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An Appraisal *of* Anthropology Today

International Symposium on Anthropology

Wenner-Gren Foundation for Anthropological Research

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C. F. VOEGELIN
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HANS WEINERT
LESLIE A. WHITE
GORDON R. WILLEY

An Appraisal *of* Anthropology Today

Edited by

SOL TAX

LOREN C. EISELEY

IRVING ROUSE

CARL F. VOEGELIN

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Preface

THIS book recounts the climax of a drama, and appropriately it is in dramatic form. The first act (in late 1951) was the planning of a great international symposium to assess the state of anthropology—and the selection of topics to serve as a framework and of the people to fill it in. The second act was the writing, the reproduction, and the distribution of fifty papers designed to "inventory" our knowledge of anthropology. This was completed in May of 1952. The last act was the gathering in New York during the two weeks from June 9 to June 20 of some eighty scholars who discussed these papers, exchanged views on the various problems they posed, appraised the state of the science, and looked toward its future. All this was the International Symposium on Anthropology of the Wenner-Gren Foundation. The volume published as *Anthropology Today: An Encyclopedic Inventory* is the accomplishment of Act II in its permanent form. The present volume fixes for posterity the very dynamic third act.

Since words in titles are capitalized, ours may be read either as an appraisal of anthropology today, or as an appraisal of *Anthropology Today*. The ambiguity is apt, for the book is some of both and not quite either. It is a report of what happened when, after reading the inventory papers, the assembled scholars began to talk. Nobody wanted or expected reviews of all the papers; many wanted to say something about many of the papers; and everybody hoped the papers would be a springboard for talk about the future of anthropology. So the discussion reported in this book appraises some of *Anthropology Today* and forecasts much of the work to be done in the still dim future.

In his Introduction at the first session, Professor Kroeber recounted the history of the symposium and the manner in which the discussions were arranged and organized. This now becomes chapter i. There followed then eighteen three-hour discussion sessions, each organized around a general topic. These are reported in chapters ii through xix. The last session was Kroeber's, to summarize fifty-four hours of very lively conversation. What he said becomes, with his revisions, our chapter xx. That was the symposium, and this is the book.

We four editors were "in on" the symposium from the beginning. A good book would require good discussion; and everybody wanted a good book. So the planning group appointed us early, and we immediately began correspondence and meetings with the chairmen who were organizing the discussions. During the two weeks of the symposium itself we breakfasted or lunched with one or another of them each day. Moreover, during the discussions we were seated conspicuously together, with the eyes of all thus fixed on the book into which they were speaking their wisdom.

Not all of the symposium is reported

here—only the more serious moments. All the participants will remember with pleasure the theater, the baseball game, the boat trip around Manhattan, the Sunday visit to the Philadelphia University Museum, the dinner at the New York Academy of Sciences and that given by the Foundation as a farewell party, all *sans* the tape recorder.

At the end we editors found ourselves with some nineteen hundred pages of discussion, including some written addenda to spoken remarks. The transcript was surprisingly good. A stenotypist worked faithfully at every session, and electronic equipment, expertly guided by Miss Lolita Binns, had recorded every word on tape and wire, so that the typist could go back where there were difficulties. Then William Thomas, the assistant director of research of the Foundation, spent long evening hours correcting mimeographed stencils of the transcription.

Marvelously, this was all done while we waited, so to speak, and mimeographed copies of all that was said were given to the editors within the fortnight that followed. We now faced the problem of reducing the record to make of it a convenient, economical, and readable book. Our goal was a 50 per cent reduction. Our deadline at the University of Chicago Press was September 15. We four were separated—Voegelin at Indiana University, Eiseley at Harvard, Rouse at Yale, and Tax at the Foundation's home in New York. The transcript was 8½ inches thick; it weighed 18 pounds. There were about 13,000 cc. all told. But the problem did not end with volume.

The reader may never have seen a transcript of free discussion and may not realize how many words most of us use. Not even the most literate of us always speaks in neatly packaged sentences. Moreover, what we say is expressive partly because of the intonations we use, the pauses and emphases, the gestures, and the very atmosphere and context of discussion. Seen in cold type, a most effective speech may be dead, meaningless, or (even worse to the man who said it) it may look like a string of platitudes.

It was our job, therefore, to bring the transcript back to the life from which it came, to see that every meaning reaches the reader as it was intended for a listener.

The content of the discussions at the symposium was for the most part significant and important for others to see. It was our task to preserve all of this. But there was also a good deal of talk which functioned to sew together the pieces of the discussion. We could not have carried on without it. But, like a good seamstress, we remove the basting before we deliver the gown. The reader will find here only the intellectual content and none of the "business" of the discussion. This is too bad in some ways, for not only is much amusing byplay missing, but the brilliant continuity supplied by some chairmen is lost; for example, Professor Brew, who spoke to such good effect as chairman of Session II, appears in our chapter ii to have said nary a word!

It is a tribute to the symposium that the discussions flowed so well that we could insert subtitles along the way without having to disturb very much the order in which remarks were actually made. But in some cases speakers were able to get in their comments only after the discussion had passed them by—and we have here made appropriate adjustments.

Although geographically separated, it was not difficult for the editors to agree by correspondence on common criteria, rules, and matters of style, particularly since we are not compulsive about doing exactly the same thing. (For examples, Rouse and Eiseley document the speakers' assertions more than Tax and Voegelin; Rouse sub-

divides his material into smaller sections than Tax; and so on.) In essentials we conformed.

In the allocation of parts of the transcript among us, and collation and arbitration of decisions, we enjoyed the good offices of Professor Kroeber, who, as president of the symposium, took the responsibility of keeping the peace among us. This he did, although he was in California and had other plans for the summer. So, by airmail and telephone, through him we co-ordinated our efforts, and between the second week of July and the last of August turned transcript into book as follows:

Chapter	Eiseley	Rouse	Tax	Voegelin
II. . ..		All		
III.....	21–34	34–43		
IV.. .		44–55	61–66	55–61
V..	83 84	67–83		
VI. .			85–99;102–3	99–102
VII				All
VIII			All	
IX.	142 51		151–59	
X .	160 70		170–77	
XI.. ..			All	
XII. .	191–202			202–17
XIII ..			All	
XIV .		All		
XV. .	All			
XVI.				All
XVII.				All
XVIII.			All	
XIX .			All	

Kroeber graciously took responsibility for his own sessions, now chapters i and xx.

The editors delivered their products to Professor Kroeber, who turned them over (with occasional comments) to Tax, who then undertook to put together the pieces, prepare the manuscript for press, and see that the general Index (co-operatively prepared) was consistent. It is only because he had this final responsibility (and because he edited a double portion) that his name is signed first below. Tax also wrote this Preface.

Anna Pikelis "joined" the symposium even before we did and became a precious resource to all who worked in it. The editors are particularly grateful to her because she took from our shoulders entirely the burden of *Anthropology Today*. On this one she did much of that tremendous job that has to be done when authors "finish" their books.

In the circumstances of the case it seems a little ridiculous to make a polite acknowledgment to the Wenner-Gren Foundation—for to the Foundation, including its board and officers, its director of research, and all its staff, this book owes whatever it is that a proud and hopeful new-hatched chick owes to the hen that laid the egg.

Sol Tax
Loren C. Eiseley
Irving Rouse
Carl F. Voegelin

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Address of Welcome

Dr. Axel L. Wenner-Gren, founder of The Viking Fund, Incorporated, was introduced to the International Symposium on Anthropology by Mr. Richard C. Hunt, President of the Wenner-Gren Foundation for Anthropological Research, Incorporated, and spoke as follows:

LADIES AND GENTLEMEN: It is with pride and expectation that I bid you welcome to this International Symposium on Anthropology. I am especially pleased to greet those of you who have come so far to be with us on this occasion—from Australia, Japan, Thailand, India, Turkey, Argentina, and many other countries.

This gathering of scholars from all points of the world truly represents to me the culmination of some of the great hopes I held when the Viking Fund was established eleven years ago. For I had then in mind an organization that would in a constructive manner support research in all aspects of anthropology. I trust that you will have found that the Foundation has done its utmost to realize these intentions.

To you, Dr. Kroeber, I extend my heartfelt thanks for your acceptance of the presidency of the symposium; I am confident that you and your colleagues will make this assembly a memorable event in your profession.

Anthropology, "the science or the study of man," is not, as some people think, something abstract and apart from everyday life. To me and to many others anthropology is perhaps the most fundamental of sciences, one which might give a new meaning to life and be instrumental in preventing humanity from committing suicide.

Anthropology not only teaches us more and more about man and his origins—it can also indicate where he is heading and what will be his destiny, if he does not use his knowledge and experience with logic and wisdom.

Quoting Dr. Kroeber, the dean of anthropology: "The human body has stood still in its evolution at least since the time when the glaciers began to recede. So far as we can tell, the evolution of the human brain was also completed by that time—20,000 to 25,000 years ago. But while the body stood still the mind went on, especially the *cumulative products* of the human mind. This ever more active accumulation by human beings is what we call culture."

This culture of our certainly is a telling example of evolution, but of a very uneven evolution. Self-preservation, which has been necessary for the preservation of life since that "single cell" of the far distant past, has now grown to such proportions that it threatens the humanity it was designed to preserve. If annihilation is to be prevented, man's more subtle qualities of mind, which express themselves in scientific inquiries such as yours, must be encour-

aged, developed, and made more widespread.

If these inquiries are to contribute to the solution of the problems of humanity, man's insight into morals and ethics must not stand still after the manner in which his body does. As Plato proclaimed: "It is not the life of knowledge, even if it concluded all the sciences, that creates happiness and wellbeing, but a single branch of knowledge—the knowledge of good and evil. Without this knowledge, the use and excellence of these sciences will be found to have failed us." The knowledge of good and evil is still a very controversial subject, and, as to the necessity of wisdom in governing human affairs, of which Plato also spoke, the present situation in the world offers a frightening example.

But the facts and scientific mentality, the objective truth, and the moral enlightenment concerning the cultural evolution of human beings, which are increasingly being provided by anthropology, are bound to have a positive, if gradual, bearing upon the general trend of thought and should contribute to a future for mankind, more worthy of the mental evolution of which we doubtless are capable. That you and the other members of your profession are doing your part to bring about a better order of things can be seen from the program of this assembly. Among the papers to be discussed, there are four on "Applied Anthropology in Government" by Kennard and Macgregor, Forde, Held, and Métraux; two on "values" by Northrop and Bidney; others, such as "Social Structure" by Lévi-Strauss, "Applied Anthropology in Industry" by Chapple, "Relations of Anthropology to the Social Sciences and to the Humanities" by Redfield, etc. These and other papers bespeak a healthy relation between theory and practice.

This tendency is indeed gratifying, and, while there can be no doubt that scientific study and research must be entirely free, I can see no reason why scientific knowledge and discoveries should not be made available to assist in the solution of the great problems of our time—above all, how we can best further understanding and peace among men.

Again, the Wenner-Gren Foundation for Anthropological Research is extremely pleased that all of you have honored us by your acceptances of its invitations. To you, as President of the International Symposium on Anthropology, Dr. Kroeber, I wish every success for this unique undertaking, the responsibility for which now lies in the most capable hands of you and your colleagues assembled.

CHAPTER I

Introduction

By A. L. KROEBER

THE INTERNATIONAL Symposium on Anthropology is the brain child of the Wenner-Gren Foundation's Director of Research, Paul Fejos. Early last autumn he suggested to me a symposium which would inventory and formulate the most significant findings achieved by anthropology, especially during recent years, and which would then proceed to consider the new problems of inquiry and methods of research arising from the achieved results.

We agreed that such a gathering would be most fruitful if it were, first of all, international in scope of representation and, second, total also in its envisagement of the field of anthropology. To insure that the approach would be both complete and balanced, of maximum coherence, without serious gaps or underweighting, it seemed necessary that the program be carefully planned, with all participants accepting individual responsibility for allocated segments of the entire field, though also with a constant eye on the relation of their segment to the whole —its meaning for the totality.

The planning of such a program was not to be lightly undertaken. It called for minds not only of competence but equipped with a variety of experiences, skills, attitudes, and imaginations on the part of the planners. Considerations of time seemed to limit choice, at this preliminary stage of planning, to residents of the United States. We agreed on Bennett, Hoijer, Kluckhohn, Mandelbaum, Strong, and Washburn. These possessed the necessary proved distinction, were, among them, identified with all the larger fields of our subject, and represented not only major institutions of learning but equitable geographic spread within the United States.

Just six months ago, this planning group met with Paul Fejos, Assistant Director Thomas, and myself, for two days. Jointly, we selected topics to be covered and individuals that seemed best qualified to treat them. These considerations involved complex balancing, and often decisions of delicacy; but we reached unanimity on all points.

From the outset, it was agreed that no papers were to be read aloud. For one thing, a comprehensive review of any subsegment of anthropology, a review of what had been ascertained in it and what problems it faced, would necessarily be of a length, in most cases, requiring an hour or two to present orally; and fifty such papers would eat up the effective span of the Symposium, leaving scarcely minutes for nominal comment, with productive discussion wholly excluded. The papers therefore were to be prepared not only in advance of the meeting, but well in advance; and Dr. Fejos undertook—and in the main accomplished—the task of having them suitably manifolded and distributed in time for us all to read them before leaving our

homes to come here. The gentle pressure needed to accomplish this—some few times—may now and then have seemed almost paternal to confirmed individualists, as most of us are and ought to be. I tried to assume what seemed a due share of the onus of such reminders. The group co-operated graciously and with unanimity. As a result, we now have before us—so to speak, under our intellectual belts—all the basic material, the inventory of knowledge, which we are to consider for its meaning during our gathering. The planning group thanks the rest of you for your helpfulness and patience.

Another consequence of this timing will be promptness of publication. The volume—or perhaps two—of inventory papers will be issued by early winter; that of discussion and synthesis soon after.

Considerations of calendar, cost, and so forth prevented full realization of the original intent to have the number of participants from Latin America, Australasia, Asia, and Europe wholly equal to that from the United States. Nevertheless, the proportion fortunately remains nearly in balance; and each session will be presided over jointly by a pair of chairmen—one from abroad, the other American.

To the persuasive eloquence of Kluckhohn is due the adoption by the planning group of a novel plan or organization of the program. This rests on the conception of there being three approaches in any field of science. One of these approaches or methods adheres as long as possible to phenomena as they are given by observation, preserving their phenomenal coherence, while organizing them descriptively, spatially, and sequentially, and interpreting them by classification and in pattern relationship. The second approach aims to resolve phenomena as quickly as possible into process, to ascertain their causality: for example, to convert phenotypic situations into genotypic ones. There is no essential conflict between this and the preceding approach: they differ rather as stages within one larger method. Knowledge necessarily begins by the first approach and tends to pass into the second; although in new or resistive fields the second way often may be delayed or remain relatively undeveloped. In default of commonly accepted terms for this distinction, the common-sense, informal designations "historical" and "processual" were adopted by us. Thus the first part of our program, from session No. 2 to session No. 5, is listed as devoted to "Problems of the Historical Approach." A second group of sessions, from No. 6 to No. 9, similarly is given over to "Problems of Process."

These two clusters of problems together constitute the field of anthropology as a fundamental or "pure" science. To these must be added applied or practical anthropology, which, with technological aids, occupied sessions 10 to 12. It is for these three groups of topics that the fifty inventory papers primarily serve as direct foundation. And, finally, our sessions Nos. 13 to 19 review some problems specially characteristic of the principal fields and themes of anthropology.

The result of this mainly tripartite organization of the program is that, for instance, linguistics will not be treated as a half-separatist bloc or subunit, dealt with all by itself on its day and thereafter ignored by the assembly. Instead, the problems of linguistics, so far as they are historical and have been summarized by Greenberg, will be discussed in context with evolutionary, cultural, and stylistic problems on our second day. So far as they deal outright with process, as in Lounsbury's, Hoijer's, and Martinet's inventory papers, they will be discussed on the third and fourth days, again in a context of similar nonlinguistic problems; whereas

Haas's review of the practical applications of linguistic studies is to receive its primary discussion on the sixth morning.

Analogously, the several aspects of biological anthropology are due to be considered, in relation to corresponding aspects of other fields, respectively in our third and fifth sessions (historical problems) seventh and ninth sessions (problems of process), and tenth session (applications); in addition to which, biological anthropology will again be considered—as such and as a field in its own right—in session 15; and, once more, for what it may contribute on the themes of patterning and behavior, in session 17.

Is this scrambling? Yes; but scrambling according to preconceived plan, with a purpose. The basic organization of our program, in short, is not by subject matter, as is convenient, perhaps even necessary, in everyday routine procedure and situations. Our organization is by problems which transcend subject matter, problems appearing in related form, even in parallel form, in all subjects and fields concerned with man. For a special occasion, for a systematic gathering of minds like this one, it is these deep-seated and recurrent problems which we have thought best to put into the forefront of the structure of our thinking. It is consideration of problems such as these, where the findings of biology and linguistics alike fertilize the study of culture, where understanding of culture in turn widens understanding of psychology and both stimulate that of society—it is such interconnected considerations that will give anthropology its widest views, its broadest significance; will help it maintain and consolidate its unity. A synthesis in depth is not lightly to be achieved; but it is worth aiming at; and it is the purpose for which we are associated here.

The Wenner-Gren Foundation occupies a special position among foundations, probably an essentially unique position, in that its field of endeavor coincides with a single department of science, and with that only. What the foundation aims to accomplish is also what the profession of anthropology aims to accomplish: the better understanding of mankind. The profession and the foundation thus share a joint responsibility. The increasing of knowledge is the job of the profession. It is we anthropologists who must execute that task. It is the foundation's self-assigned undertaking to promote that same execution—to promote it with wisdom, effectiveness, and especially with that broader perspective which tends to be more easily attained behind the firing line than on it. The profession of anthropology and the Wenner-Gren Foundation for Anthropological Research are thus coterminous in their aims and interdependent, to a quite unusual degree. There is probably no other instance of a profession being thus supplemented, almost one-to-one, by a foundation, or of a foundation centered so largely on the cardinal research activity of one recognized scientific profession. The very existence of this foundation is therefore a landmark, a new and important experiment in the public and intellectual currents of Western civilization.

There are great potentialities in the experiment; and it will be watched with interest in wide quarters concerned with the increase of understanding and the furtherance of science. Especially will this be true in a time like the present and near future, when governmental participation in science and subsidy to it, independent of that by endowed foundations, will be complicating the picture.

In general, day by day and year by year, foundation activity must largely consist of reactions to requests from individual scientists, or from small *ad*

hoc groupings of scientists. The initiative in such cases comes from the scientists; the foundation adjudges. One virtue in the functioning of the Wenner-Gren Foundation has been the fact that the relatively small size of the anthropological profession has made it possible for Director of Research Fejos increasingly to know its members face to face. This has been a boon and has kept the injection of bureaucracy and institutional filterings to a minimum.

At intervals, however, it is desirable for initiative to pass to the foundation, in some larger enterprise like the present one, where the scope of the undertaking is too broad for individual initiatives to coalesce readily into a coherent whole. Thus it is that our gathering today is the product of the vision of the foundation's director of research. At the same time, the anthropological profession has justified his faith and its own capacities for constructive co-operation by the team work manifested first by the planning group, then by the meeting of chairmen and editors, now by the international gathering here assembled.

That, Dr. Wenner-Gren, is what you have let yourself in for: a unique research foundation, uniquely conceived and conducted, administered as a trust for the progress of a science, in continuing intellectual and moral co-operation with the members of that science, in this country and abroad—eighty of whose representatives you see before you. We appreciate what you have founded and thank you, and we hope you too are satisfied with the results of your doing.

CHAPTER II

Problems of the Historical Approach: Methods

Chairmen: J. O. Brew and Kenneth P. Oakley.

Papers discussed:[1] Heizer, "Long-Range Dating in Archeology";* Movius, "Old World Prehistory: Paleolithic"; Oakley, "Dating Fossil Human Remains";* Rouse, "The Strategy of Culture History."*

Speakers:[2] Kidder,† Clark,† Oakley, Movius,† Bennett,† Krieger, Teilhard, Heizer, Redfield, Forde,† Willey,† Rouse.

IMPORTANCE OF DATING

KIDDER: . . . For several years, very early in my career, I worked at the Pecos Pueblo in New Mexico, on the transcontinental highway, and, although traffic in those days was not what it is now, there were a great many people going over the road, and they all stopped, came up to the ruin, and asked questions. . . . The principal one was, "Why are healthy, hearty young fellows like you digging up little bones and potsherds and pieces of broken arrow points?" It took a great deal of time to answer, but, as a matter of fact, it was the best thing that ever happened to us, because it forced us to explain why we were working there. The answers we gave, I think, were not wholly truthful, because we said that we were studying the past in order to plan for the future. Nevertheless, it brought us to grips with the problems on which we actually were working and showed us how extremely difficult it is in archeology to apportion one's time and one's energies in the best possible way.

Archeology and, of course, all other branches of our discipline are intrinsically so fascinating that one is eternally led into bypaths, into detail. One has to follow these things, but one must always keep in mind where they are leading one. To follow one's wishes leaves one not only unable to see the forest for the trees but also not able to see the trees for the leaves. It is a most difficult matter for an archeologist to strike an accurate balance between detail and general problems.

In our work at Pecos, however, and in all the other work I have done in archeology, I have in the last analysis come down to questions of time, both relative and absolute. . . . We all know, naturally, that time is the essence of history, and so it seems to me that a session of this sort, on time, both relative and absolute, and the methods of reaching comparative results, is of the greatest importance.

[1] Listed in the approximate order in which they are discussed, usually by title or author. Those which were scheduled for discussion or listed on the program as particularly relevant to a session are marked with an asterisk (*).

[2] Listed in order of first "appearance." Those who were scheduled as "principal discussants" on the program are indicated by a dagger (†).

VALIDITY OF ASTRONOMICAL DATING

CLARK: Heizer has given in his background paper quite a remarkably complete factual survey of the enormous numbers of methods, highly technical methods very often, of long-range dating, applied to the various branches of anthropology. I want to make just one point about this wonderful new method of carbon 14. Heizer says in his inventory paper that the more generally applicable a method is, the more important it is, since the whole object of long-range dating is to synchronize events and trends of development over the whole world. This is, after all, the prerequisite for any synthesis of human history as a whole.

It is the great virtue of carbon 14 that, when all the various snags have been ironed out, we should be able to apply it to any site on which men have lived and lit a fire. Some very interesting results have already been obtained by this method, and it struck me that it was worth making just one point: We are used nowadays, when we open textbooks about early man, to find very round figures applied to some of the stages. These figures have been brought into great prominence recently through a widely read and highly valuable book, Zeuner's *Dating the Past,* of which a new edition has recently appeared.[3] The figures are very largely based on emendations of Milankovitch's method of applying astronomical dates to phases of the Pleistocene, and it is interesting, I think, to see how far the carbon dates that we have so far for the last phases of the Ice Age in northwestern Europe, and for the human cultures which relate to those phases, confirm the dates given for the last stages of the Ice Age by Professor Zeuner.

The date given in *Dating the Past* for the Magdalenian culture, the last of the Paleolithic cave cultures, is something like 50,000 (a very large figure, you see, beautifully round; it would be even rounder if it were 100,000) to 20,000. These dates are B.P.; to change to B.C., you take away a couple of thousand years or so, and you get 48000–18000 B.C.

It so happens that in some parts of northwestern Europe the closing stages of the glacial period have been extremely finely zoned, largely by application of the method of pollen analysis, which is discussed in Heizer's inventory paper. The zones shown in the accompanying table are those employed by Jessen to divide up the late glacial period of Denmark, but they have been applied, really, over the whole of northwestern Europe, from Ireland to the eastern Baltic area, with certain modifications. There are three phases: I, II, and III. The first phase is subdivided *a, b,* and *c,* into cold, warm, and cold parts. Phase II, the Allerød oscillation, is a warm period which comes immediately before the last cold phase, the younger Dryas. This younger Dryas period is the last cold phase of the Ice Age in northwestern Europe, after which you find yourself in the postglacial period.

Now some of these periods have been dated in terms of clay varves [and the varve dates for the Allerød and Younger Dryas periods are shown in the third column of the table. These must be used with caution because they are based upon an unproved assumption that the][4] varves give continuous sequences of annual deposits. . . . I have here two carbon 14 dates for the Allerød oscillation (fourth column of table), the first from Ireland and the second from Hanover—a considerable

[3] Frederick E. Zeuner, *Dating the Past: An Introduction to Geochronology* (2d ed. rev.; London, 1950).

[4] Material in brackets is in the words of an editor rather than those of the speaker, usually for purposes of condensation.

Zones		Dates			Cultures
		Zeuner	Varve	Carbon 14	
III. Younger Dryas		.....	8000 B.C.	...	Ahrensberg
II. Allerød		.....	9000–10000 B.C.	9045 ± 500 9360 ± 720	.....
I.	*c*) Older Dryas	.....	.....	.....	.....
	b) Bølling	.....	.....	.....	.....
	a) Oldest Dryas	.....	12500–13500 B.C.	.....	Hamburgian
...		48000–18000 B.C.	.....	13797 ± 1200	Magdalenian III (La Garenne)

spread. . . . Both fit pretty well with the varve chronology. . . .

