture. In other cultures there would, in certain instances, be a good deal of twisting and cutting off to get the data into this pigeonhole rather than that one.

I feel in accord with Professor Nadel's suggestion that we should take modern logic as a model and deal with categories like "inclusiveness" and "exclusiveness" and so on—which perhaps correspond to the operations which human beings, with their particular kind of nervous systems, can and do perform universally—where there is a minimum of begging of questions by the terms chosen.

MURDOCK: This subject of universals in culture is one with which I have concerned myself, and most of the comments that I might make are available in published form. . . .

I merely want to say that I find myself in very substantial agreement with Dr. Kluckhohn. . . . He has assessed the existing status of the situation admirably. . . .

HENRY: The problem, in deriving categories of universal culture, is that such categories will not be substantive but will be highly abstract and will therefore have the qualities of invariance.

If one tries to find categories that are exceedingly concrete, the chances of failure are very high. I would like to suggest some categories of universal culture which have been very useful to me and which are ordinarily not included in general anthropological discussion. These have the quality of a high level of abstraction and consequent invariance.

I would like to consider the relationship between activities performed under constraint and as free choice . . . between a punishment for nonconformity and a reward and recognition for conformity, between what the culture promises and what the culture actually gives, between painful experiences and gratifying experiences, also the relationship between the consideration one must give to the satisfaction of one's own needs as contrasted with the consideration one must give to the satisfaction of other people's needs.

All these categories can be found in all human cultures; therefore, they suggest categories of universal culture.

KLUCKHOHN: I am in complete agreement with what Jules Henry has said about abstract categories.

CULTURAL LINGUISTICS

HOIJER: I have very little to add to the Sapir or Whorf hypotheses that I discuss in my paper. . . . I think, however, it is probably necessary to remind you that Whorf's hypothesis is perhaps only a hunch and requires a great deal of research before it can be tested adequately. My excuse for presenting it, apart from the fact that it forms a part of the inventory of anthropology, is that it seems to me to present an unparalleled opportunity to link up the many aspects of culture in ways that may be profitable and fruitful, and then we can go on from there.

KENNARD: Since the concept of structure and organization is so basic to a description of any language, it is impossible to arrive at the basic phonemic entities as they occur and to trace their distribution, or even to arrive at a listing of the morphemes which occur within any given system, without a concept of structure. . . .

Linguistic data lend themselves particularly to handling in this fashion because of their formal nature. The total entities . . . and their distribution within any given linguistic system are always limited.

The problem one has in doing a linguistic analysis, when one tries to limit one's self to the formal situation and to handle one's data without reference to meaning, is that one cannot handle the problems of the relationship of formal categories and formal arrangements in terms of conceptualization of material which are symbolized in any given language until one has completed a formal microlinguistic analysis. . . . This is a prerequisite to an examination of the interpenetration that occurs between linguistic forms and the ways in which any given people conceive of their experience and speak about the significant relationships of their particular world.

You will remember that, when Whorf described his work, he pointed out that initially, when he was handling both the bases and the categories in Hopi, this language seemed to him basically similar to European languages, and it was only after becoming aware hat the limitations and distributions and modifications which occurred within the Hopi system were not intelligible and did not parallel those with which he was familiar that the series of articles about the relation of language to thought and behavior was published.

The fact that related languages can be spoken by people of quite different cultures has been documented over and over again. Herein is an excellent problem which could be systematically investigated and would not be too difficult.

In the Southwest, we have relatively full data on the Hopi, Zuni, and Taos cultures, and also a considerable amount of analysis of the languages. Here we have a situation in which historically distinct languages are spoken by people who share many cultural characteristics; it should then be possible to determine whether the activities which can be objectively observed are conceived and related by the speakers of these different languages in similar ways. This, at least, is a problem which is within the competence of our existing techniques. . . .

I think this problem is significantly related to the problem Dr. Kluckhohn mentioned when he pointed out that universal categories tend to be loaded. In social science terminology, we find phrases such as "social space" and "social distance," and it is clear that the creation of these technical terms is related to our way of handling relationships in these terms, both space relationships and other types of relationships which are translated into spatial terms.

Another point worth making: The speakers of any language generally completely control the structure of that language by the time they are six years old, usually earlier. . . . Since a great deal of learning is presented in verbal terms, the chance to impose a particular way of ordering experience . . . is continually present. I was wondering whether this might not be responsible for the remark that Professor Martinet made in his paper that certain European linguists, in dealing with languages with which they are thoroughly familiar, tend to rely on their own intuitive feeling as to the nature and characteristics of the language. . . .

A further point is that there is no other series of data which can demonstrate so clearly the fact that the people who are functioning or operating through a particular system are not aware how that particular system operates. No speaker ever knows how many stresses occur in his own particular language, the number of vowels, or what the system of patterning is; what he presents the analyst with is a model from which the linguist makes the necessary analysis. It is precisely because of this automatic nature of a linguistic system . . . that investigation of the subtle manner in which it tends to modify or give man an image of significant relations to the world is so important.

STRUCTURAL LINGUISTICS

MARTINET: The mention of the word "drift" by Dr. Eggan reminds me that I should have tied up what I say in the second last paragraph of my paper with the concept of "drift" which was suggested by Sapir. I think there are some linguists who are interested in trying to determine more exactly what is meant by drift in linguistics—more particularly in phonology, phonemic evolution.

This idea of an internal drift in language has not received the attention it deserves from structural linguists. There is a possibility of determining how the phonemic pattern of a language is likely to change—I would not say irrespective of external influences, but in a direction which will largely be determined by the internal structure itself. In other words, if . . . two different languages with two different phonemic patterns are submitted to similar external influences, we would probably be able to ascertain that the phonemic evolution of the two languages will differ, and differ in such a way that the differences can be accounted for largely as due to the differences of the phonemic structures of the language in question.

EGGAN: You raised the question, Professor Martinet, whether linguistic structure is an artifact created by the analyst or whether it is actually implicit in the material.

KENNARD: This same problem arises in the question of the reality of phonemes, in other words, whether the phoneme is a construct created by the linguist for his convenience in ordering his data. But the phoneme can be subjected to operational tests. Two variants may have many acoustic features in common and function and be recognized by speakers of English as one phoneme; in a contrasting language people who are conditioned to hear and respond to only one of these units, a significant unit, have a terrific problem in learning both to hear and to make a distinction between these features, because within their own language they have learned to respond to only one.