Now we come to the date of 12500–13500 B.C. for the Hamburgian culture (third column). The Hamburgian has sometimes been described as late Magdalenian, and a good deal of harm, I think, has been done by that, since it is obviously a quite distinct culture. It appears as far west as northern Holland and as far east as East Prussia but is centered in the northern part of the North German Plain. Nevertheless, it has very close affinities with the later stages of the French Magdalenian. Now I know that French archeologists are rather loath to admit that the Hamburgian is a kind of cousin of the Magdalenian. The way out, of course, is to say that in the north everything gets much later, that Hamburgian really is very late, very retarded, Magdalenian. This has the great advantage that you can retain an enormous date for the Magdalenian and save your face. But I am going to suggest that Dr. Rust[5] is perfectly correct in equating, on purely cultural terms, his Hamburgian culture not with the end-phase of the Magdalenian, which would correspond to the Ahrensbergian culture of the younger Dryas period and the ninth millennium B.C., but with a fairly early stage of the later Magdalenian, IV or V.

Now I am making this supposition purely on a cultural basis, but . . . a number of dates are beginning to come in for the cave Magdalenian in France. The one I have included in the table, La Garenne, dates from Magdalenian III, which is a little earlier, if Dr. Rust is correct, than the Hamburgian. More than one date has been obtained for La Garenne, but I am going to take one which is based on the ash sample: 13797 ± 1200. It is not a very certain date, but it seems to me to tie up pretty closely with the Hamburgian date, which is just slightly anterior. Of course, I could also remind you of the date from a sample from Lascaux, which is of the same sort of order. . . .

It seems to me that we are working on a number of lines here, each of which has been followed up by technicians in their own field, and that they converge pretty well on a point. They seem to suggest that the Magdalenian, far from ending at 18000 B.C., probably ended at more like 8000 B.C.; and, far from beginning anywhere near so early as 50000 B.C., began about 15000 B.C. or perhaps even later.

I conclude by asking a question which I hope Movius will take up. If the only date in the Zeuner-Milanko-

[5] A. Rust, *Die alt und mittelsteinzeitlichen Funde von Stellmoor* (Neumünster, 1943).

vitch system we are in a position to check by means of carbon 14 is found to be as badly wrong and as grossly overinflated as this, how much reliance should we place on the long-range dating for the earlier phases of the ice age? I only ask this question. I don't know the answer.

OAKLEY (statement revised after session): I would like to make some comments in supplement to Professor Clark's. The La Garenne figure which he has quoted was based on a very impure sample (ashy material mixed with sand). Samples containing a high proportion of nonorganic material tend to give plus rather than minus errors. Two samples of carbonized bone from the same site gave lower figures, and it seems to me that these may involve a smaller error than the sandy ash. If one takes the average of the La Garenne results, say 11000 B.C., it ties in fairly well with the Lascaux figure (13600 B.C.).[6] I know there is controversy about the exact R.2 and R.3 horizons of the Lascaux charcoal; but nevertheless, I think one can say that it is very close to the "Aurignacian"-Magdalenian boundary, because associated with it were worked bone or antler points, which, according to the Abbé Breuil, could not be later than Magdalenian II, although they could be slightly older—Upper Perigordian, that is to say, late "Aurignacian" in the older terminology. Provisionally, one may put the "Aurignacian"-Magdalenian boundary at approximately 15000 B.C. It is not unreasonable that the first three stages of the Magdalenian should have occupied only 3,000 or 4,000 years.

And, finally, a general point: I think that this shortening of the later Paleolithic time scale is going to constitute a revolution in archeological thought. The Magdalenian culture was formerly supposed to have begun at least 50,000 years ago and to have lasted for some 30,000 years. Now it appears that it began only about 15000 B.C. and lasted until about 7000 B.C. I just wonder what the repercussions of this shortening of the Paleolithic time scale are going to be, for example, in regard to the probable time of entry of man into the New World.

MOVIUS: It seems to me that both Heizer's and Oakley's papers clearly emphasize the fact that there has been a tremendous lag between the rate of discovery of both fossil and cultural remains of early man, on the one hand, and the advances that have been made in the relative dating methods on the part of the natural sciences dealing with the Pleistocene epoch, on the other. . . . But, notwithstanding . . . this lag, I can see no justification whatsoever for falling back on the Spitaler-Milankovitch-Zeuner astronomical calendar and deluding ourselves into thinking that, because these are the only figures we have which pretend to extend our chronology back to 30,000 or 35,000 years beyond the present, they are therefore of significance.

The objections to this approach—and it is based on some very plausible theoretical conclusions, as they have been propounded by the astronomers, by the geologists, and by the climatologists—really add up to an overwhelming series of data which make it almost fantastic that we should take the dates seriously any longer. In fact, the astronomers state that before 50000 B.C., using Milankovitch's formulas, the magnitude of the probable error involved is of such a nature that the curve can be made to do almost anything that the individual investigator wants to have it do. This has been shown by Brouwer, an astronomer at Yale, and by Whipple at Harvard.

The geologists, looking at the degrees

[6] C. B. M. McBurney, "Radiocarbon Dating Results from the Old World," *Antiquity*, No. 101 (Newbury, 1952), p. 39.

of weathering on the soils of the late glacial period, where we have pretty definite stratigraphy, and carrying that back to the Middle Pleistocene or even the Lower Pleistocene, cannot reach any measure of agreement with the Milankovitch figures. Somewhere there is a limit to this approach, but, in so far as its relative values are concerned, the time interval following the Milankovitch approach does not anywhere nearly allow sufficient time for the tremendous decomposition that has taken place, let us say, since the second glacial advance. Nor is the time allowed for the Villafranchian of the first interglacial anywhere near long enough possibly to account for the tremendous depths of sediments, the tremendous degrees of erosion, and the great orogeny that took place at that time, following the end of the Pliocene. . . .

In other words, it seems to me that if we have only a relative date in a given instance, it is a great deal better to assess its significance on a relative basis than to drag out a figure that purports to be absolute but which partly, or mainly, because of the magnitude of the probable errors involved—leaving all other considerations aside for the moment—is apparently very far from precise.

SOIL ANALYSIS

MOVIUS: [Two potentially useful methods of establishing a more satisfactory long-range chronology are Oakley's flourine technique, if it can be combined with radiocarbon dating, and] studies of radium concentration in sea-floor sediments, along the lines that have been indicated by Piggott, Urey, Ovey, and others. [Neither, however, is practicable at present.]

Another method, which offers great prospects of success . . . is the technique of soil analysis. . . . Unfortunately, the main exponent and principal developer of this method, a German named Robert Lias, was killed in the last war, and no one has subsequently resumed the work. Soil analysis, or the mechanical analysis of sediments, is extremely tedious work. Furthermore, it is costly and time-consuming, although the necessary apparatus is not particularly expensive. Potentially, it makes it possible to establish not only the origin of a given set of sediments in a certain horizon but also the agency of deposition for the accumulation in question. Indirectly, also, you get very definite evidence of the prevailing climatic conditions at the time the beds in question were being formed.

Now the implications of results of this sort with respect to relative datings, it seems to me, are obvious. The method provides yet another check upon establishing the time when any given horizon was accumulated during the Pleistocene. But more important, I think, for anthropologists and archeologists is that, by the very employment of this technique, one inevitably arrives at important clues bearing on the environmental, biogeographical, or ecological conditions which prevailed in a given region at a given point in time in the past, and the implications of this with regard to a broader interpretation of the pattern of human activities during the Paleolithic are obvious.

What is urgently needed, of course, when we have sufficient data of a reasonably precise relative nature, is the preparation of a series of maps, each covering a given interval of Pleistocene time, on which the evidence derived from geology, paleontology, paleobotany, soil studies, and other disciplines of the natural sciences can be plotted. If, on these maps, we can then superpose the various datable culture manifestations associated with each of the several time horizons in question, it may be possible ultimately to provide a broader base for the interpretation of Paleolithic archeology.

This approach has been used successfully with regard to the Mesolithic cultures of northern and western Europe, and therefore I can see no valid reason whatsoever for failing to obtain positive results on the Paleolithic level as well; but it requires a team effort, since no single person can ever hope to become professionally competent in more than one of the various specialized fields, each with its own particular techniques; and, single-handed, no one person could ever carry such a project through. But I am confident that, if a carefully selected team of specialists in the various fields under consideration were able to devote its full time and efforts to a joint study of Pleistocene events and Paleolithic archeology in a region where a great deal of the spade work has already been done, such as western Europe, significant and fundamental results would inevitably accrue which would in large measure revolutionize our present concepts of relative dating, on the one hand, and Paleolithic archeology, on the other.

OAKLEY: I might add that Professor Zeuner is doing some valuable work on soil analysis in the London University Institute of Archaeology. He and Mr. I. W. Cornwall are developing soil analysis along lines similar to those begun by Robert Lias, and I think that work will bear important fruit, particularly in connection with Paleolithic archeology.

Quite recently, we undertook an investigation on the Bury St. Edmunds skull (which those of you who are interested in fossil man will know about), and a mechanical analysis of the brick earth from which it was obtained indicated that it was of periglacial origin. Previously it had been thought that this brick earth had accumulated under interglacial conditions. I think this bears out what Professor Movius has mentioned, that mechanical analysis can help in relative dating, because it may show whether a particular bed is glacial in age or interglacial. (Actually, it did not prove to be relevant to the date of the skull in this case, because, when its R.1 date was checked by fluorine, we found that it was almost certainly intrusive.)

BENNETT: This discussion points up the fact that many of the dating techniques require the assistance, if not the total work, of specialists from other fields, such as chemistry, physics, geology, botany, and the like. Since these other fields are not set up primarily to work for archeologists, considerable thought should be placed on the strategy of archeology, in order not to misuse the time of the specialists. For example, many of the specimens first sent in for carbon 14 analysis were such that, even if dated accurately, they would have had no significance. . . .

DEFINITION OF THE PLEISTOCENE

OAKLEY: In regard to Milankovitch dating, I think it might be useful for anthropologists to realize that those who employ that chronology have never claimed that the astronomical curve, based on the variation in the tilt of the earth through the ages, explains the Ice Age. It explains only the glacial fluctuations. There is a big unknown factor x which caused the Ice Age, and, given that, you have to explain the fluctuations of the glaciers.

I think that is a rather fundamental point, because, if there is an unknown factor x which caused the Ice Age, what happens if that factor x is also varying, as it may well be? It would obviously iron out any minor fluctuations due to variations in tilt. This consideration, I think, raises the question as to whether it is legitimate to distinguish between the "Pleistocene period" and the "Holocene or Postglacial period," because it does appear that we are still

living in the Ice Age. Speaking in my capacity as geologist rather than as paleo-anthropologist, I would say that the existence of large masses of ice on the earth's surface is a highly abnormal phenomenon.

There have been earlier ice ages, during the pre-Cambrian and in the Permian period, but throughout the greater part of geological time there were no great ice masses anywhere. We have evidence that during some past ages the vegetation in the Arctic and the Antarctic was of temperate type. The fact that even now something like 11 per cent of the land area of the earth is covered by ice shows that in a sense we are still in the Pleistocene Ice Age.

Professor R. F. Flint informed me shortly before the beginning of this symposium that evidence in the Great Lakes region suggests the probability that there was an advance of the ice sheet (Cochrane substage), following the Thermal Maximum, seemingly as late as about 1500 B.C. With this concept in mind, we should perhaps seriously consider dropping the view that we live in a truly postglacial epoch distinct from the Pleistocene. This may, of course, have pretty considerable implications for the future of humanity.

KRIEGER: I should like to ask Dr. Oakley how the Pleistocene was divided from the Pliocene in the Old World—on the basis of fauna or on the basis of glaciation?

OAKLEY: That is rather a big question. It was gone into in considerable detail at the last International Geological Congress in London in 1948. A commission reported that the three genera of *Equus, Elephas,* and *Bos* spread widely over the land areas of the world just about the same time that there was a cooling of the climate and extension of glaciers and the formation of ice sheets. Thus both criteria, the climatic and the paleontological, serve to some extent to define the base of the Pleistocene.

The paleontological criterion has proved the more serviceable, because evidence of the first glaciation is often very localized and difficult to detect. The basing of the beginning of the Pleistocene on the spread of *Elephas, Equus,* and *Bos* has resulted in the arbitrary boundary between the Pliocene and the Pleistocene being placed much lower than heretofore. A great thickness of sediments in the Java geosyncline, which was formerly classed as Pliocene, is now included in the Pleistocene. Thus the new definition has resulted in almost doubling the period of time counted as Pleistocene. The newly added basal zone is called the "Villafranchian stage." There are still some differences of opinion about it. Some people claim that that is, after all, just another name for the old Günz ice age, but other people consider that the Günz glaciation corresponds merely to the top of the Villafranchian stage. The genus *Homo,* or shall we say man the tool-maker, does not appear to have been in evidence during the earliest part of this stage of the Pleistocene as now defined.

KRIEGER (submitted in writing after session): If the beginning of the Pleistocene is defined in this manner, why cannot we similarly say that the Pleistocene has ended because its "index fossils" have become extinct?

INTERPRETATION OF STRATIGRAPHY

CLARK: In Dr. Oakley's paper, I was much impressed by something that was so obvious that it is hardly ever stated: the value of his R.1 category, that is, the importance of establishing the age relation between a specimen and the deposit from which the specimen is recovered. This is obviously basic to the whole interpretation of archeologi-

cal or physical-anthropological correlations with natural sequences, and yet it has been very much taken for granted. Because it has been taken for granted, I think we very often assume, perhaps rather hastily, that specimens, whether they are parts of human skeletal remains or archeological remains, are of the same age as the deposit in which they are found. We tend to accept what are really apparent associations for real ones, in the historical sense.

Dr. Oakley has discussed in some detail some of the methods by which you can test whether the human skeletal remains are, in fact, of the same age as the deposit in which they are found, or whether they are the same age as the remains of wild animals which appear to be associated with them. Therein, of course, resides the great value of the fluorine method. He has demonstrated it notably in the case of the Piltdown man and the Galley Hill skull.

But, of course, the same applies to the archeological material with which I am personally associated. If the fossil membranes of tree columns can survive from earlier phases of geological time and be incorporated, for example, in postglacial deposits, how much more must we expect the more resistant fossils of human culture, such as potsherds and, above all, flints, to survive as derived from their original deposits?

I have a feeling—I am not an expert in this field—that the question of derived flints has done more to bedevil Lower Palcolithic studies than any other factor. Indeed, I have a deep-seated feeling that the attempt to unravel the tangle presented by assemblages of more or less derived flints, incorporated in geological deposits, is no longer worth the candle; that we should try to direct our research to finding, first of all, and then exploiting the sites where we have complete flint industries *in situ*, really in contemporary natural deposits. We ought to be prepared to spend a good deal of money without obtaining any results in order to find such sites, and we should cease the weary and conventional round of collecting and studying with assiduity what are only the sorted accumulations of the durable parts of hunter-fisher cultures incorporated in secondary deposits.

Second, we are concerned in archeology with the question of intrusive fossils. In dealing with postglacial deposits, we have the familiar animal burrowings, the cracks that one gets in clays and peats, and the factor of sinkage of specimens. There are obvious checks which we can employ. We can analyze the pollen content of samples taken from sockets of bronzes, for example, and see if they are of the same age as the deposit in which the objects were incorporated, or we can just decide from which level in the peat or the organic mud the object has sunk. But I think the main conclusion to be drawn from consideration of intrusive fossils is that it is highly dangerous to depend on isolated finds or even on a few finds. We really need a great mass of material before we can eliminate the sources of error.

Third, we have to deal with the enormously important factor of the effect of human activities. It is not only a matter of burial; if one thinks of people living on a patch of ground and imagines all the things they do to that ground, even at a quite primitive level of culture, one can realize that there are many deceptions in store for the naïve or uncritical archeologist. In fact, I can summarize this last point in a sentence by saying that archeological stratigraphy must be interpreted, like all forms of stratigraphy, in terms of the behavior of prehistoric man.

OAKLEY: I agree with Professor Clark that it would, perhaps, be quite interesting as an experiment to try to apply

this R.1 to R.4 and A.1 to A.4 terminology in archeology, too. For instance, when surface finds are dated by archeologists, the datings fall into the category I have called R.4, and, like the other morphological datings, may be misleading. Then the establishment of the R.1 dating of pollen and flints is sometimes of basic importance in the interpretation of archeological sequences.

BENNETT: I [too] was impressed by the importance of Dr. Oakley's examples of differential ages of materials found in the same stratigraphic level or bed. In general, American archeologists have tended to neglect this possibility and, instead, have worked on the assumption that all materials from a given arbitrary stratigraphic level are contemporaneous. Now it is clear that this may be a rather serious source of error.

TEILHARD: I wish to mention, as particularly tricky, the case of stone implements collected (in rolled and unrolled conditions) in an ancient gravel *G* underlying (as they usually do) a silty formation *S*. In such a site, it is often assumed that (*a*) the rolled implements are older than the unrolled and (*b*) the unrolled specimens are all of the same age. But both the assumptions can be incorrect, the first one because the rolled implements may simply represent the rewashed elements of a workshop operating at the edge of a *running* (and occasionally flooding) river, and the second one because, in the relatively frequent case where the overlying series *S* does not represent the *first* but the second or even the third silt deposited over the gravel *G* (the preceding ones, *S1* and *S2*, having disappeared by erosion or ventilation), a number of residual younger artifacts are likely to occur at the surface of the old gravel, mixed with the genuine products of the ancient workshop. In this case (as in so many others) no good "dating" can be obtained except statistically and on a regional scale.

RELATIVE, ABSOLUTE, AND ESTIMATED DATING

HEIZER: My point has reference to the A.3 and A.4 dates in Dr. Oakley's paper. I wonder whether these are actually absolute dates. For example, a source bed from which implements come may have required a very long time for its deposition, and therefore this bed will represent a long span of time. Finds made in such a bed may be attributed only generally to that layer; or one may be able to say that they occurred in the lower, middle, or upper portion of that layer, and therefore you get a little refined stratigraphy. But, when the find is attributed only to the general layer as such and granted that the time span for that layer is known, you are not getting an absolute date. The date range may be 10,000 or 15,000 years, and hence you are dealing there, at least as far as precise definition goes, not with an absolute, but with a span, date. I was going to ask Dr. Oakley if he would consider something like a separate subdivision, which might be labeled "span" or "limit" dates, because what you really get out of a dating of that sort is only "not earlier than or not later than that."

OAKLEY: In a sense, one is entering the sphere of linguistics, because it is a question of what is meant by "absolute." To my mind, this term does not refer to the principle of dating in years at all. To say that a specimen is 10,000 to 15,000 years old because it is found in a bed which, by radiocarbon determination of peat in that bed, is within that time range—to my mind that is absolute dating. I do not mean that an absolute dating is a pinpoint date, even on a large scale. It is simply the principle of dating a specimen, whether directly from inherent evidence within the specimen or, as in A.2 dating,

where the bed itself is dated by peat, shall we say, and then specimens from it are given a date.

Now A.3 dating does not seem to me to be so very different, except that it covers a span; but so might your A.2 dating. It is just a question of how far removed you are from the dating method and its application to the specimen; and to my mind, if you say that a specimen is more than 10,000 years old, using either the indirect A.3 method or a more direct method, that is an absolute dating. I am open to correction on this; it may be that one should not use the term "absolute dating" in this sense, but it is very useful to do so.

KRIEGER: I think that the magnitude of the date span under consideration should be a factor in deciding whether the date is absolute. For example, the dates written on the board by Oakley[7] include one of 9,000–10,000 years, supported by two radiocarbon determinations, so that this span can be considered an absolute date. But the one below, 20,000–50,000 years, is merely an estimate.

OAKLEY: Yes, but that still is not a relative dating. . . .

HEIZER: Would it be possible simply to assign the term "maximum limit" or "minimum limit"?

OAKLEY: Well, there is obviously room for refinement in this sort of classification. You might say "maximum A.3 age" or "estimated exact A.3 age."

KRIEGER: It would appear that Heizer is correct in suggesting three kinds of dates in place of two, but I would call them "relative," "absolute," and "estimated." A dating which is either absolute or estimated can also be relative in the sense used by Oakley in his excellent inventory paper.

REDFIELD: Do relative datings always take the form of a statement that the specimen is "as old as or older than or younger than" something else? Is it true that the absolute datings always take the form of the statement that the specimen is "so many years old," more or less?

OAKLEY: It is the usage that I have followed. Though not a believer in the Milankovitch system, I may have been influenced in my choice of words by Professor Zeuner's terminology, because he speaks of the absolute chronology, and he has based it on the theory of the Milankovitch curve fit. But, to my mind, that is an estimate, and yet it is known as *the* absolute chronology. The Piltdown skull, 3 years ago, was said to be 600,000 years old—on the absolute chronology. Well, I used the term "absolute" in that sense, without believing in *the* absolute chronology. But, from what Professor Krieger says, it might perhaps have been better from the semantic point of view to have had a threefold classification of relative dating (R.1, R.2), absolute dating (A.1, A.2), and estimated dating (E.1, E.2).

FORDE: Could I make one remark? As no linguist has intervened, I want to put in a plea for not complicating this terminology unduly, and to remind the conference that, surely, the terms "absolute" and "relative" have a high respectability in Near Eastern and prehistoric European archeology. We had an absolute dating for the Minoan in terms of Petrie's chronology. When Petrie's chronology became obsolete, the absolute dating for the Minoan changed; so that the whole understanding of the terms "absolute" and "relative" is fairly straightforward, I should have thought, in relation to the subject.

LONG-RANGE TIME SCALES

BENNETT: I would like to raise a point, based on the first two background papers by Heizer and Oakley, concerning the need for more compre-

[7] In connection with the discussion of astronomical dating, see section on "Validity of Astronomical Dating," above.

hensive units for relative dating of long time scales. Although it is important to know that some materials are earlier or later than others, we should also have some measure of the magnitude of these time differences. Whether the scaling units be calendar years, centuries, or generations seems immaterial, so long as they distinguish contrasts in the ratio of 10 versus 500, rather than 490 versus 500. This seems to me of particular importance if the long time record is to be used for interpretations involving cultural change. We must have some relative measures of time which will allow comparability; for example, so that a unit of such-and-such a magnitude in one part of the world which showed a certain quality and quantity of change could be compared with an equivalent unit in some other part of the world.

Dr. Oakley, of course, is dealing with an enormous time range, particularly in contrast to that dealt with in the Americas. He makes the point, and I certainly agree, that great refinements of absolute dating are not too worth while for the long period. When we are dealing with an estimated 250,000 years, precise dates are of little merit. Such long time sequences present a challenge for new types of classifications which will organize the materials in a manner significant for interpretations. The current popular classifications discussed by Dr. Rouse in his paper, are essentially based on short time spans.

CHOICE OF OBJECTIVES

WILLEY: This is with reference to Dr. Rouse's paper on the strategy of culture history. In one section he deals with what he calls the "choice of objectives in culture history," referring mainly to archeology in the particular context, and he makes the distinction between a rigid, all-inclusive program, in which one objective follows immediately on another, leading up to a final objective and resulting in a sort of over-all capturing of the total archeological universe, as opposed to a program which considers each objective independently, as a goal in itself. As an example of the first, he refers to Walter Taylor's conjunctive approach, and the second is more or less the course in which archeology has developed. I wonder if Rouse would be willing to consider something of a middle ground on objectives, where a defined problem —e.g., the history of the rise of prehistoric urban centers in Peru and the cultural and ecological factors which made possible such a rise—may not be phrased and pursued in terms of a closely integrated series of planned objectives: site survey, ceramic classification, ceramic chronology, architectural types, architectural chronology, etc.

As I understand Taylor's conjunctive approach, such a program as this would differ, in that it is highly selective in its objectives or problem. It would not be an attempt to recover *all* the data in a particular archeological site or group of sites or area.

This, as Rouse indicates, leads to the question of values in problem selection. I do not see how we can avoid making such selections, because a comprehension of a total archeological universe, no matter how restricted the site or area, is impossible. Data and combinations of data must be sought which bear upon a particular problem.

An agreement on problem selection, or the values behind it, does not seem to me to be necessary or even desirable, after we reach a certain point in the course of archeological research. A near-unanimity of opinion has developed in archeology over the last fifty years as regards the importance and necessity of typological formulations, distributional studies, and chronological studies, as these are used in the

plotting of prehistoric cultures in space and time. This, I believe, is a well-agreed-upon substructure (not the specific results, but the applications of these methods). Diversity in problem choice and accompanying value judgments will enter most strongly above this level and will be concerned with problems of interpretation (historiography, process, function).

ROUSE (submitted in writing after session): The point about choosing one's objectives from the standpoint of a problem is well taken. As stated in my paper, certain objectives lead logically to others, and it is therefore advisable to perform them in a series, leading to the solution of one or more problems.

It is also true that archeologists are generally agreed as to the necessity for studying typology, distribution, and chronology (not to mention the analysis of technology and design). I do not believe, however, that they agree—or should agree—upon one particular kind of typology, distribution, or chronology. To my mind, there is as much room for value judgment here as in the field of interpretation.