Take a group of Hopi school children, and consider the distinction in allophones which we recognize in *pot* and *spot*. Now the unaspirated *p* is all that occurs in Hopi, so that when you are facing the problem of dealing with children and they do not distinguish between what we ordinarily symbolize with the letter *b* or the letter *p*, they will actually say, when you give them the spelling in English, "Do you mean a *p* like this (indicating *p*) or a *p* like that (indicating *b*)?" This seems pretty conclusive evidence for the psychological reality of that unit which we describe as a phoneme in linguistic analysis.

SOCIAL STRUCTURE

LÉVI-STRAUSS: Dr. Eggan mentioned that the term "process" does not appear often in the inventory papers being discussed. I must confess that, as a foreigner, I had difficulty in understanding exactly what was meant by the opposition between "process" and "result," and, although I was not quite clear what was meant by "process," I assumed that it was something different from result and for that reason, I left results completely out of my paper.

This is to be regretted, because in this symposium we have no paper assessing what we actually know—the results. This is a very serious gap in our program, since cultural anthropology . . . is certainly one of the most important fields, in which dozens and dozens of anthropologists are working. We are incurring the risk that we will not know what we have achieved.

MURDOCK: Dr. Lévi-Strauss's paper presents certain difficulties, the reverse of the difficulties which Dr. Lévi-Strauss a moment ago mentioned—that he had with the concept of process. Although anthropologists in different countries are commonly talking about the same thing, they frequently use different terminology . . . and much of the difficulty that some readers have had with Dr. Lévi-Strauss's paper revolves around . . . the use of certain terms and forms of expression which may initially rub an American the wrong way.

Dr. Lévi-Strauss cites Dr. Kroeber with respect to the use of the term "social structure." "The term, 'structure,'" as Dr. Kroeber says, "appears to be just a yielding to a word that has a perfectly good meaning but suddenly becomes fashionably attractive for a decade or so, like 'streamlining,'" and so on. I would agree with Dr. Kroeber here. I do not lay much stress on the words that are used . . . what we mean by those words is important. Dr. Lévi-Strauss has been good enough to pass on to me a letter which Radcliffe-Brown wrote commenting on his paper, and I shall read a few of these comments.

"As you have recognized," says Radcliffe-Brown, "I use the term 'social structure' in a sense so different from yours as to make discussion so difficult as to be unlikely to be profitable. While for you, social structure has nothing to do with reality but with models that are built up, I regard the social structure as a reality. When I pick up a particular sea shell on the beach, I recognize it as having a particular structure. I may find other shells of the same species which have a similar structure, so that I can say there is a form of structure characteristic of the species. By examining a number of different species, I may be able to recognize a certain general structural form or principle, that of a helix, which could be expressed by means of logarithmic equation. I take it that the equation is what you mean by 'model.' I examine a local group of Australian aborigines and find an arrangement of persons in a certain number of families. This, I call the social structure of that particular group at that moment of time. Another local group has a structure that is in important ways similar to that of the first. By examining a representative sample of local groups in one region, I can describe a certain form of structure.

"I am not sure whether by 'model' you mean the structural form itself or my description of it. The structural form itself may be discovered by observation, including statistical observation, but cannot be experimented on.

"You will see that your paper leaves me extremely puzzled as to your meaning. In dealing with Australian kinship systems, I am really only concerned with arriving at correct descriptions of particular systems and arranging them in a valid typological classification. I regard any genetic hypothesis as being of very little importance, since it cannot be more than a hypothesis or conjecture."

One brief comment on the last point: I feel that this position . . . is the greatest source of weakness in Radcliffe-Brown's work. With respect to social organization or social structure, we must always consider a structure moving through time, and our attention should be focused on a dynamic moving and changing equilibrium. . . . The diachronic aspect is essential to an understanding of the synchronic aspect.

To return to the first part of Dr. Radcliffe-Brown's comments, I think I understand what Dr. Lévi-Strauss means when he distinguishes social structure from reality. . . . As I see it, structure is an organization or a framework that has some permanence. If one makes an observation at a given moment . . . one has merely a number of concurrent elements that happen to be found together, as, for example, in a photograph. It is only when one observes numerous instances and sees what is constant and enduring and repetitive and what adheres to what that one discovers structure; so that Radcliffe-Brown's finding structure in what he observes at a given moment is adopting a concept of structure which, to me, is relatively meaningless and useless.

In Dr. Lévi-Strauss's paper, he uses "structure" and "social structure" in a number of different senses. . . . He uses the terms "model"; to him the social structure is a model rather than the reality; it is what you see persisting and repeating. He uses "model" in the sense of a descriptive model sometimes. . . . One goes out and observes a society and, through one's researches, builds up a model of that social system. This is not what one finds in any moment. This is what comes out of a great deal of observation.

Dr. Lévi-Strauss also points out that the people studied will themselves have a model of their own system, and this model will often be different from the model that the anthropologist constructs. We have an excellent example in Evans-Pritchard's work on Azande witchcraft and religion, in which Evans-Pritchard found it necessary to go below the conceptions which the Azande had, in order to construct a model which would explain Azande religion.

Another case would be that of Lloyd Warner in his work on American class structure. Americans traditionally deny the existence of a class structure. The scientist determines that Americans have a class structure, in that the behavior of Americans can be understood only in terms of a class structure. Thus one constructs a model that explains, to the maximum extent possible, the phenomena that one observes.

There are also models that one constructs in interpretation. There are historical models; when one attempts to understand culture changing over time, one builds a model which organizes the data of history, maintaining the data themselves, as Professor Kroeber has pointed out, and holding them together in a meaningful configuration. The scientist also constructs models in scientific theory, in which case he commonly rejects the phenomena after he has used them and preserves only the tested theory.

Dr. Lévi-Strauss makes a distinction between mechanical and statistical models, and here he touches upon something of the utmost importance in social science. He himself mentions that sociologists are primarily concerned with statistical models. . . .