CLASSIFICATION OF ARTIFACTS

WILLEY: Rouse has, in his paper, defined a "class" of artifacts as a series of objects grouped together—an empirical approach. As opposed to this, "types" are a series of attributes which are shared by a group of artifacts. Furthermore, types, as Rouse points out, are involved in questions of cultural significance, while classes are not. The criteria for the establishment of classes are listed as: material, techniques of manufacture, shape, function. I would like to raise a question on the matter of function. When the imputation of a specific function is laid upon an artifact, we are defining units of culture for interpretative purposes. As such, function seems to me to be a criterion of the type rather than the class, as these terms are defined by Rouse.

ROUSE: It seems to me that this is a matter of terminology. When I was speaking of the criteria for establishing classes, I was thinking in terms of what I would consider a descriptive approach to function. In calling an artifact a "knife," for example, we do not necessarily imply how it was used, but simply that it has qualities which make it suitable for use as a knife, i.e., that it has a long thin blade, a sharp edge, and various other descriptive attributes.

On the other hand, I think Dr. Willey is quite right in stating that we have to make a distinction between such descriptive attributes, which refer to the class, and less empirical or more theoretical traits, which refer to the manner in which the artifact was used. Such inferential traits, as he says, should be considered criteria of the type rather than of the class.

WILLEY: Rouse also mentions Krieger's criticism that in dealing with potsherds the archeologist has difficulty with the type concept, as two sherds from the same vessel may be classified as two different types. Rouse's idea of the "mode," or the classificatory unit pertaining to a part of an artifact, takes care of a part of Krieger's objection, but I believe Krieger's note of caution has some justification. There is a definite danger in the skewing of pottery-type percentages (indicative of popularity time trends) in favor of types which were represented by larger vessels (breaking into more sherds) as opposed to types represented by smaller vessels. This is not so important for sequence or chronological problems as it would be for attempts to reconstruct the life of the culture. By counting sherds alone and unless other precautions were taken, we might have the impression that large water jars were

extremely common as opposed to small decorated plates.

MIDWESTERN TAXONOMIC SYSTEM[8]

WILLEY: I have one final comment, with reference to that old classificatory football, the Midwestern Taxonomic System. In spite of Rouse's defense, I still feel that the upper hierarchical brackets of this system have been widely employed in conjunction with the components and the foci and are undeniable parts of the system and that the genetic nature of the system which is implied, even if it is not openly claimed, forms something of a barrier for historical reconstruction. The difficulties that now arise in attempting to use Mississippian and Woodland categories as major time periods (e.g., the fact that, in the Upper Mississippi Valley, Late Woodland follows Mississippian rather than precedes it) . . . derive from the confused objectives of the system. Mississippian and Woodland, in their entirety for eastern North America, are traditions rather than time horizons—traditions which each had a long persistence. Both last until historic times, although the Woodland has earlier beginnings in the area.

ROUSE (statement revised after session): I did not mean to imply that the Midwestern Taxonomic System has not been used genetically, but only that the originators of the system—and, I think, the people who have had the most success with it—did not so use it. In part, I think that this depends on how you treat the hierarchy. I do not know how many of you know the system, but it starts with a series of smaller units, which are called "foci"; these are grouped together into "aspects"; these into "phases"; and these, in turn, into "patterns." If you start at the top of this hierarchy, with the patterns, and move down, then your approach is genetic, because each of your units is a subdivision of a larger one. If, on the other hand, you start at the bottom with the foci, and move up, then your approach is not necessarily genetic, because you have the possibility, for example, of finding that a focus belonging to one pattern has amalgamated with a focus belonging to another pattern, so that there is not a one-to-one relation between focus and pattern. In that usage, I do not think the system is genetic.

While, as Willey points out, some archeologists have considered the patterns to be traditions, it can now be shown that in many cases the traditions tend to cut across the patterns instead of coinciding with them. For example, the East River and Windsor traditions of the metropolitan New York area seem to have originated in different divisions of the Archaic Pattern and then to have developed into correspondingly distinct units of the Woodland Pattern.[9] From this standpoint, the patterns represent stages, corresponding to the units of the developmental classification in Andean prehistory rather than traditions.

CULTURAL CONTINUITY

BENNETT: Before a sequence can be used for study of change, it must be determined whether it represents a cultural continuity, that is, the cultural history of a given population, or whether it pertains to the history of man in general. For example, the sequence established for the Virú Valley, Peru, was shown, by various tests of the occurrence and distribution of ceramic types, to represent the culture history of a single base population. We can thus study the cultural changes

[8] For further discussion of the Midwestern Taxonomic System see chap. v, section on "Developmental Classification."

[9] Carlyle S. Smith, "The Archeology of Coastal New York," *Anthropological Papers of the American Museum of Natural History*, XLIII, Part II (New York, 1950), 152–53.

and compare them with studies made by other anthropologists. But if we were uncertain whether the finds in this sequence represented the cultural history of one population or a dozen, such interpretations would not be possible.

ROUSE (submitted in writing after session): One way of taking care of this problem would be to apply the concept of tradition. If only one tradition is represented in the Virú Valley, then I should think it is justifiable to treat the local sequence as a continuity; if not, then it seems to me that a separate sequence would have to be established for each tradition, as is done in parts of the eastern United States.[10]

PLACE OF ETHNOLOGY IN STUDIES OF CULTURE HISTORY

FORDE: Our president has stressed to us this morning that the aim of the symposium is not to deal with the conventional fields of anthropology separately but to deal with approaches as they are relevant to all of what you might call the subject-matter branches. I have a slight suspicion that this was in Chairman Brew's mind in bringing onto the platform someone who has not worked in the archeological field for years. . . .

I think I am probably in the minority in Britain in trying to keep anthropology in existence as a subject at all. . . . By that I mean that the very great advances which have been made in the definition, analysis, and understanding of the conditions of existence of social institutions among pre-literate peoples have increasingly absorbed the attention of British ethnologists . . . that they have quite rightly built strong links with certain fields of sociology, and to a lesser extent . . . with certain schools of the philosophy of history.

The effect of this concentration of interest has been very unfortunate from the point of view of the subject of discussion this afternoon, because . . . it has meant that a great deal of material of vital importance for culture history . . . is being lost in field investigations by cultural anthropologists. We are in the habit of stressing the point that, unless we get to "the savages" quickly, there will be nothing savage in their way of life to study. That is true not only in the field of social institutions [but also] in the field of material products, and it is in connection with the material products of different human aggregates that the tie-in with culture history is mainly to be made. . . . Relative to what we have been talking about this afternoon, and the later periods of prehistoric archeology, the bridge is going to be built in terms of the techniques and manufactures of the peoples who are to be studied in the field.

Now that bridge is being rapidly destroyed, quite as rapidly destroyed as the social institutions, by the impact of European trade and the adoption of European techniques. Throughout Africa, Southeast Asia, and many other areas, we are all perfectly aware of the fact that enamelware is replacing pottery. This applies, too, with regard to the indigenous iron-smelting industry in Africa at the present time. Iron smelting is practically extinct, and iron forging is being rapidly driven back into the more remote corners of the Belgian Congo.

This is becoming increasingly important, because small beginnings are being made in the only archeological field that I know at all well, in the later phases of African cultures. I am not referring to material relating to even the African Neolithic, the Gumban, and so forth, but to cultures such as the Sao culture, which Lebeuf[11] is studying in the Chad region, and to the

[10] *Op. cit.*, p. 107.

[11] J. P. Lebeuf, *Fouilles archéologiques dans la région du Tchad* (Brussels, 1947).

Nok cultures of northern Nigeria, which are being worked on by Fagg.[12]

I think there will be something of an awakening of field archeology in these African areas. (I cannot speak for the Asiatic areas, although I imagine a great deal is likely to happen in India in the next generation.) It will be more than a pity if, when we come to the ethnographic literature or when our successors come to it, the bridge can never be crossed.

The lack of advance in the field of artifacts, manufactures, and techniques is, perhaps, fairly well illustrated in a book with such a definite culture-history bias as Baumann's study, published just before the war, on African civilizations.[13] Practically all the documentation from Baumann is pre-1920, and a great deal of it is late nineteenth century. I imagine that, to a considerable extent, the same is true for Oceania and New Guinea. . . . In other words, the great development in field work on the study of social institutions, for which I think none of us should stand to be reprimanded in any way—it has, as a matter of fact, an admirable quality, namely, that field workers have increasingly gone into the field with a better theoretical equipment, with a stronger sense of problem —this development has not been accompanied by an equivalent systematic body of work on what is conventionally called the "material" culture of primitive peoples.

To take one example, I was on one fairly recent occasion trying to find out what literature there was on the distribution and variation in type of the raffia-weaving loom in the Niger-Congo province, and the uses to which raffia cloth was put. Many of you will know that they enter very largely into the prestige systems of many Belgian Congo peoples; that in certain areas of the Guinea belt, it would look as though raffia weaving put up a considerable resistance to the introduction of cotton. I am not making an ethnographic discovery here; it is a hunch. There is no literature on the subject, as far as I can discover. . . .

The same situation can be illustrated, I think, by the way in which the antique yarn about the Hamites in Africa goes down from generation to generation. We hope that Professor Greenberg has now given it something of a *coup de grâce* in the linguistic field. [The theory is that] the Fulani-speaking peoples of West Africa are brown-skinned, they keep cattle, and therefore, they talk Hamitic languages. Then the reverse argument operates. Once the linguists are prepared to assume that Fulani is Hamitic, they become the authority for the fact that the people are cattlekeepers, as a result of their racio-linguistic characteristics.

I do not think the solution can be found in trying to get specialists, social anthropologists, regularly and systematically to make studies of primitive technology, but certainly most of them are capable of, and would be prepared to publish, systematic records of what they do have, if there were more encouragement in this field. . . .

There is one related question concerning the strategy of culture history. . . . We still have, for certain areas in Africa and, I imagine, for certain areas in Melanesia, opportunities to work out the successions of development in social institutions. For the West African Sudanic belt, one can get back to the tenth century A.D. . . . The spread of the eastern Bantu peoples, as the linguistic evidence presented, for example, by Greenberg suggests, was probably very rapid, and . . . [thus]

[12] Bernard Fagg, "Archaeological Notes from Northern Nigeria," *Man*, No. 48 (1946), pp. 49–55.

[13] H. Baumann, R. Thurnwald, and D. Westermann, *Volkerkunde von Afrika* (Leiden, 1940).

we may be able to get quite a considerable insight into the spread of African population aggregates. (I do not call them "societies" because I think they were population aggregates, continually splitting up and then re-forming into a succession of societies. It is not the societies that have the continuity so much as the succession . . . of cultural forms and social structures.) Both in the field of the history of social institutions and in the field of the history of techniques, we are in grave risk of losing documentation that is of great importance to us in linking up with the whole history of human culture.

CHAPTER III

Problems of the Historical Approach: Results

Chairmen: Alexander Spoehr and J. Grahame D. Clark.

Papers discussed: Vallois, "Race";* Teilhard, "The Idea of Fossil Man";* Straus, "Primates";* Weinert, "Der fossile Mensch";* Boyd, "The Contributions of Genetics to Anthropology"; Carter, "The Theory of Evolution and the Evolution of Man"; Movius, "Old World Prehistory: Paleolithic";* Oakley, "Dating Fossil Human Remains"; Childe, "Old World Prehistory: Neolithic."*

Speakers: Eiseley,† Hooton,† Straus, Weinert, Boyd, Koppers, Washburn, Senyürek, Oakley,† Linton, Coon, McCown, Lévi-Strauss, Spoehr, Lowie, Tanner, Clark, Movius, Lindblom, Mead, Kroeber, Forde, Rouse, Canals Frau.

AGE AND AREA: NINETEENTH-CENTURY VERSION

EISELEY: Dr. Vallois's excellent historical treatment of the subject of race and Dr. Teilhard de Chardin's growing enthusiasm for Africa as a possible ancestral seed-bed of the Hominidae have given us the fortunate opportunity of examining the amazing reversal in the course of one century of the use of the age-area concept as applied to human differentiation. The shift is so paradoxical as to wring a chuckle even from the sober man of science.

I quote from *The Testimony of the Rocks,* a group of essays composed in the 1850's by Hugh Miller, a mid-nineteenth-century British geologist. Miller speaks as follows: "Let me remark that the further we remove from the original center of the race, the more degraded and sunk do we find the several varieties of humanity. In the backwoods of America, in southern Africa, in Australia, and in the Polynesian islands, the whole Adamic type has been asserting its superiority and annihilating before it the degraded. But taking into account merely the original varieties, there seems to be a general rule that the further we remove in any direction from the Adamic center, the more animalized and sunk do we find the various tribes or races. Wherever man . . . has fallen least, wherever he has retained at least intellectually the Divine Image, this Caucasian type of feature and figure, with, of course, certain national modifications, he also retains."

In passages too long for full quotation, Miller then proceeds to characterize these marginal folk most distant in space from the original site of Eden. He speaks of the Lapps, "squat, ungraceful," having a "pyramidal skull of small capacity." He examines the "sooty-skinned," thick-lipped African, who is noted as possessing "partially webbed fingers." In Miller's pages we also encounter the squat Mongolian with his high cheek bones and "unintellectual piglike eyes." And, finally, remotest of men, we are brought face

to face with "the hideous, small-eyed, small-limbed, flat-headed Fuegian, perhaps the most wretched of men." All these varieties into which we find humanity has fallen are, in Miller's estimation, lapses from an original Caucasian type. "They do not represent," he observes, "save in hideous caricature, the glorious creature molded of old by the hand of the Divine Worker." Here, we have a perfect exemplification of the doctrine of the Fall applied to the solution of the racial problem. It contains all the naïve ethnocentrism and assured pride of position that so often accompany imperial growth.

We need not blush particularly that this happens to be a white mythos. Its parallel can be found in other lands. It all makes a quite simple story; namely, that man likes his own skin best. It is the stranger, always, who is marginal and has to be explained. Nevertheless, one may still be impressed with the latent capacity in man for simple objective humility even at great cost to previously held beliefs.

I think the shift in the use made of the age-area concept within a few decades of Miller's interpretation of the significance of distance from the "Adamic center" is a profound tribute to man's capacity for growth. Today, Father Teilhard and other scholars among us seek the "Adamic center" in the shape of crude but rising small-brained bipeds whose ancestral trail runs directly to some clump of trees on the edge of the East African savannah. The miracle, in the light of such widely held views as Miller's, is not that the idea of evolution has been opposed but that, conflicting as it did so drastically with Western man's conception of himself, it should ever have been accepted at all. In one stroke, so to speak, the fallen angel recognized his kinship with the rest of the animal world and accepted it gracefully. This great act of self-abnegation, hidden among yellowing scientific documents, remains, in my belief, man's most remarkable feat of pure intellect to date.

NEED FOR RESEARCH IN PRIMATOLOGY

HOOTON: I counted on these few moments to bring out a lot of detail and information about the primates which Dr. Straus had overlooked, but he is not the kind of fellow who overlooks anything. . . . His admirable paper, which summarizes our knowledge and our ignorance, would seem to me to lead to the possibility of some committee of physical anthropologists or some group of people plotting, in the light of this paper and in the light of their extensive knowledge, the main gaps in our knowledge of the infra-human primates, both fossil and living, and laying out a scheme of research which could be, perhaps, disseminated throughout the world to people who are interested in this subject, with the idea of having some of these subjects investigated. I have no doubt that a careful mapping of a study of primates, pointing out, as Dr. Straus does, what should be done and perhaps amplifying his remarks, might stimulate various institutions and individuals to undertake such research. The difficulty, of course, is that this interest is diffused; there is ordinarily very little coordination of the studies; and if we had some sort of central organizing committee willing to draw up a plan, outlining the things that should be done, and even making some effort to secure funds for the different primate studies, than I think we should get somewhere. . . .

We have a very promising beginning—and more—in the matter of the behavior of living infra-human primates, both in captivity and at large. We have the particularly interesting studies of Carpenter, to mention the work of but one individual; but those

studies have, so far as I know, been abandoned because of lack of interest, perhaps, in the scientific world in general and lack of support. It should be emphasized that work of the type that Carpenter undertook in the howler monkeys, the spider monkeys, the gibbon, and the macaque should be extended.

His admirable contributions are merely an instance of the need we have for encouraging isolated men in some such subject as psychology—perhaps in some institution where there is no general interest in the problems of physical anthropology—to go on with their work. That is what seems to me the greatest point that arises out of Dr. Straus's excellent paper.

EARLY HUMAN ORIGINS

STRAUS: . . . In my paper I dealt only briefly with the East African Early Miocene finds. But on the basis of the *Proconsul* material (which belongs to that noble dumping ground, the Dryopithecinae) Professor Le Gros Clark and Dr. Leakey suggested that here, at least, we have a theoretical possible common ancestor for the great apes and man. In one direction, from the limbs of the *Proconsul* type, you could get the limbs of the great apes, with a definite brachiating specialization, and, in the other direction, the limbs of bipedal man. . . .

With this theoretical possibility, I am in full agreement. We now know that at least some of the Dryopithecines had limb bones which more resembled those of quadrupedal money-like forms than do those of true anthropoid apes as we know them today. That, of course, as Professor Le Gros Clark points out, is no reason for refusing to regard them as primitive anthropoid apes, since the dentition has come to be accepted as critical evidence.

However, since discussing *Proconsul,* there are some things in Dr. Weinert's paper that I would like to note. I think it is unfortunate that he apparently felt it necessary to set up a straw man in *Proconsul,* in order to knock it down. Taking some newspaper reports, apparently, he assumes that Drs. Le Gros Clark and Leakey have decided or intimated that *Proconsul,* on the basis of the skull of the smaller species, *africanus,* is an ancestor of man—as he says, a specific ancestor of man.

To my knowledge, having thoroughly read the monograph by Le Gros Clark and Leakey, as well as earlier papers of Le Gros Clark, that is in no way claimed. As a matter of fact, these authors merely state that it is possible that some form like *Proconsul,* although, in all likelihood, not *Proconsul* itself, might have been the starting point: the road leading to man, on the one hand, and to the existing great apes, on the other.

Dr. Weinert would reject the *Proconsul* skull on the basis of its small size, which, from the point of view of what we know about evolution, strikes me as a rather strange procedure. He seems to lay a great deal of stress upon whether or not it may have had a frontal sinus. Of course, the frontal sinus in his scheme of summoprimates assumes a tremendous importance because he contends that the frontal sinus is a specialized condition. . . . He seems to have overlooked the very beautiful work of the late Viennese anatomist, Emil Zuckerkandl,[1] before the turn of the century, in which Zuckerkandl clearly pointed out that well-developed frontal sinuses occur in all the primitive mammalian forms, such as monotremes, marsupials, and many other forms; also the fact, which I have personally verified, that well-

[1] E. Zuckerkandl, *Das periphere Geruchsorgan der Saugetiere: Eine vergleichende anatomische Studie* (Stuttgart: F. Enke, 1887). Discusses paranasal sinuses of mammals.

developed frontal sinuses occur in many of the lemurs and quite regularly in all the New World monkeys except the most specialized ones, such as the woolly monkey, the spider monkey, and particularly the howler monkey.

In dealing with *Proconsul,* Professor Weinert again refers to his classification of some of the so-called "higher primates" into the group of "summoprimates."[3] Now the summoprimates of Dr. Weinert contain the gorilla, chimpanzee, and man, to the exclusion of the orangutan and the gibbon. . . . He indicates that the gorilla, chimpanzee, and man belong together as a genetic group in which the orangutan and gibbon can no longer be counted. . . .

In his book, *Ursprung der Menschheit,*[2] he brings together the evidence for which he creates the summoprimates, to the exclusion of the orangutan and the gibbon. In 1934, I pointed out that his supposed evidence of the kidney was totally incorrect and that his statements that human and chimpanzee kidneys exclusively resembled each other was not the case.[3] Two years later, in 1936, Dr. Adolph Schultz,[4] in a lengthy paper, refuted most of the points on which Professor Weinert had based his classification of summoprimates. Schultz took these characters one by one and revealed their invalidity—such points as the os centralis, the frontal sinus, and others upon which I cannot go into detail. Yet upon such points Dr. Weinert has based his conclusion that man evolved from a chimpanzee-like form at the end of the Pliocene or at the beginning of the Pleistocene.

At the end, in discussing *Proconsul,* after the summoprimates, he makes the definite statement that it cannot be regarded as a starting point of divergence of the apes and hominids, or as the beginning of the hominids themselves. I do not like such statements as "cannot" in paleontology. We are working always with probability. *Proconsul* already shows, as Le Gros Clark and Leakey point out, very distinct specialization of the teeth, which might be regarded as excluding this form from the direct line to man. But it certainly is conceivable that some such form might have represented a common ancestor. Certainly, Le Gros Clark and Leakey do not state that *Proconsul* must be the common ancestor. They merely point out that there is nothing which would debar it and that it meets the necessary specifications, in their opinion, of a common ancestor. . . .

WEINERT (Ackerknecht translating): I have reformulated my argument here in terms of drawings on the blackboard, where you see I postulate a group of summoprimates. Although Dr. Schultz has discussed this concept negatively, still he in a sense supports this because he ends up with this same picture, with the gorilla and the chimpanzee on one side of the genealogical tree and man coming up much earlier. . . .

As to the argument which Dr. Schultz based on the muscles of the

[2] H. Weinert, *Ursprung der Menschheit* (Stuttgart: F. Enke, 1932). Presents Weinert's theory of the "summoprimates" and the late separation of man and chimpanzee.

[3] W. L. Straus, Jr., "The Structure of the Primate Kidney," *Journal of Anatomy,* LXIX (London), 93–108. Includes refutation of Weinert's 1932 claim of the practical identity in structure between the kidneys of man and those of the chimpanzee.

[4] A. H. Schultz, "Characters Common to Higher Primates and Characters Specific for Man," *Quarterly Review of Biology,* II (1936), 259–83, 425–55. Shows Weinert's 1932 claims are incorrect concerning frontal sinuses, os centrale of wrist, coccygeal vertebrae, relative cranial capacity, os penis, kidney, and menstrual cycle. These are features which supply much of the foundation of Weinert's hypothesis. In conclusion Schultz states (p. 277): "Ample evidence has been discussed above to show that Weinert's widely accepted conclusions rest on many data which are at times insufficient and in part incorrect."

face, I may quote the late Hans Virchow—that is the son, not the father—who once told me that the face muscles of the chimpanzee are such that they could be drawn like a mask over the human skull and would fit in spite of the fact that the function of the muscles is different.

I have based my argument not exclusively on the sinus frontalis, but on many other characteristics. As far as the sinus frontalis is concerned, it is true that animals have a sinus frontalis, too, but it is not our human specific sinus frontalis, which is not even present in the embryo. The anthropoid and human embryos are all alike, and our specific human sinus frontalis develops only at puberty.

As far as the South African findings are concerned, I do not know really where to put them. They just do not fit into my version of the genealogical tree anywhere; I do not know, but other people do not know either.

STRAUS: . . . Dr. Weinert puts Dr. Schultz in a false light when he attributes the study of the facial muscles to him, and then says he reverses himself. The study of the facial muscles was done by the late Dr. Ernst Huber and not by Dr. Schultz, which gives a very different conclusion to Professor Wienert's interpretation.

As far as the sinuses are concerned, Dr. Schultz's paper contained a very clear picture of an orangutan with an enormous human-type frontal sinus, and it is well known that chimpanzees, for example, upon which Dr. Weinert leans so heavily in his argument, may have an enormously reduced or absent frontal sinus.

As far as the blood groups are concerned, Professor Weinert has used that as an argument, and I think Dr. Boyd will check me on this. The blood-group picture, to the best of my knowledge, most closely resembles that of man, not in the other two members of Professor Weinert's summoprimates, but in the two Asiatic anthropoids which he excludes—the orangutan and the gibbon. This is so much the case that somebody once said that if this is not accepted as evidence of close relationship—which it probably cannot be in this instance—one must assume that here is a case of parallel development. The blood-group argument of Professor Weinert is, therefore, as incorrect as is his argument about the kidney, the frontal sinus, the os centralis, etc.

BOYD (submitted in writing after session): It seems that the most recent studies on the blood groups of the anthropoids, which go a good deal further than the original work of Landsteiner and Miller, indicate (considering A, B, O; M, N; and Rh) that the chimpanzee is most closely related, in terms of blood groups, to man; and the gibbon, of the four anthropoids considered here, the least. The position of the orang and the gorilla is not yet sufficiently well studied for an opinion to be offered at the present time.

KOPPERS (Lowie translating): I am worried that, conceivably, specialists have not taken sufficient cognizance of immunological results, and particularly the question of the specificity of the albuminoid content. I would like to know to what extent those who have evolved or who have developed genealogical trees have taken into account this particular point of immunological relationships.

BOYD (submitted in writing after session): The early work of Nuttall on zoölogical relationships as determined by serological (precipitin) methods indicated, as I have pointed out, that the chimpanzee is more closely related to man than are the other anthropoids. Anthropologists took due notice of this finding at the time. Since then little attention seems to have been paid to the relatively small amount of subsequent work by this method.

Some of the neglect may be accounted for by unfamiliarity on the part of physical anthropologists with serological methods. But there are other reasons. To study the relationship of two species by the precipitin technique, one makes antibodies (usually in rabbits) against one protein of each species. Since different proteins of the body have different antigenic powers, the two proteins should be homologous (i.e., fulfil the same function in so far as we can determine). Nuttall used whole plasma or serum for his antigen. If he had used hemoglobin, he might have found results indicating different degrees of taxonomic relationship, for there is no law that states that the difference in the chemical structure of the plasma proteins of two species must be proportional to the degree of difference in the hemoglobins or other homologous proteins. Therefore, the question arises, Which is the most significant protein to choose as the antigen in such work? Or should one test *all* the proteins of each of the species to be compared, and strike some sort of average of the indicated relationships?

It was perhaps for reasons such as the above that Dr. Landsteiner never seemed too enthusiastic about substituting serological tests for morphological tests in taxonomy. He seemed to believe that the serological tests could make their contribution but that they would never replace the usual criteria.

WASHBURN: Throwing this back into a broader concept, it seems to me that this argument about the summoprimates is essentially the argument of African origin. These are the living African great apes, and, if a close similarity to man in these points is demonstrated, this is just the African argument in a different form. It is very close to the classic position of Schultz, Gregory, and Keith, it seems to me. If we subtract the reasons why they made their conclusions, the final arrangements of the primates are very similar. . . .

If we want to speak of brachiation, we have to make clear how we will determine brachiation in a fossil. Now this is not easy. We cannot see the fossils moving around; they may have used a variety of different forms of locomotion, as the South American spider monkey does. I would argue that only the shoulder joint will tell us whether or not an animal brachiated. This is the only place where the argument can be lodged.

I would further argue that if I believed in a brachiating origin of man, I would reconstruct the proximal end of the humerus of the East African form, just as Le Gros Clark did; in other words, reverse the implication and say that these forms were brachiators, in this sense.

I had a letter from Le Gros Clark just the other day, in which he pointed out that he now thinks it is quite possible that man is descended from an African pongid in the Pliocene. This is essentially, again, the same general position held by Gregory, Keith, and Professor Weinert. The point is that none of us at present can prove such a position, right or wrong. . . .

SENYUREK (submitted in writing after session): Dr. Weinert states: "For the whole continent of Africa, then, there is no sure proof for Neanderthal man if Rhodesian man does not belong to this group. It is strange that the entire west coast of Africa from the Cape to Morocco has produced no fossils thus far. The finds from North Africa and the Sahara are like the Upper Paleolithic people of Europe and need not be considered as a problem."

In this connection I would like to point out that fragmentary parts of Neanderthalers have been found in

Tangier (Senyürek) and Rabat (Vallois) in northwestern Africa.[5]

OAKLEY (submitted in writing after session): In this paper, Dr. Weinert throws doubt on the ages of the Swanscombe and Fontechevade skulls. Fortunately, the fluorine test has proved conclusively that these skulls are contemporary with the beds in which they were found. He suggests that the application of the fluorine test to the Piltdown skull showed that it is probably of Upper Paleolithic age. He should have said Upper Pleistocene, which is by no means synonymous. With regard to his datings of African fossil men, it seems to me that he is inconsistent in regarding the degree of mineralization of the Eyassi fragments as even a slight indication of their antiquity, when he denies this criterion completely in the case of the Kanam-Kanjera specimens. As far as I know, it has never been suggested that Eyassi man is "Magdalenian." The Eyassi fragments were associated with an industry of Late Levalloisian facies. Professor Weinert is scarcely justified in describing the Broken Hill skull as that of "a very early forerunner," now that it has been shown by Desmond Clark and others to be Late Pleistocene.

SENYUREK (statement revised after session): Dr. Straus is inclined to consider the similarities between some Platyrrhines and some Catarrhines as being perhaps of independent origin. In this connection, I would like to point out that the taurodontisms found in some members of the Hominoidea probably developed independently of one another.[6]

I will also refer to *Apidium* and *Oreopithecus* discussed by Dr. Straus in his inventory paper. Dr. Straus says: ". . . none of them exhibits the distinct bilophodontism of the molars that is characteristic of the Cercopithecidae. This specialized tendency toward extreme bilophodontism, it must be noted, seems to be the chief taxonomic criterion of a cercopithecoid. . . . The question arises as to whether a catarrhine primate can be regarded as a cercopithecoid in the absence of bilophodont molars, even if the rest of its body is truly cercopithecoid." In this connection I would like to point out that in *Apidium* and *Oreopithecus,* although the bilophodont character (crests between the cusps) is not yet completely developed in some lower molars, the distal cusps tend toward a transverse position and also the lower molars have some characters expected in the early cercopithecids. Thus I would place these two genera in the Cercopithecidae. . . .[7]

POSITION OF THE AUSTRALOPITHECINES

WEINERT (submitted in writing after session): Der grosse, 1949 von *Robinson* bei *Swartkrans* entdeckte Unterkiefer eines *Paranthropus crassidens* lässt wohl eine Deutung für die Entwicklungsrichtung der *Australopithecinen* zu. Ihr "menschliches" Gebiss war schon beim Taungskind Australopithecus africanus (Dart 1924) aufgefallen, obwohl der grosse I. Molar besonders überraschte und manche Autoren dazu verleitete, den Australopithecus als "Gorilla" zu erklären.

Die späteren Funde bestätigten wohl die Annahme eines menschlichen Gebisses bei den Südafrikanern; der riesige Unterkiefer von 1949 ermöglicht aber eine genauere Erklärung. In mei-

[5] M. S. Senyürek, *Fossil Man in Tangier* ("Papers of the Peabody Museum of American Archaeology and Ethnology, Harvard University," Vol. XVI, No. 3 [1940]).

[6] M. S. Senyürek, "Pulp Cavities of Molars in Primates," *American Journal of Physical Anthropology,* XXV (1939), 127.

[7] W. K. Gregory, *The Origin and Evolution of Human Dentition* (1922).

nem Inventory-paper ist die Beschreibung gegeben.

Alle 6 Vorderzähne (2 Canini und 4 Incisivi) nahmen kaum mehr Raum im Kiefer ein als die beiden Prämolaren. Die kleinen stiftförmigen Canini erfordern keine Diastemata; aber die mächtigen Backzahnreihen machen die Crista sagittalis auf manchen Paranthropus-Schädeln verständlich. Ebenso erklären die für solche Backenzahne nötigen grossen und dicken Temporalis-Muskeln die stets auffällig breiten, ausladenden und hochgeschwungenen Jochbögen. Das sind gemeinsame Merkmale bei Australopithecus, Plesianthropus und Paranthropus, die bei keinem anderen Anthropoiden oder Hominiden gefunden werden.

Nun ist das normale Anthropoiden-Gebiss (etwa wie beim Schimpansen) sicher auch die Ausgangsform für die Australopithecinen gewesen; also grosse Schneidezähne (Incisivi), Diastema, verhältnismässig grosser Caninus und ein caniniformer, vorderer Prämolar.

Diese Tendenz zeight auch noch der heutige Homo sapiens. Anders dagegen bei den Südafrikanern: hier werden die Incisivi und die Canini klein und stiftförmig; die Prämolaren werden vergrössert und bleiben molariform, während die Molaren selbst eigentlich besonders den Artnamen "crassidens" rechtfertigen.

Der Australopithecus von Taungs (1924) soll geologisch der älteste Fund sein. Bein ihm ist die spätere Form des "Crassidens-Gebisses" noch am wenigsten ausgeprägt; und so täuscht das Taungskind vielleicht eine Menschenähnlichkeit vor, die auch auf Parallelentwicklung beruhen kann.

Die geologisch jüngeren Formen des Plesi- und Paran-thropus setzen die Entwicklung fort, bis schliesslich der bisher grösste Unterkiefer von Swartkrans (1949) uns ein nie geahntes Extrem zeight, das wirklich *Paran*thropus ist. Man könnte sich vorstellen, dass eine Weiterführung dieser Entwicklungsrichtung schliesslich ganz reduziert, sodass eine Gebissformel 0. 0. 2. 3. entstanden wäre.

Die Stammbaumfrage möchte ich aber trotz dieser Erklärung nicht als sicher bezeichnen. Dass der grosse Swartkranskiefer einem Menschen-vorfahren gehört hätte, ist schwer zu glauben—aber deshalb doch nicht mit Sicherheit abzulehnen. Ältere Formen aus der Pliocen, wohin ja der Taungschädel eingeordnet wird, haben natürlich mehr Anwartschaft, zu unseren Vorfahren zu gehören als pleistocäne Funde, zu deren Zeit die Hominiden schon fraglos bestanden haben.

CULTURE AND THE AUSTRALOPITHECINES

OAKLEY: It seems to me that the smallness of the front teeth of the Australopithecines is an extremely important feature—the small incisors and the canines that you can scarcely see in that vast Meganthropus-like jaw which was found at Swartkrans. I think this must have a bearing on the ecology and the habits of life, and, surely, this is absolutely fundamental: that the bipedal, albeit imperfectly bipedal, gait of the Australopithecines had freed their hands. We have no evidence that they were tool-makers. Anatomically, they are probably Hominidae, but they are not human—an elementary distinction which does not seem to have been made sufficiently, because language developed in a prescientific era. When you try to translate "man" and "human" into scientific terms, you get into difficulties.

There has, as Dr. Straus said, been quite a ridiculous haggle about brain size, which is of no significance from the point of view of relationship. If there has been evolution in the way of increasing brain size, it must have started with a small brain; and when you are dealing with classification,

what you want to know is relationships, not what point of cranial capacity a particular group has reached. The brains may be as small as you like, and still the group will be classified as hominid, if the anatomy indicates that its relationship is with man and already removed from the point at which the modern great apes branch off.

To get back to this point of the small teeth and the bipedal gait, which, to my mind, go together, there is no evidence at all that the Australopithecines were tool-makers. But they may well have been changing their feeding habits. They are found in the veldt—dry arid grassland. The geological evidence is that the conditions under which they lived are the conditions of today, and they could only have lived under those conditions if their diet had been very different from that of the forest-bound great apes. They must have been much more omnivorous. I fancy that this is absolutely fundamental in the passage from the proto-man to man—this change of dietary habit. The environmental aspect does seem to favor the fringes of the savannah for the emergence of true man, and types which were capable of walking across open country and perhaps hunting in small packs would have been able to become semicarnivorous. Bipedalism, with the hands consequently freed from locomotion, would have improved their chances of survival.

Dr. Straus is quite right in his strictures on the interpretation of the social habits of the Australopithecines. Certainly, it is going much too far to say that we can attribute implement techniques to them. As far as I can see, one bit of microscopic smut has been used to create the specific name of *Australopithecus prometheus.* I cannot really see that there is much evidence that this form could conceivably have been a fire-user. . . . I think we should just leave it that, from the purely anatomical point of view, we have a clear indication that the front teeth were already being superseded by the use of hands and that this probably goes with alteration in dietary habits, as compared to those of typical anthropoid apes.

As regards the region in which this evolution took place, I quote from what Father Teilhard says, that the complete absence of finds of Early Paleolithic artifacts in the Lower Pleistocene of Asia is surely significant. . . . We have abundant evidence of the use of pebble tools in the Villafranchian, in Africa—deposits which just a few years ago would have been counted as Pliocene—and quite advanced implemental techniques in the Constantine region; for instance, chipped-stone balls have been found in Villafranchian deposits. I think "man the tool-maker" evidently arose in Africa at a very early stage.

After the Australopithecine level, presumably, we have something like a *Pithecanthropus,* so it is very remarkable that the Pithecanthropae that have been found associated with tools in Asia are Middle Pleistocene. One gets the impression that they are parahominids, as being on the fringes of the evolutionary story or holdovers from an earlier stage. Of course, you can get holdovers in Africa, too, and the Eyassi man may be one of them.

LINTON: You do not accept the evidence of the baboon skulls, the depressed fractures, as indicating the use of some sort of weapons by *Australopithecus?*

OAKLEY: The evidence from Taungs is very good and does, I think, indicate that these Australopithecines were eating monkey brains, but such evidence does not necessarily indicate the manufacture of a tool. Baboons have been seen to use pebbles in killing and opening up scorpions. Yet one does not have to call them implement-makers, because, possibly hunting in packs, they

have managed to corner and kill a baboon and then tear off the head with a sharp bone, perhaps, or bash a hole and excavate the brain and eat it—that is, within this tool-using stage which I referred to. The general and specific indications are that these Australopithecines were occasional tool-users, but I think it is going too far to describe them as implement-makers.

LINTON: I was referring simply to the skulls that you get, showing wide areas, with depressed fractures, at least with a pebble held in the hand or something of that sort.

OAKLEY: Yes, but whether it was used in killing or opening the brain or in excavating the brain is perhaps open to question. I do agree, however, that there are all these indications that they were semicarnivorous to a much greater extent than any anthropoid apes.

COON: One question about brain-eating. All these other forms that we find in Java, China, and elsewhere—when they ate the brain, they seem to have shoved a stick through the foramen magnum and then given it a big perforation, prying off the lower part of the skull to get at it. In these baboon skulls that you say these creatures ate, is that fracture at the base of the skull or is it up in the parietals?

OAKLEY: Frontal parietal.

COON: In other words, there is no evidence that they shoved the tool through the foramen and pried, as later brain-eaters did?

OAKLEY: No. It is a depressed fracture of the frontal parietal, and that is why I said it was uncertain whether the technique was one of killing or brain-extraction. I thought it might easily have been killing technique, because you cannot get at the base of the brain until the animal is dead.

Dr. Straus raised a point about the charcoal from Makapan. It has been suggested that the blackening of the bones was due to manganese. Actually, that is true. We had them at the British Museum, and all the blackness proved to be manganese; nevertheless, some patches of bituminous-like material were found. I cannot prove it, but I have a hunch it might be fossil guano, which can easily be mistaken for bitumen.

MCCOWN (submitted in writing after session): The sites in South Africa which yield Lower Paleolithic tools are numerous and their yield of tools enormous. Thus far no Australopithecine material has any known associations with the several varieties of Lower Paleolithic implements. The reasonable inference about these tools is that they represent cultural traditions, passed on by a learning process.

Yet there is a small body of phenomena associated with the Australopithecines from Taungs to Makapans, which suggests that something other than the hand and teeth of these manapes was used by them in hunting. The caches of baboon skulls with frontotemporal or frontoparietal depressed fractures is the most substantial part of this evidence.

A distinction of some usefulness can be made between tools, the products of learned behavior patterns, and implements, the products of individual initiative and invention but not transmitted to either contemporaries or descendants. If this distinction is used, the Australopithecines may be said to have been implement-using but not tool-using. This is without prejudice as to whether they were nontool-users or pretool-users. Popular usage of *tool* and *implement* in synonymous or overlapping ways poses some difficulty with respect to this suggestion, and the key consideration is that of deliberate transmission of behavior patterns, so that alternative words may be preferable.

In this latter connection one can

present the speculative corollary that absence of tool-making means absence of learning procedures and that these, in turn, depend on language; therefore the Australopithecines did not possess a system of communication sufficiently complex to serve for teaching a technique of stone tool chipping. Visual and tactile cues to complex behavioral activity cannot be ignored, but the empirical facts indicate that auditory cues have been and are of immense importance in all learned behavior and that such cues have greatest sustained impact when the flow of them is systematized in the form of language.

RACE AND GENETICS

WASHBURN: . . . Considering the time scale, it is conceivable that the human races, perhaps, have become races very much more rapidly than we formerly thought. If a Magdalenian skeleton looking very much like that of a modern human being were placed in the long chronology at 50,000 years ago, it would appear that European evolution had been going at an exceedingly slow rate. On the other hand, if the same skeleton be placed at 8,000 years ago, the process of evolution has been greatly speeded up. As the time scale is compressed, then, an appreciation of the process becomes more important. If the races were separate for 500,000 years, as they are shown to be in some of Sir Arthur Keith's diagrams, for example, there has been really no evolution in any recent sense. On the other hand, if the races were born during the last 20,000–25,000 years, there has been rapid evolution. This ties us, then, to the papers of Boyd and Carter, which offer us ways of interpreting this rapid evolution.

Paleopathology seems to me a most important subject, which has not received the attention it deserves. . . . Professor Hooton has done research on this, and one of his pupils, Dr. Leon Schuman, is now working on an analysis of the dentine of the teeth. Apparently it is possible to get some quite detailed information to show that an individual has been very sick or has had very poor diet during a particular time in his life. It might therefore be possible to go to an area like the Southwest and, taking a period which the archeologists regard as years of drought and famine (perhaps on the basis of tree rings), find that the frequency of these defects in the dentition is increased during this period of unfavorable climatic conditions. In that way, one could get at a kind of paleoecology" which would greatly enrich the subject of anthropology.

Perhaps the beginning of our paleoecology might be the study of a cave or a fissure in Kruger Park, where we are sure that hyenas brought all the bones. There we could see whether we could find baboon skulls approximating the forms which we now are attributing to *Australopithecus*. It is one thing to find a lot of bones in a fissure, and it is another thing to make sure what caused the pathology of the bones.

LÉVI-STRAUSS: . . . I take it that social organization is something probably very old, and I wonder if it would be of interest on the part of geneticists and physical anthropologists to study what would happen in a living group using definite rules in order to reproduce themselves, and what the result would be. We usually consider physical anthropology a kind of introduction to cultural anthropology, but the reverse possibility could lead to important results.

LINTON: May I cite one example that came directly under my own observation in one of the Madagascar tribes, the Tanala. There are two endogenous groups which are known, respectively, as the "red" people and the "black" people. In so far as I could make out . . . the two groups are practically

identical except that one of them is light enough so that you can see a flush in the skin, while the other group is a dark chocolate-brown.

In the red group there is the belief that, if a dark child is born of unquestionable clan ancestry, he will grow up to be a sorcerer, a leper, a thief, or guilty of incest; in other words, he is predestined to be a bad character; so they eliminate him promptly. Exactly the same belief is held with respect to light children who are born in the dark group.

There can be no question that you have here a marked change in pigmentation norms in these two groups, produced over a comparatively short period, because, by their own traditions, they have not been in this territory more than about twelve generations, which seems very short indeed to us.

SPOEHR: There seems to be a logical tie between the study of populations of living races and the possibility of the study of paleontological populations. This, in turn, raises the question of what we mean by variability with respect to a group living in a single period of time or, perhaps, with respect to a genus covering a greater period of time, and what the variability that we find actually signifies.

WASHBURN: As to the variability in fossil populations, I think the problem is, perhaps, best shown by the Australopithecines, where there is a large number of individuals from three different sites, a big time range, and now a very elaborate nomenclature being built up. . . . If, for a fossil group like this, that may have lasted over several hundred thousand years in time, one takes as a standard of variability any living species or population, one can find that the fossils are more variable or less variable than the group to which you compare them, according to which group you pick. The sum total of the Australopithecines is less variable than adult gorillas, females and males pooled, in many characters—dentition, crests, and so on. They are more variable than chimpanzees in the same characters. Therefore, the problem of variability in the fossils is extremely difficult.

If we want to concern ourselves more with problems of population and process, we should pay attention to the sort of questions that Dr. Oakley raised—what habits these animals had; what possible adaptive forces might have been leading to change; what kind of way of life they led—and far less to the question of detailed taxonomy of the group.

SPOEHR: I do not know whether the concept of races is any longer respectable. Would Professor Boyd throw the whole thing out in favor of, say, trying to set up a series of breeding isolates?

BOYD (statement revised after session): A race is a population, but not necessarily a breeding population. We may wish to separate man into large racial groups, perhaps corresponding roughly to the continents, but these races will not be breeding populations. Breeding populations are much smaller units and simply mean divisions of the population which freely exchange genes among themselves and more rarely with other individuals of the total population. A breeding population may even (temporarily, at least) be composed of several races, until such time as genetic equilibrium results.

One of the features not emphasized in this discussion is that races should correspond reasonably to geography. We tend to classify populations differing somewhat in certain gene frequencies as different races if they are (or were) more or less isolated from one another geographically. We do not go around the world picking out the red-headed individuals and asserting that they all belong to a distinct "red-headed" race.

Races may be defined as populations more or less reproductively isolated from one another (at least until recently) which differ in the frequency of one or more of the genes which they possess. The number of races we distinguish and the degree of difference we demand before we distinguish two populations as races will be to a certain extent arbitrary.

LOWIE: When Professor Boyd and others speak of the frequency of genes as being the determining factor in racial classification, I want to know what we really know about the frequency of genes? I gather that, in regard to blood tests, there is direct information, as there is a wealth of information on all kinds of morphological traits which have served in the past for purposes of racial classification. How are we to fit this into the new project of classification by frequency of genes? I am merely asking what is and what is not known at the present time.

TANNER: Very little indeed is known about the strict heredity of morphological characters, and this brings up the point that, so far as animals go, we have talked about primates to some extent—beasts which are difficult to breed and expensive to support in the laboratory; but, if we are to find out anything much about the heredity of physical characters, we shall have to work very much more with mammals of a somewhat lower order—dogs, rats, mice, and so on. . . . There have recently been efforts to deal with genetic characters which are not dependent solely on a single gene, as you see the blood groups are. . . .

The point that Dr. Lévi-Strauss brought up is exceedingly important. When, owing to cultural factors, the size of a breeding population has been reduced . . . the geneticist can tell approximately what some of the effects will be; that is, if he can tell what the reproductive system is and the number of the breeding population. . . . But he cannot say anything about the effect on such things as the cephalic index. This seems to me important, because it will tie up our two interests, which Dr. Straus characterized in his precirculated paper, and I think that essentially the nature and history of man is one of our interests.

Now, in order to tie these two together, what we want to know is, to a considerable extent, the genetics of those bits of man we find scattered around in prehistory, and these are not blood groups, by and large. They are bits and pieces of bone. It comes back again to what Dr. Washburn said on variability, that we really cannot say much about this variability until we know the modern factorial genetics of this situation.

In this regard, some of you will know the book fairly recently published by Professor Mather, called *Biometrical Genetics*, which is beginning gradually to feel its way into this. . . . We have not considered at this conference either the genetics or, indeed, what one could view as the transmission of culture in experimentally controlled animal societies. It reminds me of the situation in clinical medicine. One has the natural experiment made. I think that is what you do when you go to all these odd places and look at what is going on, just as we do in clinical medicine. But there are severe limitations. Nature does not produce these things just for the anthropologist's benefit, nor does it produce just the right thing.

Among animals, of course, one is faced with the difficulties of arguing by analogy, but, at the same time, with animals you can produce to some extent the cultures that you want. You can certainly produce the genetic situation you want without any trouble at all. . . . We need studies of inbred races of animals and, ultimately, of the results of growing and breeding inbred

races of such things as macaques and chimpanzees—which is going to cost a great deal of money but will be well worth it if the results are made available for social science.

BOYD (submitted in writing after session): Another reason that there are few genes of anthropological significance (aside from the blood groups) is that comparatively little effort has been put into the genetic analysis of morphological traits in human families. It is quite likely that, if sufficient effort were expended in this endeavor, many new unit characters in man could be discovered. But this is a line of investigation which has so far attracted relatively few investigators.

Even if this work were to be carried out, it is possible that the paleontologist might be disappointed. His chunks of bone have various distinguishing marks, such as knobs, hollows, holes, or sutures. It would be nice if we knew the genetic factors responsible for each of these. But bones, as Dr. Washburn often reminds us, have muscles attached to them during their development and maturation. The size, insertion, and action of these muscles are probably also determined by genes, probably by the combined action of many. The muscles influence the conformation of the bones markedly, as Washburn has demonstrated experimentally. Consequently, it may be that exact genetic analysis of bone characters will always be difficult or impossible, at least in the case of very old material where there are very few samples, and paleontologists will perforce continue to rely on the sort of intuitive comparisons which they have been in the habit of using.

CLASSIFICATION OF LOWER AND MIDDLE PALEOLITHIC ARTIFACTS

CLARK: I would like to speak first concerning . . . the Lower and Middle Paleolithic, the earlier part of the Old Stone Age. . . . Movius has recognized in his paper that the great bulk of the material [from the sites of this period constitutes] . . . the by-products in the manufacture of flint tools and implements. . . . He . . . suggests that we ought to turn a great deal more of our attention, perhaps, to trying to discover how these objects were made. . . .

How can we discover the techniques practiced by people who lived a very long time ago? Movius points to the possibility of recovering this information by experimentation, but I think, perhaps, he will not mind my inserting just a slight note of doubt. . . . The experimenters—Coutier, Leakey, Knowles, Charles McBurney, and so on—are undoubtedly doing very fine work. They are demonstrating ways in which forms, analogous to those we dig up from the soil, can be produced. The difficulty is . . . that when they have discovered, for example, how they can produce something like a burin or a hand-ax, they tend to assume that that is how prehistoric man produced it. . . . I think there is a fairly simple test of this, and that is to observe the experiments carried out by different enthusiasts, [for it makes us] realize that different people, using different techniques, can produce closely similar forms. . . . I therefore would suggest that the discovery and excavation of undisturbed workshops, the patient fitting-together of waste flakes, and the observation of the actual working tools on the site should be carried out in conjunction with these experiments.

Now a point that I would like to ask Hallam Movius to enlarge upon is the extent to which he thinks that our basic classification of Lower and Middle Paleolithic industry should be based on our knowledge, in so far as we are able to obtain it, of technique, and

what weight he would give in the classification to finished or secondary form.

MOVIUS: I agree absolutely with what Chairman Clark has just observed. The controlled experiments show only ways in which the forms can be produced, not necessarily how they were produced. . . .

I think a great deal of confusion has arisen [because the archeologist deals both] with the raw material and with an artifact. His terminology, as Dr. Clark so clearly stated, is being applied to different things and is certainly not adequate for both. I am not prepared to offer an alternative, but I think that, as work is done, we are going to come out with [a new] system of nomenclature. . . .