A point of argument between sociologists and anthropologists is with respect to statistical and mechanical models. . . . I might give an example from Dr. Lévi-Strauss's paper, in which he takes our marriage regulations in a modern Western society and points out that the prohibitions regarding marriage conform to a mechanical model, that certain specific relatives are excluded, and we can always count on these incest taboos prevailing. But with respect to permitted marriages, there is an enormous range of possibility as compared with a society in which one must marry one's mother's linked-brother's daughter, or another woman who is a substitute therefor. In such a case, you would have a mechanical model fully explaining and accounting for the behavior.

To illustrate the distinction and the importance of this for sociology and anthropology: In our work on social organization in Truk, we found that the natives gave us the rule of residence in marriage as bilocal or ambilocal. . . . In considering all marriages, by a census of households and working through genealogies, we found that about 85 per cent of all marriages today and over the past are matrilocal and about 15 per cent patrilocal. There you have the statistical model. . . .

Our objective was to convert the statistical model into a mechanical model, so we studied the cases of patrilocal marriage, in order to find under precisely what conditions they occurred, and we were able to determine that patrilocal residence takes place only under very specific circumstances, namely, when there is not a large enough number of matrilineally related women to maintain a functioning matrilocal extended family, in which case the woman, on marrying, goes to live with her husband, his sister, mother, and so on. In other words, we were able to reduce statistical model to mechanical model.

Ordinarily, sociologists think that they have gone far enough when they have constructed statistical models. Anthropologists, I think, are more sophisticated than sociologists, in that their ideal is to convert statistical into mechanical models. This point has been specifically stated in Dr. Lévi-Strauss's paper.

EGGAN: Professor Murdock talked about structures moving through time. Is the concept of co-tradition related to this, for example?

WASHBURN: In relation to some models, I may say I do not view the interrelation of fields as one of taking up words and carrying them across. I think structure in anatomy is a very different thing from structure in social systems, because the structure gets back to different ways of defining structure.

But, taking up the models used in modern genetics—Sewall Wright's models, we will say, of how evolutionary systems work—one would have to have information on the mating system, the amount of inbreeding, classes, breeding isolates, population size. These would be what one would have to have from the student of society in order to apply Wright's statistical models, in order to know what was going on in these societies genetically.

Likewise, if the social anthropologist has an idea, say, about the amount of inbreeding, if a society is fairly closely inbred, this would throw off Dr. Boyd's gene frequencies, and therefore he would not be able to tell the social anthropologist whether the system had operated the way the social anthropologist thought it had. If a closed system of inbreeding is the ideal of a society, then this will change the gene frequencies in a perfectly predictable, definite way, provided that people did what they tell you they did. Now the chances are that they did not, but this now can be made a matter of observation and not hunch.

BOYD: It would not be the gene frequency, but only the phenotype frequencies.

MEAD: There is one other point about the use that Dr. Lévi-Strauss has made of the conception of models that I think is likely to confuse some American workers. . . . Professor Lévi-Strauss has specifically drawn on cybernetics in a different way from the way that a group of us who have been working closely with people at M.I.T. have been drawing on some of the same material. We are including . . . the model in the engineering sense, that is, the model that is built by a set of engineers to a set of specifications, whether those specifications are drawn from a whole ecological system, as, for instance, the model of Ashby, or whether it is a computation machine that is built on a set of specifications about human memory or human capacity to sort, translated into mechanical operations.

A whole group of people, some anthropologists and some not, have been influenced by these experiments . . . and the term "mechanical model" therefore is changing its general communication meaning.

In my paper I refer to living models as opposed to these machine models that are built according to a limited set of abstractions. . . . Professor Lévi-Strauss and Professor Murdock are talking about what I call a "living model," except that this living model that we are coming back to is actually really identified persons. So when we talk about a living model we want to build either an experimental situation . . . or look at a living community with specified human components, each one of which is really identified. . . .

Unless we can keep these three or four points clear, I think there will be ultimately some confusion. As I understand Professor Lévi-Strauss's mechanical model, as applied, say, to a social organization situation, it would not contain identified persons but would be a picture of a person who might be a man with two sisters, two brothers, two aunts, and so forth, but he need not be an actual one.

Going through these machine models is a way of learning. We want to keep our human components identified, so we can carry our unanalyzed variables as we proceed from one spot to another. . . . If you abstract from your known social system and build a mechanical model in that sense, you can only put in what you have been able to extract and you throw away the rest.

EGGAN: What substitutes do the archeologists have for this?

WILLEY: Well, in the depths of archeology, the term "model," as used by Dr. Lévi-Strauss and Dr. Murdock, is completely analogous to our use of the term "type," as opposed to the tangible reality of the artifacts.

EGGAN: What is the co-tradition?

BENNETT: The archeologist obviously deals with artifacts. . . . Any interpretation has to be an abstraction. It cannot become a living model, since there is no way of getting at the people themselves. I once thought of the parallel more in terms of Max Weber's ideal type, which is an abstraction based on considerable reality, and then attempting to apply that once again to the archeological interpretations.

KROEBER: I would like to say that the co-tradition is obviously a construct—in old-fashioned language, historical reconstruction or an interpretation of the data.

NADEL: Yesterday, I rashly said that I never used the word "model." I find that Professor Lévi-Strauss quoted me as having used "model," particularly in connection with a small-scale model of comparative balances. I was using it in a naïve sense, as you have a model railroad.

I have a brief list of four—and, perhaps, now, with the living model, five—different senses in which the word can be and has been used by Professor Lévi-Strauss. . . .

One kind of a model is a machine built to specifications on which you can study more easily, exemplify, and illustrate more easily rather complicated conditions. A variation of that would be a machine constructed according to specifications in a different discipline which you find illustrative and useful in your own discipline. I do not think that the difference between mechanical and statistical applies to that at all, because lots of statistical observation may go into your construction and specification of the machine.

A second way Professor Lévi-Strauss uses "model" is: model equals norms. When he speaks about cultures and unconscious models and people having a different appreciation of what he calls models of his own society, what he really means is that we observe a certain society, from which, through essentially statistical observations, we derive a certain norm of behavior in, say, marriage rules. The people have their own idea of what the marriage rules are, which they may quite often put down in absolute terms. But the observer may find that the things which are claimed never occur in fact.

A third meaning is . . . the ideal type. An example of that is found in Professor Lévi-Strauss's paper, where he says, "For instance, the model of, let us say, a patrilineal kinship system does not in itself show whether or not the system has always remained patrilineal, or has been preceded by a matrilineal form, or has by any number of shifts been preceded from patrilineal to matrilineal and conversely." This seems to me to be an exact counterpart of Max Weber's ideal type; that is to say, the analysis of a situation in which you find certain implications which, for the specific purpose of demonstration, you put into ideal form, leaving out certain variations. . . . Max Weber has produced German bureaucracy as an ideal type; you can always argue, of course, that it is not a true one; it is only an idealization.

Now there is my final, fourth point, where Professor Lévi-Strauss contrasts model with reality, and here, he says that structure is a model and not reality. I am not going to talk about reality; I am going to quote Morris Cohen, who says that discussions on reality belong in religion. So far as we are concerned, we have a phenomenal world from which we abstract to varying degree. Everything is reality or not reality, whichever way you look at it. It depends on the level of abstraction.

Social structure is as real or as unreal as anything else, but on a higher level of abstraction. A great many more variable phenomenal details are ignored or dropped out. In that case, I do not think it is very satisfactory to call the social structure a model. It is the society or culture, if you like, looked at from a particular point of view, ignoring a number of variables. Two cases come to my mind; the first is the concrete individuality of the persons we see, the Toms, Dicks and Harrys of any anthropological field. . . .

The second variable we drop out is the qualitative character of actions. Whenever we construct a relationship, say, love, submission, subordination, we ignore the concrete modes of behavior out of which we construct that position picture of somebody being submissive to another person or people standing in reciprocal or symmetrical or asymmetrical relationships. I fail to see the difference between relations and social structure which Professor Lévi-Strauss emphasized, in terms that social relations are the raw material and social structure is something that is not raw material. . . .

SCHAPIRO: It occurs to me that part of the difficulty arises from the fact that the notion of model has been extended from physics and chemistry to the human field. In physics and chemistry, the models first constructed, the important models of the nineteenth century, especially of a statistical kind, were constructed to explain things which were not visible; for example, the behavior of gases or, in chemistry, the structure of carbon rings. Physicists or chemists tried by imaginative effort and by a great deal of speculation and fitting of experimental data to construct an image of something that could not be verified directly by immediate vision, and so the notion of the model acquired a quality of abstraction and artistic construction which is of a quite different order from the kind of model that an archeologist or a historian or even a linguist sets up in order to describe things which are directly observable. These he has to justify by certain methods of testing which are different from the tests used in the chemical and physical field, where the mathematical formulation is important.

The problem, therefore, may be put in this way: In the social fields, there are processes which are hidden from us, which cannot be described adequately by simply putting down what you see before you. It is therefore necessary to construct a model of such a kind that we do not test the model by saying it has a one-to-one correspondence to what we see but, rather, that it permits us to deduce certain things which can then be verified.

What is deduced from the model to verify? In some cases the model constructed by chemists or biologists—for example, the model of chromosomes—has been confirmed by powerful microscopic methods, and such a confirmation is a wonderful triumph and an encouragement to go on with the construction of such models. But unless we keep clearly in mind that there are at least two types of model function, depending upon the kind of objects we wish to describe, whether they are overtly given objects or are hidden objects and processes, then we will, I think, constantly oppose one another because of the strangeness of the model described by Dr. Lévi-Strauss in one case and the model described by a person who wishes to deal with immediately given social situations.

BREW: I think this discussion has established that, from the prehistoric point of view, we are dealing with a model which is based on inference. In archeology we can never get anything but a small fraction of a culture, so the model we use and produce is an inferential model, outlined so clearly by Professor Schapiro.

BOYD: I think Dr. Nadel pointed out beautifully the different senses in which the word "model" is being used, and I think for each of them a rather simple English expression could be substituted. For his first meaning, I would simply use the word "analog"; for his second meaning, I would use the word "ideal.". . . The third I would call "pattern formulation"—

NADEL: That was Max Weber's ideal type.

BOYD: I would call that "pattern formulation"; and then the fourth I would call a "sketch" or an "outline." I submit these as simple English expressions in place of the word "model" used in four or five different senses.

GREENBERG: An important one, I think, has been left out by you and was mentioned by Professor Nadel, namely, all-or-some statements which are not quantitative, and those involving definite quantification, like "Sixty-two per cent of the marriages are cross-cousin marriages."

CASO: Can we use the word "archetype" for model in archeology, for instance, as an abstraction? Unfortunately, the object itself seems to have a tendency to be different from the ones the model has established. There are many things in the object that are different from the model. The model is only an abstraction. It is a kind of archetype.

MURDOCK: I think the word "statistical" has led to two confusions. In Dr. Nadel's remarks, he mentions statistical in the sense of the accumulation of data for the construction of a model. . . . That is a means of arriving at the model. In Dr. Lévi-Strauss's paper, I think he makes a confusion of the word "statistical" when he says that I try to construct mechanical models with the help of a statistical model. Well, I used statistics as a method of arriving at a mechanical model. It is not a statistical model because I used statistics. . . .

EGGAN: Isn't he thinking that your series of types is made up of a composite type rather than of individual types?

MURDOCK: Well, that is a question of fact.

LÉVI-STRAUSS: This interesting discussion about my paper is a fine experiment in cultural linguistics. . . . In three respects, at least, there are some agreements and confusion which I think arose exclusively from linguistic problems.

I have already mentioned the process question. . . . So I shall lay that aside and pass on to another confusion in respect to Professor Radcliffe-Brown's letter, that is, about the word "genetic." There has been a complete misunderstanding between us on the word "genetic," for he takes it in a historical sense while I was using it in a purely logical sense.

In the field of geometry, for instance, we make a distinction between two kinds of definition. If we want to define a circle, we may say that it is a pattern made up of points which are equally distant from another point which we call the center, and this is a very good definition. Nevertheless, it is not genetic because you cannot make a circle with the help of the definition. But if you define the circle as a pattern resulting from having a segment of a line revolve around one of its ends, this is a genetic definition because it tells you how to make a circle. But it is entirely different from a historical definition of the way any given circle has to come into existence. Therefore, when I argue against Radcliffe-Brown, that his interpretations of the Australian kinship system are not genetic, I am reproaching him, not for failing to bring up the history of the Australian kinship system, but for not explaining how they are made.