. . . We all agree that form is the main criterion on which we base our assessment of what term we are going to apply to a certain object, but in the form we have two levels: (1) the unretouched, unshaped, unsecondarily worked artifact, as it was originally struck from the raw material, and (2) . . . the finished artifact or utilized piece of stone. In applying terms to the latter, we have gotten it confused with the former. We have talked about techniques of manufacture when, in reality, we were talking about primary and secondary techniques of working stone, right in the same breath.

Perhaps if we can get at this question through careful excavation of undisturbed sites and really study the refuse material, we may be able to see more clearly what was going on at the first level. We know a great deal about what was going on at the second level, or at least we have a very imposing set of names that we apply to these various categories of tools; but what people were doing, under given conditions, with a certain kind of raw material is something we know almost nothing about as yet, except by inference.

This brings up a second problem, on which I admit complete defeat, and that is the influence of the raw material itself. Obviously, not only the shape of the material, as it occurs in nature, but also its size and inherent characteristics are going to play a very important part in whatever kind of technique is applicable to it in a given instance. We have tried to experiment with such widely different kinds of materials as fossil wood, silicified tuffs, silicified limestone, flint, and quartzite, using the instruments employed in mineralogical laboratories for testing siliceous rocks . . . [but have not obtained] any significant differences between these media. . . .

In various parts of western Europe, however, flint occurs in different forms, and there we find different techniques used on the same time horizon, a fact which . . . certainly is a reflection of the way the material occurred in nature. Beyond that, I have no idea where we go. Experimentation is not going to get us anywhere.

OAKLEY: . . . I am impressed by the extraordinary uniformity among Acheulean hand-axes in whatever materials they were made, whether they are of silcrete in Cape Province, of porphyritic lava or obsidian in Kenya, or of flint in the Somme and Thames valleys. Although in these different regions there are local, specialized types of hand-ax or cleaver which may perhaps have arisen out of the peculiar qualities of the local stones, nevertheless there does seem to be, for the most part, an extraordinary uniformity, and I think that has to be borne in mind in assessing the influence of raw material on technique.

FUNCTION OF STONE BALLS

LINDBLOM: . . . One item in Dr. Movius' paper interested me as an ethnologist. Speaking of stone balls found

in East Africa, he states "Since these normally occur in groups of twos or threes at the camp sites, they are regarded as bola weights for use in hunting."

I have never heard about the occurrence of bolas in Africa. . . . However, people in this region still use clubs with round stone heads; I collected such clubs myself over forty-five years ago. . . . Hence the balls are probably club heads rather than bola stones.

OAKLEY: I think there is an overready assumption that the stones in Kenya occur in groups. If you have a random distribution, it is very easy to find groups of three. My honest impression, having been to all the sites, is that you can see these groups only if you have the idea in advance.

The fact that similar stones have now turned up in the Late Villafranchian deposits in North Africa[8]—these are really the oldest known artifacts—suggests to me, at any rate, that stone balls must have served a very basic use, perhaps for pounding food or something like that, and that they do not represent some elaborate sophisticated implemental technique, such as is implied by the term "bolas."

DISTINCTION BETWEEN CLACTONIAN AND ACHEULEAN

OAKLEY: I am not sure that I agree entirely with the implication in Professor Movius' paper that Clactonian may not represent a culture separable from the Acheulean. To my way of looking at the evidence . . . a pure Clactonian and a pure Acheulean culture can be distinguished. It so happens that they are in superposition. In the Thames Valley the Clactonian comes before the Acheulean. But, if you go to other parts of northwestern Europe, you find that this relation is reversed. If two groups of animals fail to interbreed in contact, they are regarded by modern systematists as different species, even if you cannot see any differences between them. Using this as an analogy in Paleolithic archeology, I think one is justified in talking about those abstractions, the Clactonian people and the Acheulean people, provisionally at any rate, on the basis of the available evidence. . . .

It might be that there was some climatic, ecological factor which made the same group of people practice the Clactonian technique at one time and the Acheulean technique at another. That has been suggested by various people. It arose in the first place through a rather humorous circumstance. I seem to remember the Abbé Breuil once relating that M. Coutier was trying to use the wooden-baton technique for making Acheulean handaxes on a very cold winter day; he failed to get any results, and, in a fit of temper, he threw down the piece of flint. It struck another block, and a large Clactonian flake was the result of this accident. It is possible that under cold, damp conditions the Clactonian technique works better than the Acheulean. If you ask the Brandon flint knappers, they will tell you there are big differences between how flint will work under different conditions of humidity and temperature. It may well be that factors of that sort determined what techniques were practiced by the Lower Paleolithic peoples. Nevertheless, we cannot prove that; and until we know more, I think it is legitimate to . . . talk about "the Clactonians" and "the Acheuleans."

We have to make an attempt to interpret the actual objects that we find and see. The fact remains that you have a site and a layer with hundreds of thousands of implements which are one type; that one sort of technique was

[8] C. Arambourg, "Présentation d'objects énigmatiques provenant du Villafranchian d'Algérie," *Compt. rend. Société géologique de France,* No. 7 (1949), pp. 120–23.

used almost exclusively at that site; and that at another place in the same region another technique and other types of implement were being produced. I think that, for the time being, we should go on regarding those as separate cultures and perhaps (rather on the analogy of different noninterbreeding species) as representing different ecotypes of man.

The Clactonian is closely paralleled by the Hope Fountain industries found in central Africa (typically in southern Rhodesia). These consist of crude flakes and chopper tools. . . . At Olorgesailie in Kenya there is a series of occupation surfaces in Pleistocene lake beds. On most of the surfaces Acheulean hand-ax industries occur; but at certain levels they are replaced by a crude industry of the Hope Fountain type. This evidence suggests that there were peoples of two cultural traditions in the same region, each . . . resisting the ideas of the other group, perhaps each adapted to a different environment.

RELATION OF PHYSICAL TYPE AND CULTURE

OAKLEY: Another point I would like to make arises out of the interesting comment in the paper of Professor Movius that, from the point of view of intellectual capacity, there does not seem to be any reason to suppose that a given group of Paleolithic hunters would have been incapable of acquiring any of the tool-making traditions that came their way. . . . He suggests that geographical barriers were more likely to prevent a group from acquiring a given culture than that this was due to the members of the group having inferior brains.

I think one can support that point of view by pointing to the fact that, in a broad sense, the Levallois technique was used by at least three different species of man. *Homo rhodesiensis,* as we now know, was associated with a crude Levalloisian industry; the Palestine Neanderthaloids also practiced the Levallois technique; and so did the fully *Homo sapiens* types in the late Middle Stone Age of Africa. One cannot put a specific name to the Swanscombe man, but we now know that the Levallois technique, as Professor Movius points out, was, in fact, practiced by the Acheulean people. . . .

CLARK: This is a very thorny and difficult matter, but actually it coincides with the second [comment I wished to make on] Dr. Movius' paper. . . . From a purely cultural point of view, looking at the whole of prehistory, I have often been just a little disturbed at the very great prominence that has been given to the invention of farming. . . . This development is obviously of very great importance, but I think we ought not to overlook the staggering difference between the Upper Paleolithic culture as it is revealed in the caves of France and everything else which happened before.

Carbon 14 dating seems to suggest that the Upper Paleolithic developments came rather later than we thought, and this only heightens the impression of a very great speed-up in cultural development and differentiation. Not only do you have the appearance, very rapidly, of very diverse cultures, even in quite small areas, but you also have, as far as I can see, quite new spheres of human awareness opening up, with the appearance of cave art. After all, the cave art of France is something which you can set against the production of . . . any community that ever existed, and, in its own way, it is quite comparable. One feels that we have simply nothing on those people at all. They were subject to much greater economic limitations. Just think how few of them there were, what small groups they were living in, and what they produced; and one cannot

help wondering whether an advance of this kind, on this scale, is not to be correlated with a greater organization of the brain, whether this cultural change cannot be brought into some sort of relation with the facts of physical anthropology.

It is, after all, significant that wherever the blade and burin flints are found, whether in Africa, Europe, or western Asia, associated with various industries, the people concerned are always of a developed *sapiens* type—I suppose you can call it the Cro-Magnon type. Conversely, wherever Cro-Magnon men have been found associated with cultural material, this cultural material, as far as I know, always belongs to the blade and burin group.

MEAD (submitted in writing after session): More serious consideration needs to be given to the possible existence, simultaneously or antecedent to the elaborate cave paintings, of comparable art in a more perishable form, such as that found in bark painting, sand painting, feather mosaic, etc., in Australia. When this possibility is carefully considered, cave painting will not appear to represent such a great discontinuity in cultural level [as Dr. Clark suggests].

MCCOWN: The point that Dr. Clark has raised is an interesting one, because you certainly do find a consistent association between what he has described as the blade and burin technique and what we know of the development of *Homo sapiens.* In certain situations, however, which are not numerous but are very puzzling, you find something that looks like a blade and burin tradition associated with a more numerous element of artifacts which clearly represent other traditions, and in none of those situations have we yet been able to determine what kind of hominid was involved. I have in mind, naturally, the situation with which I am best acquainted, the occurrence of the burin and quite unmistakable blades associated with what Miss Garrod called the late or upper Acheulean of Palestine. We know something about the population or people who came later in time in that particular situation, and they clearly could not, I think, even by any liberal interpretation of Dr. Clark's definition, be regarded as merely a characteristic kind of *Homo sapiens;* so that we are again faced with a problem. . . .

In general, I think that Dr. Oakley's point, that it is clearly premature to attempt to associate in any over-all way a particular kind of hominid of the Upper Pleistocene with particular kinds of traditions, tool work, etc., is well taken and should be more widely regarded and put into practice than it frequently is.

ORIGIN OF THE UPPER PALEOLITHIC

CLARK: When I was listening to Dr. Teilhard de Chardin's discussion earlier this morning . . . one of the things which struck me was the very great emphasis which he laid on the role of Africa not only in the early emergence of man but also specifically in the emergence of *Homo sapiens.* Then, in reading Dr. Movius' paper, I thought one of the great points he made . . . is that the blade and burin culture hardly affected Africa at all. It affected it marginally and late. From the point of view of the Upper Paleolithic world as first revealed in western Europe Africa was indeed a dark continent. It was a place where multitudes of hand axes were made and where the Levalloisian tradition existed right down to Neolithic times. . . . That, I feel, does open up a problem.

MOVIUS: Dr. Clark has pointed out what seems to me to be a very real dilemma. Practically speaking, the blade-burin complex is absent from the Pleistocene of Africa. In central Asia it seems to be likewise absent, on the

basis of the reconnaissance work that the Russians have been doing there; and in Palestine, if our tentative chronology is correct, it is late. That leaves us right where we started. [Perhaps] we do not have to go outside Europe to study its origin. . . .

DATING THE UPPER PALEOLITHIC

McCOWN: The problem of dating the Upper Paleolithic in Palestine is one which comes back to Dr. Oakley's background paper of yesterday, and it is crucial to an undertanding of the relationships that took place during the Upper Pleistocene, either on a cultural level or in terms of the differentiation of hominids. In Dr. Movius' abstract,[9] he says: "Historical problems in the development of early human cultures can be understood only after a reliable time scale has been established." I can substitute for that, or add to it, the phrase "development of hominidae," because again and again we assign to our human fossils some sort of an R-date which has very largely been compounded out of the apparent association of these hominid fossils with stone tools.

This has a particular bearing . . . on the situation in which we cannot date our human fossils with any degree of precision and are struggling to know how to arrange them and how to understand the kind and rates of differentiation that were going on during the Upper Pleistocene. . . . For example, I would like to point out that the date for the Levalloisian skeletons at Mount Carmel is one which was predicated on a good R.1 and R.2 relationship of human fossils with certain Pleistocene mammals. But the A-date for these skeletons falls into Dr. Oakley's class 4, I think, in which we have to use the Pleistocene mammals of Palestine to give us some sort of means of interdigitating these phenomena with the Pleistocene faunal sequences that are either known or reasonably well established for Europe.

The official statement with regard to the date of the Palestinian Neanderthals is that they are very late Third Interglacial. We based this on their association with certain changes in the mammal fauna which took place, according to our very distinguished paleontologist, the late Dorothea Bate, at the commencement of the Würmian glacial. Professor Movius has been going into this problem in great detail, and he has argued that the actual time at which these fossils lived . . . was the interstadial between Würm I and Würm II.

At the present time, I think that the total amount of concrete evidence makes it uncertain which one of these views is the most true, but . . . I think Dr. Movius' arguments are very persuasive. I am not quite prepared to accept them yet, but I would like to underline the fact that, if the Palestinian Neanderthals are as late as Würm I or II, then Professor Vallois's conception of a hiatus between the earlier and later portions of the Upper Pleistocene population is . . . simply an accident, consequent upon our inability to date either the cultural or the human materials with any real degree of precision. . . .

COMPARISON OF OLD AND NEW WORLD CHRONOLOGIES

KROEBER: It struck me, as I listened to yesterday's papers and this morning's and thought ahead to some of the ones which are coming up this afternoon, that a very definite difference is developing with regard to views about chronology in the Old and the New Worlds. In the Old World, the tendency is all to shorten chronology.

[9] Abstracts of all the inventory papers (usually, as in this case, written by the author) were available at the symposium. They are not published.

Milankovitch's chronology was questioned; a lot of the fossils that we thought were old, fluorine is making quite young; and so on. In America, however, where we got to work with no serious help from geologists and for a long time had to stand on our own feet, we archeologists and anthropologists began very conservatively, with a short chronology . . . and the tendency since we have obtained carbon 14 has very definitely been to lengthen it. This is merely an accident of circumstances, but it is perhaps noteworthy that, in order to balance up the Old World with the New, we are making the Old World younger and the New World older.

DEFINITION OF THE NEOLITHIC

CLARK: . . . The Old World Neolithic is an enormous subject, and, although Professor Childe was not able to be here, he has, I think, given us a very fully documented inventory paper. . . . I would like to bring out one very general point. Childe has shown that we have quite abandoned . . . any attempt to define the Neolithic in terms of material objects. . . . We are now much more concerned with the basic economy on which the societies which produced [these objects] depended. . . . It is, of course, the origin and spread of farming that marks the beginning of the Neolithic chapter in human history. The chapter is closed . . . by the invention and diffusion of metallurgical industry, which could not be practiced to any extent except on the basis of specialization and trade; whereas our Old World Neolithic people were very largely self-sufficient farmers.

FORDE: With regard to the classification of the major phases of cultural development, I think we are moving toward, but have not yet reached, the point that exists in physical anthropology. In our earlier discussion this morning, the point was made that . . . one has to deal with actual population and gene frequencies in the population, although we cannot yet do the job . . . because our knowledge of genetics is inadequate.

Now Professor Childe keeps the word "Neolithic," but he has taken the "lithic" out of its definition. My point is . . . that we shall have to go to a still higher level of abstraction. Actually, the terms "Paleolithic," "Neolithic," and "urban civilization" are not on the same level of discourse. Farming and urbanity are not the only way in which you can get a sessile population with a surplus of labor. Some nonagricultural peoples, for example, on the Northwest Coast, were fully as stable from the point of view of food supply, fully as numerous from the point of view of society, as any of the Neolithic populations of the ancient East; and I imagine the same situation may very well have held in parts of Latin America. It so happens that these nonagricultural, sessile populations with surplus labor for specialization, the development of fairly high technology, art, etc., have not proceeded to metallurgy, but I regard that more as an accident in culture history than as something that is implicit logically.

CLARK: If the chairman might just elaborate on that point, it is one on which he feels very great personal agreement. Anyone who has followed the work done by Gjessing,[10] for example, in the circumpolar zones of Europe will recall that he is dealing with people who were beyond the possible range of farming in early times. Yet you have there, particularly in the coastal regions of northwestern Norway, fully sessile populations living in very substantial houses, using pottery, polishing flints, and so on. They were

[10] Gutorm Gjessing, "Circumpolar Stone Age," *Acta Arctica,* Fasc. 2 (Copenhagen, 1943).

not growing crops because it was not biologically possible to do so with the grains they had. . . . They were living by hunting sea mammals and fishing. . . .

I would agree with Forde that our definitions really ought to take account of all such variations. The circumpolar group may not have been numerically very large in relation to the temperate peoples of Europe, but its density, of course, was quite as great.

CULTURE AREAS IN OLD WORLD ARCHEOLOGY

CLARK: Our conception of the development of a new form of economy and its spread seems to me to call for some comment in the light of the remark in the paper of Irving Rouse, in which he claimed that the culture-area concept was . . . "more applicable to ethnology than to archeology." I would rather question that, because, although we do not know very much about the culture of prehistoric times, we do know the outlines of its development over very long periods of time. I had thought that people like Professor Childe, who comprehend the main history of sequence development in western Asia and, to a lesser extent, in the Nile Valley and are able to trace the spread of successive forms of economy and the material culture based on them over the Mediterranean and temperate Europe—that these people are, in effect, showing how areas of culture developed in time. . . .

One aspect of the carbon 14 research in the Old World which did not come up for discussion yesterday is its use for dating earlier appearances of farming. One thinks, of course, of the date for Jarmo, etc.[11] It so happens that most of the farming peoples have left behind them material which is particularly suitable for dating by carbon 14 methods; and so we should be able to secure dates not only for the kernel areas, where you have the first occurrence of the new form of economy, but also for its spread or gradual diffusion outward, in terms of the various contours which Childe produces in the latest edition of *The Dawn of European Civilization.*[12] With these time values, we could really make a film, as has been done, showing how the spreads occur.

ROUSE (statement revised after session): The only reason I suggested that culture areas can be more easily established in ethnology than in archeology is that ethnological material tends to be more reliably dated than archeological (except, of course, when some inferential technique, such as the age-area concept, has to be used). When and if archeological areas can be accurately dated through carbon 14 analysis, as Dr. Clark suggests, I agree that they will be equally as good as ethnological areas.

ACCULTURATION IN OLD WORLD ARCHEOLOGY

CLARK: . . . A second point which harks back to Irving Rouse is the question of acculturation. I have said that we bring our Neolithic to an end with the development in a restricted area and with the spread over much more extensive areas of the practice of metallurgy—in most cases the working of bronze or copper. Now, Rouse has said quite justly that the ethnologists are very preoccupied with problems of acculturation, and he suggested that archeologists and students of history, on the other hand, deal more often with a mutual interplay of cultures on a more equal basis.

Well, of course, I would agree that

[11] C. B. M. McBurney, "Radiocarbon Dating Results from the Old World," *Antiquity,* No. 101 (Newbury, 1952), p. 39.

[12] V. Gordon Childe, *The Dawn of European Civilization* (4th ed.; London, 1947), maps I–IV.

. . . there is a much more equal basis. But still I think that possibly Rouse's remark might slightly obscure the fact that the main motif in the study of the later stages of European prehistory is precisely the effect which the highly developed civilizations—first, the Bronze Age civilizations in western Asia and the Nile Valley; later, those of the eastern Mediterranean; and still later, the civilizations of the iron-using Greeks and Romans—had on the much simpler barbarian peoples with whom they came into contact. . . .

There were very major differences between these urbanized literate civilizations of antiquity and the barbarians outside. I would have said, for example, that the spread of metallurgy during the so-called "Bronze Age" in Europe really reflects the rise of the original Bronze Age civilization and the gradual expansion of its economy rather than the development or evolution, as it were, of the European peasant peoples. In the same way, phenomena like the development of La Tène art were quite obscure until classical archeologists started to study them.

ROUSE: In discussing acculturation, I was, perhaps, overly influenced by my New World experience. . . . In so far as I know the prehistory of the New World, I do not believe that we have the type of spread of civilization from one or more centers which has been described by Dr. Clark for the Old World. We can establish the diffusion of certain elements of culture, such as agriculture and pottery—or at least we assume such diffusion—but we do not have the situation in which urban civilization, advanced metallurgy, a written language, etc., spread from a center to surrounding areas.

I suspect that the American Indian was approaching this situation at the time of the discovery of the New World, with the rise of the Aztec and the Inca. Nevertheless, conditions at that time seem to me to have been more similar to those in the Old World during the first half of the Bronze Age. The type of development you get in the latter half of the Bronze Age and the Iron Age, when the civilizations of the Near East spread to the so-called "barbarians" in the north, is not, so far as I read the evidence, duplicated in the New World.

SOUTHEAST ASIA AS A SEPARATE NEOLITHIC CENTER

LINTON: One thing that has impressed me very much about all Dr. Childe's work is his complete focus on southwestern Asia. I think anyone who has worked in the archeology of Oceania and the Far East recognizes that a distinct center of agriculture developed during the Neolithic in the monsoon region, extending from perhaps central India eastward to what is now Indonesia and Southeast Asia.

This was the region in which agriculture was based upon root and fruit crops instead of on seed grasses. The loom apparently was not developed here, but, instead, bark cloth took its place. The existence of pottery is certainly questionable at the beginning of the period, its place being taken by the earth oven and bamboo boiling. Domestication of animals was economically insignificant, the pig being mainly a prestige animal, and so on down the line. We had a Neolithic center here which also became a center of diffusion in the directions set by the ecology: southward and out into the Pacific, where the particular plants and animals could be grown successfully.

CANALS FRAU (submitted in writing after session, referring to Childe's paper): Me parece un trabajo excelente, y apruebo en general todas sus conclusiones. No obstante, quisiera dejar sentado que el hecho de que aunque la mayoría de datos actualmente a

nuestra disposición muestre que la cultura neolítica, en su forma europea y norteafricana, ha de haber surgido en el Cercano Oriente, en "alguna parte entre el Nilo y el Jaxartes" como muy bien dice el Dr. Childe, de ello no podemos deducir que en otra parte de la Tierra, por ejemplo, en el Sudeste de Asia, no hayan existido ya con anterioridad formas culturales basadas en el cultivo del suelo, aunque las plantas allí cultivadas sean otras que el trigo o la cebada.

Son varios los datos que señalan la posibilidad de que el cultivo de la tierra se haya originado en alguna región del Sudeste asiático. Y el hecho senalado por el mismo Childe, de que por ahora dispongamos de muy escasos datos seguros sobre el particular, nada prejuzga en contra de la posibilidad mencionada.

CHAPTER IV

Problems of the Historical Approach: Results

Chairmen: Alfred L. Kroeber and Pierre Teilhard de Chardin.

Papers discussed: Krieger, "New World Culture History: Anglo-American";* Bennett, "New World Culture History: South America";* Caso, "New World Cultural History: Middle America";* Greenberg, "Historical Linguistics and Unwritten Languages";* Schapiro, "Style."*

Speakers: Teilhard, Kidder,† Rouse, Kroeber,† Lowie, Eiseley, Bennett, Caso, Koppers, Dias, Eggan, Willey, Beals, Murdock, Rowe, Birket-Smith, Haas,† Held, Voegelin, Heizer, Greenberg, Lévi-Strauss, Mead, Schapiro.

SIGNIFICANCE OF AMERICAN CULTURE HISTORY

TEILHARD: I wish to emphasize what, to my mind, ought to be the specific contribution of American anthropologists to general science. In the case of vertebrate evolution, few facts are more illuminating than the particular and self-sufficient development undergone during Cenozoic times by a group of early Tertiary mammals accidentally trapped in South America. In the case of this limited and isolated pulsation of life, some of the most general laws of general evolution become exceedingly clear.

Similarly, if American prehistorians do not share the privilege of their eastern colleagues in studying the very first traces of man on earth, they have, as a compensation, exceptional opportunity for studying, at close range, the semi-autonomous neo-development, over a large and closed continent, of a transplanted fragment of Mesolithic humanity. Some general and fundamental laws in human development are certainly hiding there. They have to be discovered.

Not only a detailed analysis of the various cultures of North, Central, and South America but a synthetic view of the expansion and differentiation of "New World man" (in comparison and in contrast with "Old World man's" birth and rise) is ultimately what we expect from American anthropologists, in order better to understand, in their most fundamental processes, the springs and wheels of human evolution.

KIDDER: New World anthropology . . . provides materials for attack upon such fundamental problems as the ability of man to repeat basic inventions and to give us information as to the rise, florescence, and decline of cultures under conditions that were apparently much more stable . . . than was the case in the Old World. That was due, I think, particularly in later periods, to the fact that no riding animal was brought under domestication in the New World, which inhibited the great sweeps that slowed up the development and often led to the fall of Old World cultures.

ROUSE (submitted in writing after session): Another reason for the rela-

tively greater stability of New World civilization may have been its failure to develop adequate sea transportation, such as that which led to expansion of the Minoan and Phoenician civilizations in the Old World.

SENSE OF PROBLEM IN AMERICAN ANTHROPOLOGY

KIDDER: Mr. Krieger feels that American anthropology, or North American archeology at least, has failed to reach a point which has been attained by other sciences. He quotes Aldous Huxley in that regard. But I think that both he and Huxley fail to take into account the fact that we do not have behind us nearly a century of taxonomic arrangement which has given the practitioners of other disciplines the groundwork for arriving at syntheses. . . . However, there is no question, as Dr. Bennett also brings out, that we need a sense of problem above almost all things. . . .