This is also the reason for the different approach between Professor Murdock and myself, when I mentioned that he built up a mechanical model out of statistical data, because his approach is extremely different from a truly geometrical approach. It would mean considerable, for instance, to say that the theorem of Thales or Archimedes is true because it is verified in 60 per cent of the cases. I think, therefore, geometry is using mechanical models, which are entirely different from statistical ones.

The third question is the relation between model and reality, and this is also mostly a linguistic problem, because in English it is difficult to distinguish between reality and concrete reality. I do not know how you could quite qualify it. In my mind, models are reality, and I would even say that they are the only reality. They are certainly not abstractions, as was suggested by Professor Nadel, but they do not correspond to the concrete reality of empirical observation. It is necessary, in order to reach the model which is the true reality, to transcend this concrete-appearing reality. . . . Of course, a model can be very close to concrete reality, or it can be very far from it. In his letter Professor Radcliffe-Brown takes a very nice sample, because a sea shell is an empirical reality which is very close to its model. Unfortunately, in the field of social science, we very rarely meet with this kind of concrete reality, which shows the model in a very apparent way. However, it seems to me that some approximation can be found in the field of linguistics. There are, in a vocabulary, certain categories of terms which are very close to the model; let us say kinship vocabulary or the terms for parts of the body or the terms for the color scheme. Here the model is, in some languages at least, quite apparent, but the fact that it is not apparent for all parts of the vocabulary and that it is necessary to go to a deeper level to reach it does not prove that it does not exist, and it does not prove that the model is an abstraction. It proves that the reality is more hidden in some cases than in other cases.

Now I was asked to explain what I call a "model." For me, a model is exactly what Professor Schapiro stated. The model is not the mathematical formula, and the model is not the result of direct observations. Perhaps the best thing to do would be for me to give a few examples. Even before succeeding in seeing chromosomes, the geneticists were already making maps of chromosomes and genes, and this was a model; and, although no physicist ever saw an atom, nevertheless he was able to build an image which did explain all the properties of the atom and could be verified.

Another example: It has been discovered quite recently that crystals have a spiral-like growth, and this can be seen with the electronic microscope, although no actual photograph of any given crystal shows a perfect spiral. All the spirals are incomplete, deformed, but, nevertheless, the spiral itself is the model which may help to explain all its properties.

The distinction between the living model and the engineering model is a practical distinction which can be of great use, but, nevertheless, I do not think it goes extremely far. For instance, I agree with Professor Nadel that a small railway is a model. If I see that for the study at hand I do not need cars which are painted or built exactly like actual cars, but I can just use plain wooden squares, then the model will be a simplified one. It will be quite satisfactory if it can explain all the facts I am trying to explain, and it will to some extent be an engineering model, a machine built to specifications.

When I was assigned this paper, I discovered much to my surprise that I had no idea whatsoever of what social structure was, and that I had written quite a deal on social structure without knowing what it was. . . .

I will try to show that it is at the same time more and less than is usually thought; less than usually thought because I insist upon the distinction between social structure and social relations. Social relations are what are truly observed. . . . But, on the other hand, social structure implies a problem of a very different nature, and I felt obliged to go very far toward demography, because it is impossible to study structure without studying numerical properties of groups and, on the other hand, religion.

I have noticed that religion was not a problem listed in the field of this symposium, and I am surprised that religion was not introduced, except in a negative way by Professor Nadel. But I do not think it is possible to understand social structure without taking into account the fact that there are structures which, instead of being related to another structure, opening new correlations, are really related to all the structures taken together and help to close the social structure. If we had not had orders of religion, the social order could extend indefinitely, and there would be new correlations arising, one after the other. It is only because there is some religious structure in human society that it is possible to close up the social structure.

KLUCKHOHN: We must not confuse reality with substance, and I take my favorite example from the philosophers. You have a brick wall. O.K. You take the bricks out, one by one. Materially, you have destroyed nothing, but a form is gone. You can take it further, of course. You can take each brick and pulverize it, and you have still got all of the matter, all of the reality in the sense of substance, that you had at the start, but only a damn fool would say you had not destroyed something.

I would like to link this to the question that Professor Schapiro asked; whether, in problems of culture, we have something analogous to the problems which physical scientists faced in the last century and still are facing, of creating inferential constructs which will help us to understand—understand in the sense of predict—what will happen, but which are drawn only indirectly from what is seen. Of course, to this question, I would answer, unequivocally, "Yes."

Henry Murray always says there are three orders of phenomena about people; one can say that there are some things an individual knows about himself and is prepared to tell you. Then there is a category of things that he knows about himself but which he is unwilling to tell you. Third, there is a series of propositions which are true, but which the individual cannot tell you, not because he is unwilling, but because he does not know them. And I think the same thing is true about cultures.

GREENBERG: All things that we talk about, obviously, are real; a statement about something is just as real as the thing is. A model of a shell is just as real as the shell. It is a metaphysical question which we think we do not have to go into. But there is an important distinction. . . . Somehow it is different; a shell is not the same thing as the model of a shell. What is it? I think we must make a distinction between things and discourse about things; and to discourse about things, we have to have symbols, so we must have this relation of reference of a symbol to the thing. I think that is probably all that is involved. It brings up, I think, an important concept of symbolism and symbols, which we have not mentioned at all.

KLUCKHOHN: Not just things and discourse about things, please. Things, discourse about things, and forms, i.e., arrangements of things.

GREENBERG: "Discourse" is a better name for it.

NADEL: Professor Kluckhohn has introduced an important concept . . . that is, the word "inference." I limited myself to discussing models and structures of a descriptive or illustrative kind, since that was what Professor Lévi-Strauss was doing.

Now it has been pointed out that models can also be used to reveal hidden mechanisms which are explanatory of an observed regularity or constellation of phenomena. Here we are dealing with inferences from an observed effect in order to explain that effect. I think these are two entirely different approaches. Admittedly, as Professor Lévi-Strauss explained, we do not know yet what social structure is, but one thing I think we can say negatively: It is not an explanatory top category. It has not to do with forces or hidden mechanisms, which we infer, and for which we then construct models. Structure is still a descriptive or illustrative or diagnostic model. I do not think it includes anything in the way of forces which have to be interpolated or inferred to account for existing constellations of facts.

MEAD: In addition to the point that Professor Schapiro made, I think we have to consider the use of models as a method of communication between the sciences. One of the most important things in Professor Lévi-Strauss's paper is the discussion of which forms of mathematics are suitable for the number of likely observations in the sort of phenomena that he was discussing where we have little runs or short runs.