KROEBER: . . . I think it is true that there was for a long time a lack of sense of problem in American archeology and, more than that, a lack of courage. I think back to the time, say, of the first World War, when Kidder, Nelson, Spier, and I in the Southwest and Gamio, Boas, and Spinden in Mexico were thinking . . . in terms of sequences. . . . Up to then, it is astonishing how, in North American archeology, the past was viewed completely flat. . . . I suppose that that was in a sense, perhaps, a reflection of the fact that this country as a nation has so little past and has prided itself on the present. But, at any rate, it is a strange sort of thing for archeology, devoted to the sequence of time, to grow out of. . . .

ANTIQUITY OF MAN IN THE NEW WORLD

KIDDER: I have thought that . . . it might be a good idea briefly to outline some of what seem to me, at least, the most important problems of New World prehistory. . . . The first, which is taken up by each of the authors but particularly by Mr. Krieger, is that which concerns the coming of man and his diffusion throughout the continents. I have always wanted to have more chronological elbow room for American developments than used to be the case in Holmes's and Hrdlička's days. Krieger goes cautiously—if one can stick one's neck out cautiously—for a considerably greater possible age for the coming of man than is usually accepted, even now when we are much more liberal with our datings.

LOWIE: I am reminded that the geographer Penck[1] dealt with the spread of man into the New World from the point of view of adaptability to different climatic zones. His position is especially significant because he postulated a number of millennia for each adaptation to a major zone from Bering Strait to Tierra del Fuego, arriving at some such figure as 25,000 years for the duration of the total journey. . . .

EISELEY (submitted in writing after session): If we survey the difficult adjustments necessary in entering so many zones demanding differences in food-getting habits and long adjustment to quite distinct faunas, great time periods would indeed seem involved. The obstacles to rapid movement south of the Rio Grande appear formidable.

However, a suggestion can be made as to how this barrier was overriden with reasonable rapidity. We all know that at the close of the Pleistocene (now being lifted upward in time) there was a far-reaching decimation of the large mammalian fauna of the New World. Camels, elephants, horses, and several antelopes, among other forms, became extinct. This, particu-

[1] Albrecht Penck, "Wann kamen die Indianer nach Nordamerika?" *Proceedings of the Twenty-Third International Congress of Americanists* (New York, 1930), pp. 23–30

larly in Middle and South America, enormously impoverished the Holocene fauna and made the movement of hunters relying upon a far-ranging fauna much more difficult, if not impossible. In other words, the adaptations to survival *had* to be of a far more local character.

On the other hand, if we assume that man, in the hunting stage of culture, passed southward in the Late Pleistocene, possibly down the highlands, when grass, water, and big game were more abundant than today, Penck's obstacles appear far less formidable. A hunting pattern based primarily on big game could have carried man to southern South America without the necessity at that time of great localized adaptation. It could have been effected with relative rapidity, so long as camel, horse, sloth, and elephant were available. All the indications point to the fact that they were.

ORIGIN OF MAIZE

KIDDER: The second great problem is certainly the rise of New World civilization. . . . It has always been assumed that this was dependent upon agriculture and, as maize was . . . the staple crop throughout practically all the New World which had reached a stage that one could call "civilization," the problem of its place and time of origin is of outstanding importance.

Of recent years there have been . . . a number of discoveries which alter some of our previous opinions as to the life-history of maize. First among these was the discovery of a very primitive form of maize in Bat Cave in New Mexico by Herbert Dick. . . . This discovery makes it very probable, to my mind, that the place of origin of maize was not in the lowlands of South America, as was postulated by Mangelsdorf, but somewhere in Central America, possibly in Guatemala. There may be a little chauvinism in that, because I am a Guatemalan and I know that it would please Morley. . . . But the point is, would it have been likely for such a relatively inferior cereal as Bat Cave corn to have been passed on all the way from somewhere in the lowlands of South America, Paraguay for instance, to New Mexico, at such an early date? A second, much shorter journey, and certainly one with much better . . . diffusion possibilities would have been between Central America and the Southwest.

Another very important point is that the deposits worked by Junius Bird on the coast of South America indicate that there was a strong development of agriculture before the introduction of ceramics and also before the appearance of maize. If a plant as valuable as maize had existed anywhere in South America, it seems to me it would presumably have reached the people who laid down the deposits worked by Bird.

BENNETT: . . . Most of the habitable regions in the high Andes of Peru today are above the growing limits of maize, and consequently the root plants —potatoes, oca, and a few others—are and always have been, as far as we know, the basic crops. I wonder how much effect this might have had upon the coastal people. It might well mean that, even if the origin of maize were in Paraguay, it would spread up the montaña side of the Andes and not cut across to the coast for some time.

CASO (Métraux translating): I would like to quote an article which appeared in *Science News Letter* of May 24, 1952, about corn pollen which was found in the city of Mexico and may be, according to estimates, 25,000 years old. Of course, this does not mean that the cultivation of maize was known at that time, but simply that maize may have existed at a period in the valley of Mexico which precludes its importation from South America.

TRANSPACIFIC INFLUENCES[2]

KIDDER: A third great problem, which has recently been brought to the fore by Heine-Geldern and Ekholm,[3] is the vital question of the amount of Eurasian influence which reached the New World and what effect it might have had upon the growth of New World civilizations. It is awfully hard to get over one's prejudices. . . . I always say that this is an open problem and that much work should be done on it, but I think I still remain 100 per cent American in the matter.

[This question], of course, bears upon the problem of diffusion as against independent invention. I think another prejudice I have is against [extreme assumptions of] diffusion. . . . It seems to me illogical to hold that if human beings can make an invention once they cannot make it twice. There are so many examples . . . of duplicate inventions which can be definitely shown to have been made in entire independence.

KOPPERS (Kroeber translating): . . . In my work in India I discovered maize entering into ritual relations in a way that is not likely to happen with a recently introduced novel food plant. Not that that proves that Indians had maize from time immemorial, but it suggests that, if it really was introduced there after the seventeenth century, something unusual has happened to make it take a more important place.

In Indonesia there is another case where maize is coupled with rice on terms, as it were, of equality. . . . Again, I am not arguing in any way against the American origin of maize, but that these Asiatic phenomena will have to be accounted for. I would suggest the possibility that maize may be of American origin but have reached Asia in pre-Spanish and Portuguese days.

DIAS: In Portugal, we also have maize with old traditions, [but this is] because maize has been substituted for millet. So it may be that [in India and Indonesia a new plant] came in substitution for an older one. . . . Only the botanists can resolve this question, I think.

KROEBER: As to the matter of the Eurasian influence on the history of the New World . . . I am an Americanist and believe that the great part of the contents of native American culture and many of the styles originated in this hemisphere. I think, however, . . . that a certain trickle, an intermittent flow, or perhaps sporadically a bigger flow, came in various ways, trans-Pacific, over Bering Strait—we do not know how. I would prefer to declare myself a 90 per cent chauvinist in Americanism.

RELATIONS BETWEEN NORTH AND SOUTH AMERICA

KIDDER: A fourth great problem is that of the relations between North and South America. . . . It is obvious that there were contacts between the two continents. Nobody believes that maize originated twice, and so it must have been transferred from one to the other. And there is also the question of contacts and relations between the circum-Caribbean area and the Amazon, on the one hand, and Mesoamerica with the Southeast, on the other. Then we have the extraordinary parallelisms in development that took place between the high cultures of the Andes and Mesoamerica, in spite of the fact that direct contacts between the two areas have not been clearly demonstrated. Of

[2] For further discussion of this problem see section on "Diffusion and the Age-Area Concept" in chap. v and section on "Basic Stages of Cultural Evolution" in chap. xiv.

[3] Robert Heine-Geldern and Gordon F. Ekholm, "Significant Parallels in the Symbolic Arts of South Asia and Middle America," *The Civilizations of Ancient America*, ed. Sol Tax (Chicago, 1951), pp. 299–309.

course, the region which must give us the answer to that question is lower Central America, a region which is archeologically almost virgin, particularly on the earlier horizons, where fundamental contacts or diffusion should have taken place in one direction or the other; and for that reason I have been particularly pleased by the fact that Dr. Willey is tackling that problem in explorations in Panama. Those, of course, should be carried . . . into northern South America and up into lower Central America.

There is one very interesting matter: the apparently strong Mesoamerican influences that turn up on the northwest coast of South America, particularly in Ecuador, where there are spindle whorls . . . which closely resemble those of Chichen Itza. Dr. Bennett tells me [that this is the only place in South America where] the obsidian flake blade is found; and also there is a strong resemblance between the clay figurines of that region and those of Mesoamerica.

EGGAN: I notice [that Dr. Kidder has used] the term "parallel development." . . . Isn't the term, rather, "divergent development"? At least, my reading of the literature suggests that the developments are quite diverse in terms of emphasis, quantity, and quality. . . .

WILLEY: . . . I think the parallelism is in the type of development. There is great diversity in content of culture, but both Middle America and the Andes are characterized by a formative horizon, which in each area has a period of uniformity followed by great regional diversity, by a stage of florescence, and by a third major stage which is characterized by a certain amount of militarism and, apparently, interest in politics.

CASO (Métraux translating): In regard to the problem of the interrelations between Mesoamerica and South America, it would be erroneous to set up a table listing all the common traits and opposing them to traits in the region more to the north. The parallels are really few, and, after all, we must realize that we are dealing with two regions which have reached a similarly high level of culture. It is quite natural that parallels should [appear because of the level].

STAGES OF DEVELOPMENT IN MESOAMERICA

KIDDER: Passing north into Mesoamerica, we fail to have as yet any evidence of what Dr. Caso, in his inventory paper, calls the "Primitive Stage," i.e., the stage when people were passing from a pre-ceramic and pre-agricultural existence to a farming life. . . . I think probably both transitional or primitive and very early formative remains must exist in one form or another in Middle America; and the reason why we have not found them is merely because it is such a vast area, so much of it is very unfavorable for surface examination, and such few regions have really been adequately explored. . . . Aside from the valleys of Mexico and Guatemala and adjoining valleys, almost no work has been done in sites or areas which might be expected to turn up remains of this most important early phase, I think largely because the attention of archeologists has been overly diverted, as has often been said, by the spectacular remains of the later periods. . . .

Dr. Caso . . . [has made] what I consider a very helpful [contribution to our] conception of the various stages of Mesoamerican development, in that he has split what used to be known as the "Archaic" into one stage which he still allows to be called "Archaic" and another which he calls "Formative." . . . It has become more and more obvious of recent years that the Archaic, Pre-Classic development throughout Mesoamerica has two very well-marked

phases: one with no sign—at least, none yet discovered—of great public works, and therefore presumably indicative of a fairly unstratified society; and the later Pre-Classic or Formative, during which time there were erected great temples and other public works, which, I should think, . . . were very probably the product of an organization which could call for the efforts of great numbers of people—a form of society which has usually been supposed to have been theocratic. . . .

The next stage in our present arrangement of the periods of development in the Mesoamerican area has usually been called "Classic.". . . This period . . . saw the growth of various high cultures, which, although they had many similarities, were in many details quite different.

[As to the] conditions which permitted the rise of the Classic civilizations in Middle America, I am inclined to think that it may have been due to what I spoke of a few minutes ago, namely, the lack of a riding animal, which precluded the mobility of pastoral peoples. Therefore, the classic civilizations were free from the inroads of such peoples, which it seems to me time and again probably slowed up, and in many cases destroyed, the orderly development of high culture in the Old World. . . . Another thing was the possession of maize, which was adaptable to cultivation in a great variety of environments, so that the people could spread out; and I think the pressure of population came relatively slowly in the New World. Although the Indians were not, as a whole, subject to the more virulent types of epidemic diseases, nevertheless mortality, particularly of infants, was very high, and the lack of sanitation, pulmonary troubles, etc., must have served as brakes on the development of very large and crowded populations.

Then came, at the close of the Classic period, a time of stress and tribulation . . . [with] warfare and large-scale migrations, which apparently had not taken place in earlier times. This has been attributed to pressure by wild tribes from the north, which, in turn, led to the fall of the empire—yes, you might call it an empire—of Tula. It has been suggested that this started a sort of chain reaction which ran clear south. In the case of the Maya, the post-Classic weakening might possibly be attributed to cultural inbreeding and lack of social sagacity; but, of course, no one knows. I doubt if we ever can put our fingers exactly on the causes of the breakup of some of the great Mesoamerica civilizations.

It seems to me that . . . we have been giving a great deal more attention to the rise and efflorescence of civilizations than to their decline and fall. We in America at the present are at the top of the heap, and I think we ought to give a great deal of thought to what has happened to the high cultures of the past.

Lastly, both in South America and Middle America, comes the stage which has been called "militaristic" or that of empire-building. The development of true empires was very slow in coming about in Mesoamerica, but I think it was definitely on the way not long before the European conquest and possibly during earlier periods in South America.

BEALS (submitted in writing after session): Although I am convinced of the urban character of Mesoamerican civilization, a calculation of the man-hours necessary to build, e.g., the pyramid of the sun at Teotihuacán suggests that a relatively small community could have accomplished the work in less than a decade.

Attention might also be called to the possible influence of erosion resulting from periods of high population density

as a factor causing the periodic collapse of Mesoamerican cultures.

THE OLMEC PROBLEM

KIDDER: Then there is the great Olmec problem. . . . Olmec was a culture which occupied a limited area in the southern part of the Gulf Coast of Mexico. It enjoyed a great efflorescence in art, a most peculiar and distinctive art style, which seems to have existed relatively unchanged for a very long time, but, unfortunately, very little is known as yet about its actual duration. I used to disagree violently with both Dr. Caso and his colleague, Miguel Covarrubias, as to the antiquity of the Olmec culture . . . but I have since seen materials they have recovered which show that the beginning, at least, of the Olmec culture [probably] took place . . . during the Archaic.

WILLEY: . . . I would suggest the possibility—I believe it has been mentioned before—that Olmec may be both a culture and a rise in style. Perhaps you have in the middle or late Archaic, such as at Tlatilco, the first germination of this style, and then its passing out and becoming incorporated into other cultures, something comparable to Chavin in Peru or, say, Hopewell in the eastern United States; so that, in a more limited sense, Olmec is a full culture and, in a wider sense, a rise in style.

RELATIONS OF MEXICO WITH THE SOUTHWEST AND SOUTHEAST

KIDDER: Another important group of questions concerns the relations between Mexico and the Southwest, on the one hand, and the southeastern United States, on the other. Those relations were the subject of one of the round tables held in Mexico some years ago under Dr. Caso's auspices.[4] Incidentally, I wish here to pay a tribute to Dr. Caso for what he has done in Mexico in developing a definite sense of problem among his disciples, of which he has many. . . .

KROEBER: I want to express my agreement as to the importance of what Dr. Caso has done in being problem-conscious and of the inspiring leadership which he has given his younger fellow-countrymen.

PRIORITY OF THE SOUTHEAST OVER THE SOUTHWEST

KIDDER: . . . A matter which has been a little painful to me as a southwesterner is the discovery through carbon 14 dating that a high culture seems to have developed in the Southeast considerably earlier than in the Southwest. In the Southwest we cannot get much back of the year 1, and now the southeasterners are talking about B.C. . . .

Why the Southwest did not take more from the developing cultures in Mexico is a problem. I think that the harsh southwestern environment may have inhibited progress and that people there were not so ready to accept Mexican stimuli, particularly ceremonial, as they may have been in the much more favorable environment of the Lower Mississippi Valley or of the Southeast as a whole. Possibly, ecologically induced isolationism existed in the Southwest, particularly in the north; and this may have led to the extreme conservatism of southwestern cultures and caused their strong resistance to elements from without. . . .

The northern southwesterners, the "Anasazi," so-called, were extraordinarily resistant to any outside influence; and in modern times their descendants, the Pueblo Indians, have resisted European acculturation more successfully than any other native American, certainly North American, Indians. . . . The ancient Pueblo Indians failed to profit from the developments in the East. . . . Take a simple, useful imple-

[4] Sociedad Mexicana de Antropología, *Tercera reunión de mesa redonda sobre problemas antropológicas de México y Centro America* (Mexico City, 1943).

ment, such as the snubnosed scraper, which enjoyed great antiquity and strong use all through the Plains. It reached Pecos, which is on the extreme eastern edge of the Pueblo area, but did not even get as far over as the ruins that were excavated by Nels Nelson some 15 or 16 miles to the west in the Galisteo Basin. . . . In the rest of the Pueblos there is little evidence of eastern influence, except for some incising of Rio Grande pottery and the appearance in one small area of the conical-based cooking pot. Dr. Linton has a very illuminating paper on that subject.[5]

WILLEY: . . . I have a hypothesis that the florescence in the East, climaxing in Hopewell, is the outgrowth of a very rich Woodland tradition, which probably stems from the circumpolar cultures of northern Europe and Asia, and which, finding a suitable environment in the eastern United States, produced an efflorescence at a relatively early date, whereas the Southwest, lacking this environment, might have been less affected.

NEGATIVE AND POSITIVE CRITERIA IN SETTING UP CULTURE AREAS

LOWIE: In an article of Professor Murdock's, cited by Professor Bennett in his inventory paper, the author makes the important point that "marginal" peoples possess a far richer culture than they are usually credited with. This is a sound position, but the inference that *therefore* trait deficiencies should be relegated to a subordinate place is fallacious. It is one thing to define the character of a culture as an independent system, quite another to define its character *comparatively*, as must be done in classifying it. In the latter case, negative features are bound to weigh heavily. The best proof of this is that Professor Murdock, evidently sensible of this fact intuitively, does precisely this—in contravention of the proposed classificatory procedure. To take areas I am somewhat familiar with, in defining his Fuegian culture he stresses at least three negative features of vast significance: lack of agriculture, of pottery making and other complex technological processes, and of class distinctions. His Pampean area is defined by lack of agriculture, of class distinctions, and of inheritance rules; fishing is said to be insignificant. His Chaco area lacks loom-weaving and metallurgy. I believe that Professor Murdock's intuitions were right in each of these cases and that the negative traits he mentions are extremely important for classificatory purposes.

MURDOCK: I am not so sure that [Dr. Lowie and I] are so far apart. What I object to is bringing together peoples scattered over a very large area like a continent who can be characterized only by the absence of traits. I would first group the people on the basis of their positive traits, using the absences of traits as secondary, but still important, elements. . . .

ROWE: The problem is apparently one of emphasis. In a comparison, mention of a trait as present in certain groups should imply its absence in others not listed; mention of a trait as absent similarly should imply its presence in the groups for which absence is not noted. If this point is admitted, it can then be claimed that a comparison which lists either absences only or presences only is clearer and fairer than one which mentions presences in some cases and absences in others.

In existing attempts to classify cultures, however, the selection of traits for listing as either present or absent has clearly been influenced by the classifier's preconceived ideas as to which traits are "important." Selection of certain traits as "important" is usually based on evolutionary ideas of which the classifiers are more or less

[5] Ralph Linton, "North American Cooking Pots," *American Antiquity*, IX (1944), 369–80.

unconscious: absences are stressed in certain groups traditionally regarded as "primitive" and ignored in groups regarded as "complex" or "civilized"; greater weight is given to technological traits than to traits in the fields of curing and social organization; the particular traits formerly used to define stages in cultural evolution tend to be particularly "important" (e.g., agriculture —type usually not specified—metallurgy, writing, pottery, etc.).

In examining existent classifications of South American cultures, the outstanding fact observed is that there is a marked lack of consistency in the application of the standards used. Murdock, for example, makes a great show of listing the criteria he is using, but in practice he does not hesitate to set up certain areas for which he has no information regarding most of his criteria and others which are catchalls, in which he puts those cultures which he does not want to include in any other area, regardless of their lack of homogeneity in terms of his own specified criteria.

The point of lack of information perhaps deserves some further development. What is the value of any classificatory system applied to hundreds of cultures when we have adequate descriptive information for only a handful of them? What is the justification for assuming that a culture about which we know nothing is probably similar to a neighboring culture on which we happen to have some kind of report? . . .

Another point . . . relates to the date of description. It is widely recognized by proponents of culture-area schemes that the areas are laid out without a time dimension. The fact is that, even more in South America than in North America, culture-area classifications ignore time on a gigantic scale: one area will be classified on the basis of a sixteenth-century description and another on the basis of a twentieth-century one. Does this practice involve an unconscious assumption that there is no significant change in native cultures under aboriginal conditions, so that change begins to be an important factor only with the advent of white contacts? If such an assumption is made, it becomes legitimate to take "first contact" as a line of comparability, without regard to absolute time differences. If such an assumption is not made, what justification can be offered for setting up culture areas for the whole continent on a single map?

STABILITY[6]

LOWIE: The comparison of North and South American culture elements involves the question of stability. Is it possible that myths and customs are preserved virtually unchanged for centuries or even millennia? My revered teacher, Professor Boas, thought not, in abstract discussion; but this did not prevent him from assuming far-reaching stability in the tales shared by American and north Siberian aborigines.

It seems to me that the historical civilizations offer positive evidence for a stability of at least several thousand years. The Greek peasant is still reported to believe in Nereids and Moirai. In the Caucasus Von Luschan found natives still playing a dice game with the very same type of astragalus die, partly of lead and with a wooden spine, occurring in a Hittite site of 900 B.C.

Of course, this does not *prove* that the Chaco hockey game is genetically related to its North American counterpart; but it does prove the possibility. The probability will naturally depend in each case on the degree of similarity and the distinctiveness of the features compared.

[6] For further discussion of stability see chap. v, section on "Rate of Cultural Change."

ROWE: . . . Lowie is quite right that the existence of documented cases of long persistence establishes the logical possibility that hockey in the Chaco is genetically related to a similar game in North America. . . . The probability of such a relationship is another problem and depends on much more than the degree of similarity and the distinctiveness of the features compared. To argue about the probability of relationship, one needs to know the full distribution of the hockey game and similar ones, with dates of reported occurrence and notation of known absences, and one also needs to consider the logical association of traits in the game complex in terms of limitations of possibilities.

MARGINAL CULTURES

LOWIE: As to "marginal" cultures, I wholly agree with Professor Bennett that the term is often misleading. I should favor restricting it to geographically marginal peoples. If it is desired to stress lack of complexity, terms such as "simple," "nonagricultural," etc., are quite adequate. In other cases "peripheral to" would eliminate all ambiguity.

BIRKET-SMITH:[7] . . . When Ratzel coined the term "marginal peoples," he used it purely geographically.[8] In ethnology it should be employed in relation to cultural centers, but—and this is a very important "but"—it is entirely meaningless if it is not at the same time referred to some specific center. Aboriginal America is, as a whole, marginal in relation to Old World civilizations; but in America we find two secondary centers, in Mesoamerica and the Andes, respectively. The Eskimo is marginal in relation to Mexico but, from an American point of view, far less so in relation to Asia. Within the Eskimo area we have two cultural centers, the principal one at Bering Straits and a secondary one in the central regions. Greenland is marginal in relation to both, but to some extent we may speak of a third subcenter in the central part of the Greenland west coast.

ETHNOBOTANY

LOWIE: There are probably no more potentially useful results for culture history than those of ethnobotany, ethnozoology, and such other highly specialized fields of research as musicology and metallurgy. Unfortunately, the specialists are so often in disagreement that the culture historian, unqualified to judge the evidence, is left in perplexity. I recall the almost ecstatic welcome Sapir gave to Von Hornbostel's argument for a trans-Pacific connection on the basis of Melanesian and South American Panpipes.[9] But Von Hornbostel's student, Dr. Herzog, remained unconvinced; and my colleague, Professor Manfred Bukofzer, a first-rate musicologist, is quite negative in his reactions.

ROWE: Lowie's remarks on the caution necessary in assessing the results of studies in plant genetics are sound. I doubt, however, that the plant geneticists are going to reach a consensus without any interference on our part. There are many parts of their arguments in which we are quite qualified to intervene, because their theories depend at least as much on diffusion theory as they do on genetic observations [as the following examples will show]. . . .

In case after case [for lack of information] the botanists have had to map present-day distributions and assume that they were the same as those of 1500 B.C. I do not believe it. In some striking cases they have assumed that the absence of a wild variety ("ances-

[7] Transposed from fifth session.

[8] Friedrich Ratzel, *Anthropogeographie*, Vol. II (2d ed.; Stuttgart, 1912).

[9] Edward Sapir, *Current Anthropological Literature*, II (1913), 69–72.

tor") in a particular area today implies its absence there 3,000 years ago. This assumption does not convince me either. . . .

Another favorite assumption is that the present area of greatest concentration of wild varieties is the center of domestication of the related domesticate. There is said to be some direct evidence against this assumption; in any case, it depends on the assumption of the previous paragraph that present distributions can be projected into the past. It is more or less related to our own age-area principle, with regard to which I will only say that the people who use it refuse to test it and the ones who have tested it will not use it.[10]

Our extreme diffusionist belief that no trait can have been invented more than once is matched in some botanical arguments by the assumption that no domesticated plant was domesticated more than once. I do not like this assumption any better in botanical "history" than in cultural "history."

SIMPLE CULTURES

LOWIE: Dr. Bennett makes a strong point of suggesting in his paper that the simpler and earlier cultures are more uniform than others. Here the principle of stressing positive traits seems to me to be properly applicable. Not to go beyond material culture, it is obvious that Yaghan and Ona, despite geographical proximity, mutual borrowings, and comparable simplicity, present a radically distinct adaptation to the conditions of existence.[11]

ROWE: . . . Why is the impression abroad that "simpler" cultures are more uniform? Of course, that assumption was common in evolutionist thought, but I wonder if the inadequacy of most ethnographic reports is not a contributing factor. Ethnographers who ignore differences between groups and between individuals in a culture they are describing . . . are at least not contributing anything to the disillusionment of people who think that "simpler" cultures are more uniform. . . .