Professor Wiener, in his first book on cybernetics, claimed we could not handle social science mathematically because of the length of our runs. One of the advantages, therefore, of borrowing from the models, in the sense that Professor Schapiro was describing them from physics, back and forth between the sciences, is that it permits us to see whether we could use the mathematics that another science has developed to work with a particular kind of model. . . .

LÉVI-STRAUSS: When I was trying to use mathematical methods for the study of kinship rules, I had great difficulties with mathematicians, who all told me it was impossible, because there was no mathematical way to describe marriage, in the same way as Professor Schapiro the other day was saying that there is no mathematical way to show what form in art is.

Then a mathematician came and said this was irrelevant, because he did not care about marriage, he was interested only in the relationship between the forms of marriage. From his point of view, there were only the relationships.

. . . This is very important and has been brought up by the new qualitative approach of mathematics in topology or group theory, which I think is applicable to problems in social structure.

EGGAN: I want to add one thing for consideration during the recess. When you have different degrees of abstraction, what does that do to the problem of deriving process? Does it make it more difficult or easier, or does it make it impossible?

SYNTHESIS

KROEBER: I must admit to a certain countersuggestability, especially where new words are concerned; and, if I suspect they are new words for familiar ideas, I become doubly countersuggestive.

Mr. Murdock agreed this morning with a stricture I once made about the word "structure.". . . The passage which Professor Lévi-Strauss cited did not really attack the idea of structure. It attacked the gratuitous dragging-in of the word "structure" by the hair because it was fashionable at the moment. I still feel that . . . to call a personality a personality structure adds nothing to clearness of thought and may confuse it.

I am making these remarks with the idea of finding certain common elements in all five papers. . . . With regard to Dr. Boyd's paper, we have a sample summation of what the new methods can do. The new methods are important and are going to be more important. But it seems to me that Dr. Boyd's classification into six races, when compared with extant classifications, gives us in the main the old races which were characterized by certain common qualities and also by certain geographical continuities. It is gratifying to see that, in the main, genetics validates those races. The one race of his which I am most dubious about, the Old European, seems the weakest in point of fact. It is an interesting suggestion, and it may be that the evidence for it will grow, but I think it is as yet a hypothetical construct with insufficient evidence—at least there is not so much evidence as for races which were discovered by observation and by anthropometry. Here is a case where new ideas and new tools have come in, but the value of the new tools has been to confirm what is sound in the old points of view. Whatever they do not confirm will go out, so I do not see that it is a matter for anybody, whether he is an old-fashioned anthropometrist or a very new-fashioned geneticist, to worry too much about the differences that exist.

Take, for instance, the matter of language; the metalinguistics that Dr. Hoijer examines at length, the thesis of Whorf and of others. . . . Basically, it seems to me Dr. Hoijer is sound in that he does not tell us just how sound Whorf's and Lee's approach is. He has committed himself to the approach because he participates in it, but he has participated in it about to the extent of more than a toe, but not much more than a foot, being dipped into the bath. In other words, the future, as in Dr. Boyd's Old European race, will bring ultimate decision.

Another point about language is in Dr. Kluckhohn's mentioning the phoneme and the morpheme as being universal constants with which we can operate in linguistics, and therefore voicing a hope for the same sort of thing turning up in regard to culture as distinct from language. I do not want to say that the phoneme is merely what we used to call a sound. It is definitely more refined, it is sharper, it is an infinitely better conceptual tool. But, basically, it is the sound, as the morpheme is the word or the affix, with sharpened definition and greater utility. Again, we do not have a break between the past and future . . . but a ɔntinuity of thought and development.

Now somewhat the same applies to ıodels. I may be mistaken, but it seems › me that the statistical model that rofessor Lévi-Strauss talked about is ot so very different from a certain ⁄pe of mathematical formulation. It is n equation, or can be put into an quation or at least something similar ıathematically. Put in that form, it ɔses all the glamour, of course, of beng as of 1952.

My past being greater than my fuure, I feel sympathetic toward the ʒood old solid accomplishments of the ɔast. Consequently, when, as is true of ɜvery culture, I find with regard to a new concept that it is 20 per cent improvement and 80 per cent retention of something previous, it gives me a certain comfort. It may do the same for some of you who are younger than I but who are also interested in the continuities, as well as in the growing point or the advancing edge of new conceptualization.

On one point I must differ with Clyde Kluckhohn, with whom I have spent much time arguing about universals. . . . It seems to me that the universal categories of culture are unquestionably there, but they are not culture. Dr. Kluckhohn says that himself. He does not say it as flatly as I, but in his paper he speaks of biological and psychological and social constants. Now these have to be recognized. All culture rests on them. Any anthropologist who attempts to operate in any large way with culture without recognizing this underlying fact is bound to run into sterility and ultimately into nonsense. But it is also important to recognize that things which underlie culture are not the same as culture. My own feeling is that these constants exist, but they exist essentially on the subcultural level and that is why they are constant.

A moment of reflection about the relation of physical chemistry as against biology will show the same thing. There are constants of a sort in life, but they are not so constant as the physiocochemical constants which underlie life. While I am not saying that there are no constants or high frequencies strictly on the cultural level, I think these are recurrent things that come up. Dr. Kluckhohn is mostly talking about the ones that are on the other level.

One more point. I think it is in Dr. Steward's paper. He states that anthropologists have been too much interested in the diversities, and that is why they have not found recurrent fundamentals. I do not think anthropologists have been unduly interested in the diversities.

When we study a tribe and bring out its pecularities, we are bringing out the physiognomy, the characteristic quality, the value system of this tribe. We have to bring this out, and it inevitably has a certain degree of uniqueness, and therefore it is a diversity. If we did not look for those things, our labor would be wasted.

When we get the value system of one group, the question comes up how it agrees or disagrees with the value systems of the adjacent groups, and then with the others of the continent, and then finally with the value system of the whole world. This can be compared to the situation in biology, where the man who is finding or isolating a new species, or a new fossil, has to bring out what is characteristically unique, but at the same time, of course, he also brings out—and there is less noise made about this—in what respect and how far the new form relates to other forms and to which ones. Sound procedure in cultural anthropology has been interested in the common relations of the form, as much as in the distinctivenesses of form. One without the other is essentially meaningless and self-defeating.