BENNETT: This question of the simpler cultures being more uniform I do not intend to defend. I do think, however, it is assumed by those who are reconstructing on a large scale the development of civilization in Mesoamerica and in the Central Andes. . . . That is, we cannot actually trace the spread of high civilization very far. The Peruvians are pretty much limited to Peru and . . . Ecuador. The Mesoamericans are equally limited. Then what is the basis for the similarity in development of both of these? Obviously, it lies in the early periods, the simpler cultures. But, if something is going to grow out of the simpler cultures, it seems to me necessary to assume that they are more uniform and that this is the cause of parallel developments in Mesoamerica and the Central Andes.

AGRICULTURE AND POTTERY

LOWIE: Apropos of the American data [see Dr. Krieger's paper] showing that pottery and agriculture are not coextensive, I should like to propose a world-wide survey of this correlation, often announced as an a priori postulate. It is often stated, for instance, that pastoral nomadism and pottery are mutually exclusive. Well, the Yakut are horse- and cattle-breeders, and . . . yet, according to Jochelson,[12] they have made pottery everywhere in Yakutsk Province except in the northern districts. . . . Perhaps the correlation, so

[10] For further discussion of the age-area principle see chap. v, section on "Diffusion and the Age-Area Concept."

[11] For another example of diversity in simple culture see chap. v, section on "Evolution."

[12] W. Jochelson, "The Yakut," *Anthropological Papers of the American Museum of Natural History*, XXXIII, New York, 1933), 62, 157 ff.

far as it exists, is really not with agriculture but with a certain measure of residential stability. In any case, the problem illustrates that questions of functional correlation and of geographical distribution merge.

ROWE: . . . Some other factors to be kept in mind [in a survey of the correlation between agriculture and pottery] would be: availability of clay and fuel, suitability to climatic conditions of the type of agriculture available, existence of alternate types of containers (stone vases, gourds, metal cauldrons), attitudes toward portable property (people with the same moving habits may have different preferences regarding what is to be moved), methods of cooking used, and methods of packing and transportation. . . .

HISTORICAL LINGUISTICS

HAAS: In Dr. Greenberg's paper it is said that recent interest in descriptive linguistics has been at the expense of . . . historical studies. It is true that preparation of a descriptive statement about a single language is time-consuming. Since few linguists can devote full time to research, rarely is a complete descriptive job, consisting of a grammar, text, and a dictionary, completed in less than five years, and more often ten to twenty years elapse between initial field work and availability of the material in printed form. . . . At that rate, says the anthropologist, when are we going to know more about genetic relationships?

I question whether the rise of descriptive linguistics has led to the neglect of historical linguistics. . . . Among linguists devoting themselves to Indo-European languages, many still confine themselves entirely to historical linguistics, but among linguists in general . . . descriptivists not only are interested in historical linguistics but are making important contributions. . . .

However, linguists have shown some disinclination to tackle problems of remote relationships. . . . Such relationships may be ones that are not immediately obvious; in that sense, they . . . are only a small part of historical linguistics. To many linguists the suggestion of a remote relationship . . . is a working hypothesis, but the real work remains to be done.

Linguists interested in both descriptive and historical linguistics frequently have the bias that historical work must follow descriptive work. . . . This, however, is only an ideal, particularly with unwritten languages. Many linguists relent . . . to the extent of saying there should be a descriptive analysis of at least one language in a family in which historical work is being done. In general, descriptive linguists have not neglected historical studies. Many, indeed, have undertaken . . . descriptive analysis because they want . . . to do historical work.

Another problem mentioned . . . concerns vocabularies. . . . The simplest and oldest way to discover possible remote relationships is by comparison of vocabularies. Among the hazards of this method is the problem of loan words. . . .

In describing . . . comparison of vocabularies, Mr. Greenberg mentions . . . "fundamental" or "basic" vocabulary. Several people have asked me . . . what he means by these terms and by what criteria they were established.

At one time numerals were considered part of basic vocabulary. We now know a number of examples where this will not work at all. An example given by Mr. Greenberg is the Thai numerals which have been borrowed from the Chinese. The numerals 3–10, he says, actually 2–10, were borrowed from the Chinese at a very remote time because they can be reconstructed as prototypes. Cambodian, or Khmer, has

the numerals from 10 up borrowed from Thai or from Siamese proper.

Pronouns are also mentioned as basic. But in many East Asiatic languages, pronouns are not pronouns per se. They are kinship terms, or they may reflect status relationships; for instance, if speaking to a higher person, you may refer to yourself as a slave.

Body parts are also mentioned, and so on. In each case it is possible to find exceptions. . . . I wonder whether selection of basic vocabulary is not . . . more or less intuitive—one gets a feeling for what may or may not work.

Another thing that might be mentioned is the number of items being compared. These may very well be important. . . .

The problem of loan words in East Asia and, I think, everywhere—in East Asia because we have old Chinese records which we can go back through—is important in establishing time chronology. Study of loan words is equally important . . . in the establishment of genetic relationship. . . . There have been, in the Thai languages, loan words from the Chinese from earliest times to the present. . . . To separate out the time chronology is a very difficult problem . . . which is increased by additional loan words from Pali, Sanskrit, English, Cambodian, and many other languages. . . .

Perhaps I should mention specifically one suggestion about area relations made by Mr. Greenberg—the one about Southeast Asia, with respect to Thai and Malayo-Polynesian. . . . I know little about Malayo-Polynesian; I know something about the difficulties of Thai languages. Siamese proper . . . is not considered good for comparative purposes; it is a literary language and . . . has more loan words. However, it preserves one archaic feature lost in all other Thai languages except the extinct Ahom, namely, some initial consonant clusters which are essential for comparative work.

There is also a problem of meaning. Some words have such a broad area of meaning—and I believe this is also true in Malayo-Polynesian—that it is difficult to know which of several words a word should be compared with. A simple example in Thai . . . is *'lu·g*, which means, in a general sense, "offspring." It means "child" or "people" or "animals"; also "fruit"; also "round object"; also it has many other meanings.

The possibility of morphological borrowing is always difficult to handle. There is an interesting example in modern Siamese, which has a great many loan words from Khmer. Siamese has borrowed several Khmer words which have infixes, and they look like infixes even in Siamese. They have, for instance, *da·n*, "to walk," from Cambodian *da·r*. The ordinary word is *da·n*, "walk." There is also *damna·n*, "to walk with royalty," with an infix *m-n;* this is common in Cambodian and no problem. But . . . there is in Thai *sian*, "sound," "voice," or "tone," probably a Chinese loan word. That word has a pair, too—*samnian*—used as a technical term for phonetics, or sometimes for "accent," "brogue."

Here is a case of Cambodian infix borrowed into Siamese, then inserted in a Chinese loan word; if this goes on indefinitely, Siamese is going to look more and more like Mon-Khmer. . . .

Touching North America briefly, I would like to mention another kind of problem . . . and illustrate it from Sapir's Siouan-Yuchi hypothesis.

In one of Sapir's six North American superstocks, namely, Hokan-Siouan, he puts Yuchi, which lacks dialects, with the Siouan languages, which have many dialects. Now the Siouan word for "water," reconstructed, is *mani* and the Yuchi word is *ce*. In any cursory

examination of vocabularies, one would never connect those two, and yet . . . it is possible, using other more remote languages, to show that *mani* goes back to *kw,* which has become *m* assimilated to *n,* and I have to go back even a little further, and the *ce* has developed from this. Whether this means that Yuchi is closer to Siouan than it is to other languages of this general area is still an unsolved problem, at least as far as I am concerned. . . .

HELD: I would like to comment on the value of Dempwolff's work as, for instance, used in the solution of the hypothesis of Schmidt.

The problem here is, What can you do with theories such as Dr. Dempwolff's reconstruction forms now? . . . I believe that Dutch linguists are not so persuaded of the direct application of Dempwolff's reconstruction as perhaps Dr. Greenberg is. Therefore, a few of the linguists now try to develop other theories for approaching the linguistic problems in Indonesia, particularly for historical data. I am referring here to field work done a few years ago in the island of Lombok by Professor Teow, in which he used a dialectographical method based on the older Indo-Germanic theories as used in dialectography in Europe in general.

If we want to make language useful for culture-area descriptions, I think we need more detailed linguistic material than we have now; for this reason I draw attention to this new method as applied by Dr. Teow. I was amazed by the variety in linguistic forms that he found in Lombok. As yet he has published only his maps, not his conclusions. If Dr. Teow's work proves to be successful, we shall have another historical approach to the solution of language problems. If it is not, then it is only another field method.

VOEGELIN: I will confine myself to the first three pages of Greenberg's paper. In general, I agree with Mary Haas's remarks. I should like to add, however, that my colleague, George Herzog, who is an Africanist, has been reading Greenberg's papers on Africa, using the method that he here proposes, and finds them eminently usable and very stimulating for a person who has independently read the African material (which I have not). . . . I think Greenberg, perhaps by being intuitive as well as methodological, has come out with some valuable results on Africa.

Now, I am reminded of a quotation of Santayana's. "We do not," says Santayana, "repudiate our predecessors; we bid them fond farewell." I raise the question whether Greenberg has repudiated his comparative-method predecessors or whether he is intending to bid them fond farewell. He says, for example: "The fundamental assumption concerning language on the basis of which historical interpretation of linguistic similarity becomes possible seems to have been first explicitly formulated by the great Swiss linguist Ferdinand de Saussure in his *Cours de linguistique générale* although its relevance for historical problems is not there stated."

Now, most of the comparative method was worked out for Indo-European before De Saussure, so this gives me the feeling that Greenberg does not quite like the pre–De Saussuren comparative work that has been done. He also says, a little before this, that methods should not be lightly abandoned unless, of course, the data demand it. This, again, suggests that perhaps the comparative method—the "traditional" method, as he calls it—of Indo-European might be abandoned.

Greenberg again says: "In spite of the fruitfulness of the Indo-European hypothesis and the further successes of similar hypotheses in establishing the

'inno-Ugric, Semitic and other fami-es, the assumptions on the bases of /hich these first victories of linguistics s a science were obtained were never learly formulated, and the extension f these methods to other areas of the /orld has suffered from the beginning rom a lack of clarity regarding the riteria of genetic relationships."

This, I think, is an error in fact. Certainly, Meillet's 1912 book is a nasterpiece of clarity. . . . But I think hat we have many books which have 'ery clearly formulated the comparative method of Indo-European.

Speaking of the traditional Indo-European method, Greenberg says: "It s evident that the absence of a direct ipplication for early historical periods s a distinct methodological handicap." This was not Leonard Bloomfield's position in his comparative Algonquian work, which Greenberg cites, and it was not Sapir's feeling. The older languages which the Indo-European comparatists used are not used so much for documentation of the old stages as to effect a saving of time. There is no use in reconstructing Romance when you have Latin. You can take Latin and Greek and Sanskrit as your earlier languages and proceed from there. Methodologically, the older languages are in a sense irrelevant. Indo-European can be reconstructed by using contemporary dialects; older dialects are used for convenience, but methodologically I believe the thing is the same.

Some years ago Harris and I published a paper on the scope of linguistics and suggested that comparative work has never stopped in speed. Furthermore, it has been used fully. I find that, of all the people here whom I know who are working in linguistics, Greenberg and Kroeber—Kroeber in his early work in California languages —have used the counting-samenesses method in one form or another, and the rest of us—Hoijer, Mary Haas, Lounsbury, and myself—have used and published more in the comparative Indo-European method. Greenberg's contention that the focusing of attention on synchronic problems has led to a comparative neglect of basic problems of historic research in unwritten languages is, if the people in this room are any sample, simply not true. We do it also.

The comparative method is one of the most successful methods ever innovated for anything concerning the habits of man. It was a great triumph in the nineteenth century, equaling such twentieth-century triumphs as genetics or quantum mechanics. If I may add one more remark: if, in this excellent symposium one paper is lacking, since the symposium is so heavily historical, it is a paper on the comparative method in linguistics. This is a great lapse.

HEIZER (submitted in writing after session): Although Dr. Greenberg's remarks on North American Indian language classifications are prefaced with the qualification that they are "somewhat impressionistic," the mere fact that these remarks will be printed in a volume which is certain to become a standard reference work leads me, a person with no special knowledge or facility in linguistics, to sound a precautionary note with specific reference to California Indian language classifications.

I am surprised, for example, that "even cursory examination" of Yurok and Wiyot reveals evident relationship with Algonquian. Sapir and Bloomfield once aired their opposing views on this subject in the pages of the *American Anthropologist,* and, although Kroeber assisted Sapir and accepted the relationship, it seems not to have been actually proved to the satisfaction of

all. Thus Hoijer[13] recently took the position that the relationship was "speculative," and Bloomfield[14] says this kinship is "suspected." As one who is concerned with culture history in California, the actual status of such classifications is important for the reason that, as we slowly accumulate new data on California archeology and digest the mass of ethnologic information, our goal is to develop a reconstruction of the events in aboriginal California culture history. The Wiyot-Yurok area is one which displays a distinctive recent culture type, and this can be projected back in time to about 1000 A.D. If, as now seems probable on the basis of available archeological information, the Wiyot and Yurok entered the area about 1,000 years ago, their actual linguistic affiliation will be important in suggesting a possible direction from which they came. Whether Wiyot and Yurok are or are not of Algonquian affiliation, therefore, is important.

With the recent expansion of our knowledge of North American prehistory and the broad reconstructions of culture history which will ultimately be essayed, it is going to be important to know whether Sapir's 1925 superstock classification has been verified by the last twenty-five years' work. Greenberg implies that this is so, but Kroeber at this symposium implied that it is not. My remarks are not in criticism of Dr. Greenberg, but a plea for some agreement on what we actually know about classification of North American Indian language affiliations beyond the Powell classification of 1890.

A second query of the same sort concerns the classification of Yuki speech. The solution of an important local culture-historical problem depends in part upon whether Yukian is independent (as held by Kroeber) or is a member of the Hokan stock (as proposed by Sapir). The Yuki proper display a highly distinctive physical type, which is, however, shared with the neighboring Athabascan Kato. The near-by, but geographically detached, Yukian-speaking Wappo are, however, of the widespread Californian physical type. Obviously, the correct reconstruction of events involving factors of language and physical types will be possible only if we know that Yukian is of independent rank or a divergent Hokan tongue. If the latter is true, the problem will be framed with reference to the wider Hokan problem in California.

These remarks are intended only to show how doubtful language classifications may obstruct the solution of problems which are phrased, on the recent time level, in a linguistic frame of reference.

GREENBERG (submitted in writing after session): I believe the questions raised by Drs. Haas, Voegelin, and Held can be answered by referring to various points in my inventory paper.

1. I do not consider that historical linguistic work has been neglected in the present period of our science, simply that it is not the center of theoretic discussion that it was during the nineteenth century. I do not consider this state of affairs deplorable but rather salutary and necessary, as should be clear from the fact that I allude to a number of ways in which theoretic insights afforded by structural points of view have contributed and in the future will make even greater contributions to our understanding of process of change in language.

2. I do not consider fundamental vocabulary the sole basis for establishing linguistic relationships. My insist-

[13] Harry Hoijer, *et al.*, *Linguistic Structures of Native America* ("Viking Fund Publications in Anthropology," No. 6 [New York, 1946]), p. 13.

[14] *Ibid.*, p. 85.

ence on its value stems from an attempt to counteract the egregious neglect which it has suffered in some parts of the world, notably Africa. All linguistic data—morphologic, syntactic, and lexical—should be taken into account. Where genetic relationship really exists, evidence from all these areas points to the same conclusion.

3. The example of the borrowing of an infix from Khmer into Thai, adduced by Haas, is not surprising. I do not deny that any particular morpheme may be borrowed. I do maintain, however, that a careful consideration of the various means of distinguishing loan words from cognates outlined in the paper is sufficient to decide such cases. Dr. Haas herself is evidently able to carry out such analyses in practice, since she declares that Chinese-Thai and, presumably, Khmer-Thai resemblances are the result of borrowing.

4. I confess myself unable to understand Dr. Voegelin's allusion to my abandonment of the comparative method. To me, this term refers to a body of techniques for reconstruction of linguistic history as practiced by Bopp, Grimm, Schleicher, Brugmann, Meillet, Sturtevant, etc. These methods have been used by me in my own work in this field, e.g., in Bantu and Hamito-Semitic. I do not believe that they are likely to undergo radical transformations in the future.

My allusion to De Saussure refers to another problem, the establishment of hypotheses of genetic relationships among languages. Here I have merely sought to make explicit the partially unstated assumptions, on the basis of which those hypotheses have been reached which have turned out to be correct.

5. In answer to Dr. Held's query regarding the work of Dempwolff in the field of Indonesian languages, I mean to imply only that Dempwolff's reconstructions, so far as I can judge, are, on the whole, valid. I may note in passing that I accept certain modifications proposed in the last few years by Dyen.

Dempwolff's work can give us what any such reconstruction, wherever carried out, is capable of, namely, a picture of the systematic changes by which Proto-Indonesian has become transformed into the contemporary languages of this family. Needless to say, this precludes neither the continuation of linguistic descriptions which are useful in testing the reconstructed *Urindonesisch* nor the application of the methods of dialect geography, whose introduction into this area I heartily applaud.

KROEBER: I think Dr. Greenberg's title should have been altered. His paper is called "Historical Linguistics," and the linguists thought it was in linguistics. . . .

GREENBERG: May I say something at this point? There was originally intended a section which dealt with historical method . . . which I eliminated with regret because of limitation in the number of words.

KROEBER: This paper was intended primarily for anthropologists who are not linguists, and I would entirely indorse Dr. Greenberg's remark that he is not against the comparative method. He is trying to do something exceedingly simple, which anybody who knows nothing more about linguistics than having learned to speak and write his native language can understand. The simple problem—Are or are not certain given languages related in origin?—always has historical and ethnological implications.

This problem has been left largely to linguists, and linguists find it so elementary that mostly they will not bother with it. They have developed precision instruments for precision work, such as reconstructing the original of a language family, and they do not bother with these things, or, if they

do, it is in a most offhand way, as Sapir did, in setting up six families for North America, but never giving the evidence.

All that Dr. Greenberg has done is to take languages from the same general part of the world, compare them, and, primarily by simple inspection, decide which of them show similarities beyond what you would get on the basis of chance.

In other words, he has worked on the simple principle that every word has a form or sound which can be written. It also has a meaning. The relationship between the form and the meaning, except in the case of a few onomatopoeic words, is completely arbitrary; and, if you get between two languages more than a certain small percentage of words of definitely similar sound that have definitely similar meaning, that fact must have a significance, and the significance is that of historical connection.

Now the reason why he had to do the work he did is that eminent linguists like Meinhoff and Westermann in Africa . . . forgot the simple elementary principle of identification of what is similar and related as against what is accidentally similar or what is different. They built up language classes on meaning alone or on form alone, and the result is that they got classifications completely or very largely artificial and unreal. Greenberg has simply returned to the basic method of doing the thing simply, so as to get results. These are not results which will much interest the theorizing linguist.

The situation is much like that of the matter of fingerprints . . . of what is functional and what is adaptive. If you want to identify who killed whom and who ought to be locked up in jail, you do not want adaptive traits; you want traits that are not adaptive, like fingerprints, which are just as arbitrary in their relation to physique as word and meaning are. There you get clinching evidence of who is who and what is what. That is what Dr. Greenberg has been after. I think he has thrown on the scrapheap a lot of obfuscations, a lot of false classification that has grown up through the years.

STYLE

LÉVI-STRAUSS: . . . It seems to me that style is one of the major operational tools we have at our disposal to try to understand the correlation between nature and culture, and it is in this light that I would like to pose a few questions to Dr. Schapiro.

We know, in a purely intuitive way, that "style" not only refers to works of art and to the works of man but also is widely applicable to nature, that trees have style and animals have style, that human profiles often have different styles. This is so not only in the field of empirical observation—we know that crystals have style, that physical representations of mathematical functions have style, and that even molecules have style. It is one of the great achievements of scientific thinking during the last three or four years, for which we are mostly indebted to new disciplines like cybernetics, that we now know that the structure of the molecule provides a much better model for understanding what human culture is than does the behavior of the chimpanzee. Now, if this is so, the main problem of style is to develop common methods to translate into the same language the phenomena of style in nature and in culture. I can see hardly anything which can help us do that except mathematical methods. The common application of the notion of style puts style very close to entropy, and perhaps they are one and the same thing. . . .

The result of applying mathematical methods, of course, raises many problems. There is no doubt that in many

fields different styles consist of topological transformations. For example, it is obvious that the Navajo Indian representation of the human figure is very close to that of the Pueblos. Were this only a topological transformation, it would mean that the representations were extremely unrelated; but there are more difficult problems, like the relationship between the Sulka and Baining arts, geographically close to each other, in Melanesia. These exhibit a common principle that may be expressed as an interest in transitional stages in matter organization. However, Sulka art results in an incipient stage, with all the freshness and greenness of something new and growing, while Baining art represents the dry and decadent aspect of the same transitional stage. Thus I am not at all sure that with this mathematical method it would be possible completely to identify natural styles and cultural styles. The same mathematical formula could explain the difference in the shapes of the oak leaf and the chestnut leaf as well as the painting of Klee and Picasso.

I tried to do this once in the field of music, and there is no doubt in my mind that it is possible to pass from a classical to a modernistic melody by a purely mathematical transformation, of which the composers are, of course, entirely unaware. But the outstanding fact about style is that the human mind is working unconsciously along lines similar to nature's.

Now what is the conclusion? Either mathematical method will prove that it is possible completely to explain natural style and cultural style in the same way, and then the problem of relationship between nature and culture will be solved; or else there will remain a residue—there will remain in human styles something which cannot be explained by the use of this method. Nevertheless, I submit that a large part of the problem will be solved when we hear one of the more rigorous and precise ways to approach the problem of this relationship.

In conclusion, Professor Schapiro suggests that Marxist thought has probably come closest to a true understanding of the relationship between style and the other aspects of social life, and I fully agree with him. But there is one condition: style must be understood by itself, the grammar of style must be elaborated independently of the grammar of social institutions, such as kinship or technology or economics, and it is not between the raw materials that the correspondence can be found but only between the systematized forms, which should first be abstracted on the different levels.

MEAD: . . . If we are going to interpret style in terms of process, it is necessary to link abstract analyses of patterns to human behavior. . . . [Rather than have a series of isolated studies on certain segments of society, we need to study society as a whole, with special attention to the course of development of individuals in the total environment], as mediated by the existing cultural forms. In the same way, Marxian analysis, which says there are correspondences between the art of a period and its economic institutions, [has never been related to what happens in the individual under changing conditions]. . . . Professor Schapiro says that the application of individual psychology to group psychology as an interpretive mechanism is inadequate. I think that is because [of the way historians have taken] individual psychology as thought of by psychologists and pinned it on a group. . . . [For the interpretation of art, we must rather study] individual behavior within a known, defined, specified artistic system. The decay of an art cycle, a problem of interest to culture historians, [is exemplified by Maori art].

Maori art style is complex, very pervasive in the material culture . . . and sufficiently alive so that educated Maori children can produce new and attractive textile designs if given new colors. But when these children are examined with projective techniques, they show an extraordinary lack of imagination, which has suggested to Professor Ernest Beaglehole that Maori art no longer . . . satisfied the growing needs of the individual and might have been expected to break up even without the European conquest.

If we can make detailed studies *in situ* of small communities and of all the stylistic elements in the community, watch the development of style when we give the inhabitants altogether new materials and also their spontaneous behavior with new techniques and introduced forms, and record the effect on individuals of the group, then we can begin to relate style to the organism. I would like to stress the necessity for studying learning rather than merely analyzing the style in itself.

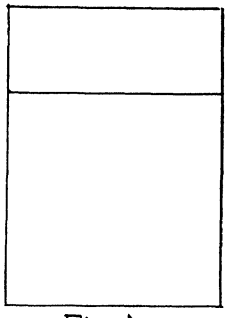


Fig. A

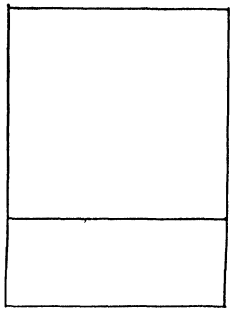


Fig. B

SCHAPIRO (statement revised after session): The question raised by Professor Lévi-Strauss represents a variable tradition in the study of art. In the ancient world, in the Renaissance, and in the modern period, there were people who seriously believed that one not only could grasp the form of a work of art by mathematical methods but could also create works of art by mathematical methods. That view has been rejected by most artists and also by mathematicians who are really sensitive about art.

There are problems of art which are so simple that they can be handled by a mathematician and are often discussed in mathematical treatises. When we turn to such approaches to art, we must be careful to distinguish two things: (1) the use of mathematics as a tool to explain some peculiarity of a work of art—and here, statistical methods are very helpful—and (2) the notion that the nature of art is inherently mathematical and that discovery of the mathematical structure of a composition will give us a principle from which we can deduce a great many other aspects of the work of art. I will give you a simple instance of mathematical analysis of art in which we will see how the difficulties emerge on the lowest level.