LINTON: My comments are limited

to this matter of universals. I agree with Dr. Kroeber in the distinction between a culture pattern and the drive in psychological terms for the individual, or the social inheritance, the necessities implicit in social living. . . .

The difficulty in finding universals at the level of cultural behavior stems very largely from the fact that every culture pattern subtends several different needs simultaneously; it has a complex of functions. This particular complex will never be identical for any two societies. Any semanticist will recognize that there are no identities anywhere in the universe. Nevertheless, in many cases, particular culture patterns in different societies are much alike—not identical, but similar at many points. On this basis, it is possible to establish certain categories within culture which are universal.

As far as I know, there is no culture which does not have some form of aesthetics. There is supposed to be only one culture that does not have something which might be termed warfare: that of the Chatham Islanders, and that was because there was only one tribe on the island and they fought among themselves, but it could not be called warfare. You do have certain basic categories, then, which may be regarded as universal on this basis of similarity and not of identity.

WHITE: It seems to me that in the papers that have been presented and in the discussion, there is some opposition of motive. On the one hand, we have heard it said repeatedly that we are looking for broad generalizations, broad categories. . . . In the field of cultural anthropology, some of the discussion has made it rather clear that there is a feeling that such broad, general, or universal categories as might be formulated are so general as to be useless. If we cannot find universal categories within cultural anthropology that have any significance other than being obvious and commonplace, how, then, can we expect to find or to formulate concepts and categories that will embrace all fields of anthropology, where the diversity is much greater than it is within the field of cultural anthropology itself?

It seems to me we have to make up our minds whether some of these broad generalizations are worth reaching and do have a significance. Some that now seem obvious, such as a body at rest remains at rest—which, incidentally, took a very long time to formulate—are not wholly without significance. . . . It does not seem to me that we can take this ambivalent attitude of wanting broad categories, yet saying they are so obvious as to be worthless. It seems to me that we have to cast our lot one way or another.

REDFIELD: As Dr. Kroeber has referred to his strictures on structure, I shall not meddle with this muddle of models, but comment at once on Professor Kluckhohn's paper.

Dr. Kluckhohn laments the culture-bound character of many of the categories which we use and compares the state of linguistics with regard to the phoneme and morpheme in this regard. Professor Kroeber's remarks have also suggested a point I would like to emphasize, namely, that our categories become less culture-bound as we go on working and invent new terms and try them out. It is not so much a matter of discovering something comparable to phonemes and morphemes (although this seems to me not entirely impossible in the field of mythology, for example) as it is that we purge the terms that we use so that we become less culture-bound. In the field of familial institutions, there is a long tradition in this regard from the original treatment of matriarchs and patriarchs.

We do this also by beginning with phenomena far removed from the subcultural phenomena which Professor

Kluckhohn has emphasized. We will, of course, improve the general understanding we have of the biological and psychological and societal conditions for all cultures and be able to state them more precisely with regard to the universals and the specialties, but I think we will also make progress by beginning at the top end.

Professor Kluckhohn's reference to comparative value studies presented good possibilities. There, too, we are still culture-bound. . . .

As we come to make these propositions with regard to culture convincing, we find that many of them are propositions about human beings rather than propositions about culture. It is true, we do say some things about culture which seem to stick. If we say that all cultures are characterized by technology and morality, we say something. If we go farther and say that, in all these moralities or value systems, the people close to one are preferred to the people less close to one, and it is better to do less murder on our neighbors than on more remote people, you say something which is true and meaningful, and which has content. But, on the whole, one tends to find one's self saying things about the way people think and feel and act rather than about a system or an institution, and it may turn out that there will be a considerable content of universal or near-universal which will take the form of statements about what people are like.

This would not be terribly surprising, because that is the way common sense goes about it, and part of science, it seems to me, is not only the collection of data but also the confirmation of common sense. If you took an ordinary human being and put him in some remote part of the world, no matter how uneducated he was and how unprepared for the phenomenon of culture and the wholesome implications of the philosophy of cultural relativism and all the rest of it, if he survived at all and was not put into a pot and stewed, he would get along with these people on the basis of certain assumptions that he would make to the effect that these people were, on the whole, like him. "Well, this is in some ways the way I do," and "This fellow seems to be angry; I suppose he is angry," and "This young person seems to be embarrassed about something; I don't know what the embarrassment is yet, but it is embarrassment." And he would get along.

But this kind of common-sense hunch is, perhaps, reducible to more formal and scientific propositions. This is the line on which many students who are classified as social psychologists work. When Professor Hallowell takes an interest in the phenomenon he calls "self-awareness," I believe he is re-cultivating some territory which students of behavior, social psychologists, have been interested in before. One says that all people are aware of themselves. Self-consciousness has certain important consequences which one states fairly formally, and relates to classes of situations. In this way, one may build up generalizations which are not quite cultural in the sense in which Wissler's categories were, but are probably useful, too. There are many roads to the understanding of the resemblances or the differences between groups of people and among men themselves, and we will probably find ourselves going along a number of them. They may or may not lead into one another. My last remark is a much more general statement, a confession of viewpoint with regard to method in social science: you get along as well as you can with the combination between what you might call understanding, on the one hand, and science (understood strictly), on the other. We understood a lot about Americans by reading Tocqueville, and it takes a lot of science to amount to that much understanding of Americans, in my opinion. This is a field in which our business is to make the science correct and support the understanding. But I do not think it is a field in which we can expect perfect success. I think a great deal of our understanding here will not be scientific, and the comparisons therefore will not be, in Professor Kluckhohn's term, genuine. We improve it somewhat, but my own feeling is that it could go on into infinity, somewhat increasing the area of science but also increasing this other thing which is very useful, "understanding."

FORDE: Professor Kluckhohn's paper has the title, "Universal Categories of Culture," but, in fact, the universals dealt with at the end are determinants of culture, not categories of culture. We must make a distinction between the universals and categories in cultural description. Terms like Tylor's "artifact," "idea," "institution," "custom," and "morals" are universally applicable to the pattern behavior of human beings.

We must keep the notion of universal determinants, that is to say, factors which are variable and which are operative in every cultural activity and underlie it, and which are, as Dr. Kroeber put it, pre-subcultural. We must distinguish these from the categories of the cultural phenomena themselves. We can discuss one or the other; we can discuss both seriatim; we can discuss the way in which variations in the determinants will give you variations in the content of the particular categories which are brought under discussion. But to confuse the categories and the determinants seems to me to make it impossible for us to talk about them intelligently.

STEWARD: Possibly two separable propositions are involved here. Perhaps there is a unit between them. One proposition would be along the line of what Dr. Redfield has just said, a search for universal properties of human beings, which could be biological and psychological and so forth and which are potentially cultural, but only potentially. That is, they would be manifest or not, in particular concrete cultural behavior, only under certain circumstances.

The other proposition would be that these are indeed categories of culture, but then that raises this point: As categories, it would seem to me they are like any taxonomic scheme, whether it is developmental or culture-area or something else. This point has been argued, but I think that the purpose of the categories would depend on the problem. In that case, you could put, as the problem, culture change, culture process, rather than descriptive comparison of concrete manifestations of culture.

There we come to this matter of abstraction; for instance, you could have a category of subsistence activities, which would probably be universal. I am not sure it would be adequate, because now there are . . . cash commodities that might take a wider category. Let us limit it, say, to agriculture. There is a category, and the problem then might be agriculture in relation to social structure. That would require a very high level of generalization or abstraction. You could make it less abstract and more concrete and more limited in cross-cultural occurrence if you said irrigation agriculture, and so on down to particular societies, where you had only one which was relativistically unique.

If you are looking for actual process in connection with the concrete manifestations of culture, I would say the problem here is what we discussed yesterday in connection with evolution —How far can you generalize abstractly and cover all mankind? I am reminded in this connection of a comment Ruth Benedict, who was suspicious of parallels, used to make. When somebody would suggest a parallel, she would smile and say, "Yes, all mankind has culture, hasn't it?" Beyond the particular manifestations, she was unwilling to say, "Here are genuine parallels." To do that, you have to abstract, and then you come to the question of taxonomy in relation to problem.

EGGAN: If you take Professor Lowie's conception of culture as at all times and all places, then maybe you can compare these segments within it without worrying about being culture-bound.

MARTINET: The only way we can proceed, in considering linguistic categories, is inductively in trying to consider all languages, trying to determine all categories which are found in the languages we know. But we are not too sure we would know or be able to know and describe the categories in all languages spoken at this time in the world, and, of course, there have been languages for millennia and there will be languages, and we cannot foresee what the categories would be. So the whole thing boils down to stating that the only universal categories of language are the categories which are included in our definition of language and nothing else.

FORDE: The very notion of a concept of culture implies the categories.

KLUCKHOHN: Certainly, it is true that we must not confuse determinants and categories. I am at fault for not making plain that I have talked at such length about the determinants merely to suggest that, if there are these universal determinants, there must be corresponding categories. . . . I agree with Redfield when he suggests that this ought to move from both ends, but I was a little astonished to hear him say, like a British social anthropologist, something to the effect that "I don't like this; there is too much about human beings in it and not enough about institutions." If he meant by "institutions" what I took him to mean, culture of course deals with much more than institutions. Culture begins from human beings, from what they say, from what they are observed to do, from study of the artifacts they have made, etc., and it always goes back to human beings.

Of course, whole cultures, by definition, as totalities are in some sense unique, or they would not be cultures. But I am unhappy at a tendency I observe sometimes, even in Professor Kroeber, to identify culture with the unique elements only. The unique elements are there, and if you take the total system, it is, of course, unique as a system just as each personality is unique as a totality. But this is like certain experiences I have had with clinical psychologists and psychiatrists, who are strongly motivated to factor out of the personality everything which is similar to other personalities. They finally say, "But that is not this man's personality, that is part of his culture." But in the total functioning of this individual organization of experience that we call "personality," this is certainly a part, and every science must be adequate to explain both the similarities and the differences in the body of phenomena with which it deals.

And so I say, as far as culture is concerned, what Dr. Kidder long ago called the "likenesses"—rather than the identities, of which admittedly, there seem to be few—are also, even if they arise from subcultural underpinnings, cultural . . . and a part of the totality of learned, socially transmitted behavior.

As Boas used to point out, in all languages there are pronouns that correspond, roughly, to I, you, and he. . . . Surely they are part of each culture in which they exist, in spite of the fact that they are found in all cultures. So if we consider a culture as an organized system of meanings, those portions of the system which are found in other cultures do not cease to be part of culture. . . .

LEWIS: It seems to me Dr. Kluckhohn has misunderstood the point Professor Redfield made. I thought his emphasis was in the other direction, namely, that he saw too much of the institutional in the cultural analysis and was trying to get a little more emphasis upon human beings as human beings, not specifically within the context of institutions.

REDFIELD: Dr. Lewis's statement corresponds with my intention.

BEALS: It seems that some of the difficulties in seeing universals come precisely from certain immaturities in our position. We give attention to the specific, or, to use a biological analogy, to . . . species identification and fail to see that underlying the great variety of forms there are certain processes and certain types of interrelationships which provide us with uniformities.

These are, however, uniformities that lie in the field of process and interrelationship. Developing these processes, these interrelationships, making the kind of abstractions that we are only beginning to make, will produce the kind of broad generalizations, broad universals, that will have the deeper meaning that I gather Dr. Kluckhohn would like to have us get.

ROWE: Several remarks . . . have related to the relationship between similarities and generalizations; I want to comment on the relation between differences and generalizations.

The generalizations that we try for part of the time, anyway, are put forward in the form of hypotheses, and the importance of cultural differences in relation to a generalizing hypothesis about cultures is that the differences serve to test the hypothesis. They indicate the cases that the hypothesis does not cover, does not apply to, or that have to be treated as exceptions to it, and I do not think that we can overemphasize either the similarities or the differences if what we are trying to do is set up generalizations. We have to take both of them into account all the time.

NADEL: I quote from Professor Kluckhohn's paper: "The implicit assumption is that our categories are 'given' by nature." I do not know whoever thought that categories such as economics or politics are given by nature. If they are given anywhere, they are given in the organization of our mind and our way of looking at things and that, surely, does not stand still. I remember, when I was a student, one used to talk about political economy. Today, one talks about economics. . . . This is our own reaction to what we think we see.