Consider this rectangle (Fig. A), divided in this way. We can study the proportions and see whether another division, a little higher up or a little lower down, is aesthetically preferable. Now I am going to show the same diagram upside down (Fig. B). It has exactly the same mathematical properties as the preceding one. In *Aesthetic Measure,* the work of a great modern mathematician on the properties of form and the conditions for optimal choice in forms, Professor Birkhoff studied ornament mathematically with respect to the relation be-

tween the number of elements and the number of relationships among the elements. His formula would apply alike to the object upside down and right side up, to a piece of music played forward and a piece of music played backward. Yet it is obvious that Figure A appears to us very different from Figure B. An artist who will make the one division has another conception, another sense of the whole, than an artist who makes the other. Mathematicians will perhaps be able to introduce into Birkhoff's formula an operator for orientation; but that would require, first, some psychological studies of form perception and the factor of direction in patterns. So far it has not been done, even with something as simple as a rectangle.

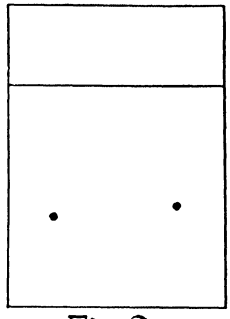


Fig. C

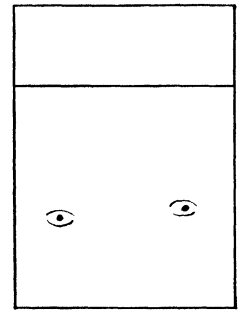


Fig. D

This is not yet a work of art. When we come to a work of art, we are dealing with elements which are already so numerous and of which the relations are so much more complex than those of the so-called "aperiodic" crystal to which Professor Lévi-Strauss made reference that I doubt if any mathematician, even with the modern instruments for rapid computation, would be willing to undertake such a problem. Furthermore, there are many other elements which are not structured in the geometrical sense. Let me give you a simple instance.

If I put one dot here, another dot there (Fig. C), there is a certain pull between them. They do something to the space around them, to the field. But if these dots are made into two eyes (Fig. D), something else happens. It is a very different kind of relationship. In other words, the presence of some factor which belongs to meaning and to expression introduces values hard to measure or specify. It does not mean that we cannot handle them rationally. There are methods which permit us to deal with complex qualities by arranging examples in an order with respect to the intensity of a quality and by defining classes and subclasses accordingly. But here we are no longer in the domain of the quantitative or geometrical, as in the mathematical study of natural morphology.

Let me give another illustration of this difficulty in the definition of style by mathematical means. Suppose one wanted to describe the style of Egyptian art from a mathematical point of view. One could study the relationship of the lines, the way they fill the field, and the proportioning of the figures. But that would not take account of a characteristic of Egyptian art which I think belongs to the style, although some people would deny it, and which I have described in my paper, namely, that in an Egyptian drawing, the head is in profile, but the eyes are frontal, the shoulders are frontal, and the feet are in profile. That method of representation, by giving to each part its maxi-

mum expansion and treating the whole as an aggregate of parts, is something we cannot state in mathematical language unless we introduce such long summations that we recognize at once it is much easier to say, "The figure that looks like that."

Another property of style, discussed especially by Boas, are the small marks made by the tool. One can measure such elements of touch: say that the touch has such a pressure; that the elements range in size from such a magnitude to another magnitude, have a given average distribution, and are repeated so many times before a break occurs. This treatment, nevertheless, does not correspond to the morphology of nature. Not only do we appreciate the touch in terms of these properties, but we sense that these are operations of the human hand; that there is somebody speaking, doing, acting; that the physiognomic aspect of the work of art is very different from the description of a line.

Let us try to distinguish between the meaning of a line for a mathematician and for an artist. For a mathematician proving a theory, it does not matter whether he draws it in red chalk or white chalk. It does not matter whether his triangle is big or small or to the right or left of the blackboard. But for an artist the precise thickness or thinness of a line and the colors and the position with respect to others—these are really constituent factors. That does not mean that there are not, in forms, in compositions, certain recurrent patterns of relationships that, in a broad way, can be described through mathematical equations or through geometrical schemas. But I think that is so primitive, on so elementary a level of structure, that we do not advance far into the character of the work of art.

We may illustrate further with an example from literature. An English mathematician, Yule, studied the problem of the disputed authorship of *The Imitation of Christ* by making frequency distribution curves for the vocabularies of two proposed authors, John Gerson and Thomas à Kempis, and comparing them with one made for this book. Agreement was found between those of Thomas à Kempis and *The Imitation of Christ*, thus confirming the attribution to that writer. But this mathematical method handles only one trait of style, the choice of words. Yule found that the two authors differed in the quality of the words used, one favoring feeling words, the other choosing abstract conceptual terms.

Of course, other branches of mathematics—group theory and topology—have been applied to art, especially to ornament. The mathematician Speiser, in his *Gruppentheorie*, uses two fields to illustrate transformations of groups: crystals and ornament. His illustrations of ornament are taken mainly from the book *Stilfragen* by the Austrian scholar, Alois Riegl. In his book, Riegl's aim is not to give a group-theoretical analysis but to describe the patterns so that we can grasp their compositions and qualities more vividly and follow their historical development from one another.

The aesthetic and historical problems of ornament are not advanced far through the valuable mathematical analyses given by Speiser and others. For that, one must study the meaning of ornament and the processes of art.

With regard to Dr. Lévi-Strauss's observation on Marxism, I tried to make clear in the statement at the end of my paper that Marxism has the enormous merit of being the only attempt to create a comprehensive theory which relates art to society. I did not say that the Marxist theory is able to solve these problems; on the contrary, I indicated that it has not solved these problems. But, in so far as we are concerned with the problem of constructing a theory, we have to mention the important fact

that Marxism attempts to view the social process as a whole in which the way human beings create their means of life, the stratification of society in classes, and the conflicts in the course of which society is transformed all affect the way that people think, their institutions, their religious life, and their arts. And we may say, moreover, that Marxist writing actually does not have a clear conception of the structural forms and that in only a few writers, whom we find to be exceptions —in a recent book by Arnold Hauser and in a book by a Hungarian scholar, Antal, now in England, on Florentine art—do we find a serious attempt to observe the relationship between economic and social life, religious life beside it, and the art forms that arise within the religious sphere and their expressive and psychological aspects.

As to Miss Mead's question, the fact is that, when professional psychologists talk about art, they never quite convince us. I believe, however, that the future of the investigation of style still depends on the development of psychology, but I do not see that the right kind of psychology for it exists yet. It will have to be along lines which are being developed at present, but I think in a much more refined and subtle way than we can find in the works which are offered to us for use on these problems.

LÉVI-STRAUSS (submitted in writing after session): . . . I agree with Dr. Schapiro that the mathematical analysis of an art form would offer insuperable difficulties. However, the problem can be more easily solved by defining not the art form in itself but the *relationship* between two art forms. For instance, even if it were not possible to define in mathematical terms what a tree is, it would remain possible to define the kind of transformation making it possible to pass from, let us say, an oaklike tree form to a firlike tree form because the *redundancy* of the system (as linguists would say)—that is, what makes a tree—can be left out and replaced by another and much simpler question, namely, that of the difference between two kinds of trees. When I tried to use mathematical devices to study kinship systems, some mathematicians objected that this was impossible, since there is no mathematical way to define what *marriage* is. This was settled when a mathematician (Professor Weil, of Chicago) remarked that the question was irrelevant, since not *marriage* but *relationships* between marriage forms was the object of the study.

Therefore, the question remains open whether "qualitative" mathematical methods, such as topology, set theory, group theory, and the like, might not help us to correlate in a precise fashion, on the one hand, relationships between differential values in "natural styles" and differential values in "cultural styles" and, on the other hand, relationships between changes of style and changes taking place in other fields of culture.

KROEBER: In summarizing . . . I need simply remind you that style is what archeologists live by. Without style, they would have to go out of business. On the other hand, the moment one begins to consider culture wholes, one is immediately confronted with the problem of how far such a culture whole is an assemblage of related styles or, possibly, represents a style in itself. In other words, the problem of style is one that is fundamental in practically every aspect of anthropology.

CHAPTER V

Problems of the Historical Approach: Theory

Chairmen: Robert H. Lowie and Wilhelm Koppers.

Papers discussed: Strong, "Historical Approach in Anthropology";* Movius, "Old World Prehistory: Paleolithic";* Steward, "Evolution and Process";* Kluckhohn, "Universal Categories of Culture"; Clark, "Archeological Theories and Interpretation: Old World";* Rouse, "The Strategy of Culture History"; Willey, "Archeological Theories and Interpretation: New World";* Vallois, "Race"; Carter, "The Theory of Evolution and the Evolution of Man."*

Speakers: Lowie, Birket-Smith,† Forde,† McCown,† White, Steward,† Movius, Rowe, Willey, Koppers, Canals Frau, Linton, Vallois,† Hallowell, Washburn.

UNITY OF ANTHROPOLOGY

LOWIE: . . . I should like to make a few introductory remarks which I think connect fairly well with the activities of our dean and president of the symposium, Professor Kroeber. It is one of the characteristics of his scientific career that he has done work in each of the important subdivisions of our highly synthetic discipline. He not only has worked intensively in ethnology, including field work and theory, but also has conducted significant archeological excavations in Mexico and Peru, to give only the outstanding examples. And he has done a tremendous amount of linguistic work. I think he has published well over a thousand pages of purely linguistic material. Even in the field of physical anthropology, he has been active, although naturally not to the same degree as in the other subdivisions which I have mentioned.

I should like to call your attention to the fact that, whatever specialization may have occurred in the last decades, there nevertheless has been, on the part of the masters of anthropology, a feeling that these subdivisions are interrelated. . . . The idea of this symposium rests on the assumption of unity of certain branches of anthropology and even auxiliary sciences as well.

As you all know, Tylor, the great figure in British anthropology, was not a contributor to physical anthropology and linguistics, but he was very keenly alive to these branches of knowledge. In one of his successors, Alfred C. Haddon, you have another figure who took a very broad interest. Starting as a zoölogist, stimulated by Thomas Henry Huxley, he made a trip to the Torres Strait Islands, saw the natives, and became enamored of the study of ethnography. Within ethnography he is best known for his studies of material culture, of technology, and of art. Nevertheless, it is interesting to note that, actually, he maintained an interest in all the branches of cultural as well as physical anthropology.

Recently, in going over old letter files, I encountered a number of letters

from him and, among others, one in which he discussed discriminatingly my paper on the age societies of the Plains Indians. I also recall that when I was still a graduate student at Columbia University, Haddon came to visit Boas and gave an exposition of the genealogical method of determining kinship systems.

I think these great figures in the past of our science are exemplars for the interest in anthropology as a whole. It is obvious that, with the growing diversity of interests and the growing perfection of techniques, we cannot all do everything. We cannot certainly be, at the same time, linguists who are rated as such by the specialists in that field, ethnographers, physical anthropologists, and prehistorians. We can and I think we must, however, take an interest in all these branches, and, what is more, I should suggest that, as various of these papers indicate, we ought also to take an interest in what is being done in neighboring fields.

Some of you, of course, have done that in the past. Some of you very naturally and legitimately have been taking a profound interest in psychology, for example. I think this trend is not only entirely justified but necessary.

BIRKET-SMITH: May I say a word on anthropology as a whole. . . . Of course, in the final analysis all science is an inseparable whole; the boundaries between what we call "separate" sciences are nowhere distinct, and it would, indeed, be highly detrimental to knowledge if they were so. In my opinion the distinction between separate sciences is largely practical, i.e., one of methods. We all strive for the same goal, but we follow different paths and employ different methods, and nobody, I believe, with the exception of a very few geniuses, is able to master, for instance, the methods of physical anthropology, linguistics, and enthnology at the same time. I should prefer, therefore, to consider each of them a science in its own right, and the same applies to archeology, too, although the boundary lines between archeology and ethnology are even vaguer than those between the other branches of what you call "anthropology."

. . . In the following I shall limit myself to speaking of ethnology and archeology, because I do not feel competent to discuss either physical anthropology or linguistics. I believe that we can all agree that the aim of ethnology is an understanding of human civilization, and we cannot expect to arrive at an understanding without studying both the function of existing cultures and their history in the past. Both aspects are indispensable, and I quite agree with Duncan Strong when he says in his paper that "either one of the two approaches is limited and even meaningless without the other." It is quite legitimate to doubt how reliable our historical reconstructions are, but the difficulties of a problem should never prevent a serious attempt at solving it. The present is not understood except as a result of the past, and therefore the vital problem of ethnology, as I understand it, must be a historical one.

Another point on which I agree with Dr. Strong is that ethnology should be neither sociology nor psychology. "Social anthropology" and "social psychology" have become fashionable shibboleths within the last decades, but the interests, for instance, of many British "social anthropologists" seem to be so limited and one-sided that they are hardly recommendable.[1] Nobody will deny, of course, that ethnology can be greatly assisted in its work by both sociologists and psychologists, just as we can get help from botanists and zoologists in the study of cultivated

[1] G. P. Murdock, "British Social Anthropology," *American Anthropologist*, n.s., LIII (1951), 467 ff.

plants and domesticated animals, but this is no reason why we should abandon our own field and throw ourselves about the neck of foreign sciences.

FORDE: I wish briefly to take up one of Professor Birket-Smith's remarks, which I thought . . . quite lamentable. I refer to the fact that things have become so different and unrelated one to another that he was willing to concede that archeologists and ethnologists might continue to talk to each other, physical anthropologists might be allowed in the same room, but it was not clear whether linguists could be talked to at all. . . .

Now this seems to me to be a sin against the Holy Ghost, in the sense that the basic assumption which we make, without formulating it, is that variables in one field of human activity —fields being defined by ourselves, of course—have significance in another field of human activity. . . . For modern peoples, in the ethnographic situation and in Western civilization, I think all of us are prepared to accept—this is the theoretical position, not our knowledge in detail—that there may be variables at the level of genetic inheritance which will be significant with regard to the rate and direction of reaction of different populations to given external stimuli. For example, we may say, hypothetically, that it is not a reasonable theory that all human populations are of equal intelligence. If we have such theories concerning the importance of variables in the biological field, the technological field, and human activity in affecting the final equation with regard to ideological systems and social structure patterns in human groups, then it seems to me that methodologically it is enormously important to keep our minds open to these variables.

The social anthropologists, or that small provincial branch of social anthropologists coming from the British Isles, have been castigated this morning as sociologists. In a way, I think they do lay themselves open to criticism . . . in that they tend in analyses and writings to concentrate very largely on given single social systems as closed systems with an internal set of variables, without taking into account variables coming from the external environment. To give a concrete example, one of the most admirable analyses of the detailed social structure and ritual systems of a people that has been published in recent years is Professor Fortes's study of the Tallensi,[2] which, I think, is widely recognized as magnificent in the wealth of the material brought to bear and the elaborateness of the interdependencies discovered. But you will not find in Dr. Fortes's book any reference—you find a reference, but no consideration of the implications—to the fact of external slave-raiding from outside, which was endemic in the Tallensi area.

EVOLUTION

LOWIE: [One of the points which struck me in reading Dr. Movius' background paper was] his rejection of the old scheme of evolution as presented in the classical treatises of De Mortillet. It is quite obvious, both from Dr. Movius' treatment and that of other prehistorians who have presented papers, that that scheme is now definitely abandoned. In other words, we no longer believe that the old succession of Chellean, Acheulean, down to Magdalenian . . . represents a necessary series of stages in the development of human culture.

MCCOWN: Implicit in Dr. Movius' paper, I think, is a clear recognition of the fact that prehistory has moved from the stage in which all its phenomena could be conveniently arranged in what Dr. Steward would call

[2] M. Fortes, *The Dynamics of Clanship among the Tallensi* (London, 1945).

the unilinear sequence to one in which you have quite definitely a multilinear concept. . . . You now have, very clearly I think, a conception that the phenomena of the Pleistocene have to be represented by something which is much more nearly a bush than a straight line subdivisable into a series of phases or stages.

One thing that I think should be kept in mind is that the only part of the Old World which, up to a period of about twenty-five years ago, had any considerable group of prehistorians working with a really large body of material from a quantitative standpoint was South Africa. If you look at the course of South African prehistory, you will see that there always were, right from the very beginning, some tendencies toward ambivalence with regard to the conception that the whole of Old World prehistory could be conveniently defined in its major formulations in terms of the completely unilinear system beginning with the Chellean, ending with the Magdalenian, and going on into the Mesolithic.

At the present time, the South African phenomena, the Indian phenomena, and the phenomena that we know from scattered points in Asia quite clearly indicate that at the very beginning of the record as we have it—this is the Middle Pleistocene in most parts of the Old World—there was already a very marked diversity in terms of our knowledge of the tangible results of human behavior.

LOWIE: Dr. Steward has quite properly brought out in his paper the point that we need not reject evolution because we have abandoned the thought of unilinear evolution, which, as I said before, all prehistorians now reject in studying the prehistory of western Europe. I myself pointed out years ago that multilinear evolution is involved in such a scheme as that of Father Schmidt. The word may be disagreeable to Father Schmidt; nevertheless, he does constantly use the term "Entwicklung," which means "development," and that is a word which Lewis H. Morgan also sometimes substituted for "evolution."

The old scheme of Father Schmidt as an emendation on Graebner may, perhaps, be put in the form shown in Figure 2. You have a primeval level (I do not like the word "archaic," which has assumed so many meanings). From this base, you have three independent developments, one of farming, the second one of higher hunting, and the final one of pastoralism. The idea is that each of these three fundamental cultures developed independently of the others from the primeval stage, which is, according to Father Schmidt, best represented by the recent pygmies.

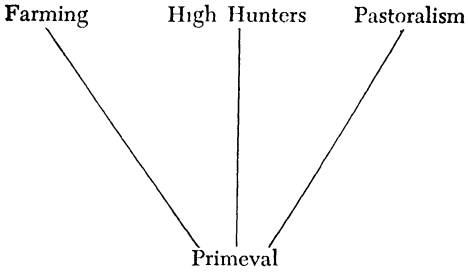


FIG. 2.—Chart of Father Schmidt's hypothesis.

FORDE: It seems to me that Dr. Steward makes a false dichotomy between universal and multilinear evolution. The universal evolution approach of Childe and White, I would suggest, is at a different level of abstraction from the multilinear evolution conception as referred to by Professor Steward. . . . When White and Childe are talking about the development or the emergence of patterns of organizations and patterns of production, they are dealing with them at more abstract levels, just as, when one talks about the emergence of class system and of large state organizations, one is at a different level of abstraction than when one is talking

of a particular cultural configuration. It is quite possible and useful to consider problems of universal evolution at one level of abstraction, and very important to do so from the point of view of heading up problems of basic processes and the conditions at which they operate.

WHITE: As you know, Dr. Steward has a threefold classification which he calls "unilinear," "universal," and "multilinear." It seems to me that a logical inconsistency inheres in this classification.

Culture can be considered as one or many. Unilinear and multilinear are opposite, but inseparable, poles of the same phenomenon. Dr. Steward's unilinear category, it seems to me, is simply an aspect of what he calls "universal evolution."

If by "unilinear" he means, as I think he says, to refer to the evolution of particular cultures and if these cultures followed one and the same line, then the evolution is universal. If they do not follow one and the same line, then it is multilinear; so that it seems to me that it would be better to have a classification with only the multilinear and the unilinear categories. As I say, it is not an either-or proposition. Each term implies the other.

With regard to Dr. Steward's comments on unilinear evolution, I do not know that any anthropologist, or any of the nineteenth-century evolutionists, ever said that every people had to pass through the same series of stages of development. Spencer and Tylor expressly emphasized the multilinear aspect of cultural evolution, although Tylor also presented a unilinear conception. Morgan was particularly interested in the unilinear aspect of cultural evolution, but he recognized, of course, its multilinear aspect as well.

With regard to who these unilinear evolutionists are and also with regard to the distinction between unilinear and universal, Professor Steward apparently has not been able to make up his mind on some points. For example, on an early page of his paper, White and Childe are unilinear evolutionists, but on a later page they are universal evolutionists. Some jump has been made in the intervening pages.

Professor Steward makes it very clear that his preference and interest were in a consideration of the multilinear aspect of cultural evolution. In this it seems to me that he belittles, perhaps unnecessarily so, such contributions as the universal evolutionists have to make. In his paper, on two different occasions, and also in previous publications, he has—it seems to me, at any rate—tended to disparage these contributions, calling them "so commonplace that everyone would accept them," or else he says that there is nothing startling about them, which it seems to me introduces a somewhat novel criterion for the evaluation of contributions to science. In expressing this point of view, Dr. Steward's statements are, I venture to suggest, more expressive of his own personal interests than they are an impartial evaluation of the contributions of the universal evolutionist. After all, some of the universal evolutionists have done something besides say that hunting and gathering must precede agriculture.

Dr. Steward wants his evolution piecemeal. He wants evolution in restricted areas and in restricted segments. If, however, evolutionist processes and evolutionist generalizations can be made in a number of independent situations and regions, why cannot generalizations be made for evolution as a whole? If there has been evolution of badgers, bats, birds, and butterflies, why is there no evolution in general?

It seems to me that I notice a rather curious conflict or contradiction of motives in Dr. Steward's scientific work. On the one hand, he seems to be very

much interested in generalizations and strives to reach them. On the other hand, he anchors himself to the particular, to the local, to the restricted, which, of course, tends to inhibit the formulation of broad generalizations.

I think, perhaps, I might close by again remarking that it seems to me that there is no conflict between unilinear and multilinear evolution. They are simply complementary aspects of the same phenomenon, two different points of view from which processes can be observed. . . . I would, of course, greatly welcome particular and restricted studies of cultural evolution, although, to the extent that you restrict them to local areas, you introduce a factor of history of particularity which militates against the formation of generalizations; but I would like to suggest to Dr. Steward that, if evolutions can be discovered and evolutionist formulations can be made in a number of particular regions, I see no reason why over-all, comprehensive, all-embracing generalizations cannot be made.

STEWARD: . . . I have no particular feeling about what one ought to do. All I have tried to do is raise the question of what now is possible in terms of the methodology which we have developed. Actually, it would seem to me that most substantive research—dealing with ethnologies or with archeological sequences in their concreteness —have, when they have dealt with generalizations at all, been limited to very slight resemblances of sequence, form, function, or process. I know of very few in which it has been possible to universalize.

Now I do not want to go into that point here. It seems to me that it involves a point which Dr. Kluckhohn treats in his paper. Whether the question of universality is one of function or sequence (process through time) makes very little difference conceptually or methodologically, in my opinion. I simply raise the question of whether, when you universalize either function or sequence, you are left with anything but such very broad abstractions that, to me at least, they present nothing really new. I would call attention to the fact that the generalizations made by White and Gordon Childe do not mainly take into account their own field research, i.e., they are as yet dealing with culture generally and not with cultures in their concreteness.

Daryll Forde made a point here which I think is quite valid. We may abstract or generalize from the particular culture, regarded relativistically as unique, to something that is universal. I simply doubt that at this point we have any very meaningful conclusions on a universal scale. I think the more concrete—that is, the less abstract—our conclusions are, the more meaningful, to me at least, they have been until now. Nevertheless, I would like to see universal processes of all sorts, and I hope we shall achieve them.

PARALLELISM

LOWIE: Dr. Steward's treatment of [evolution] involves . . . the question of causality and parallelism. He has personally devoted himself especially to a study of the effect of certain ecological conditions on sociopolitical structure, and I think that he has, on the whole, been very successful in showing a correlation along those lines.

When we take any line of development such as that envisaged by Professor Steward, we have to ask ourselves which of a number of traits that are regarded as constituting that line of development is really basic in the sense that it is antecedent to the others. . . . For example, if you have a matrilineal complex, is the element of descent the antecedent element, which leads to all the others, or is it some economic factor, such as women's activity as a tiller of the soil? . . .

Now I venture to say one other thing on the question of parallelism and causality. I think there is a certain amount of confusion in treating all cultural elements on the same level. When we deal with technological features or, perhaps, more broadly, with features of material culture, we often do have a necessary presupposition for a particular development, and parallelism in such cases may be, if not impossible, at least improbable. This does not necessarily apply to the nonmaterial phases of culture. I could never understand, for example, why my friend Professor Kroeber at one time thought that the mother-in-law taboo had to be diffused from Mexico to the Indians north and south. I admit that, perhaps, the mother-in-law taboo has been of great importance from a Darwinian point of view; perhaps those tribes that practiced it had a better chance of survival than those that did not. Nevertheless, everything considered, it seems to me that the intellectual feat of establishing these respect relationships which we label as mother-in-law taboo may very well arise on any level of development.

Similarly—and I do not believe that on this point I am really in disagreement with my friend, Professor Lévi-Strauss; at least, our disagreement is only on the manifest level—I think that moieties can and do develop in certain circumstances independently. I do not want to enter into that more fully now.

CONVERGENCE

LOWIE: Another thing that I should like to call to your attention, because it has not only theoretical but also methodological implications, is the evidence which prehistorians such as Drs. Clark and Movius give concerning what is known as "convergence" in the strict sense of the term, that is to say, the arrival at similar or like results through different paths of development. In Dr. Movius' paper, for example, I find a quotation from Watson: "It is likely that a Stone Age community quite readily adopted the shape of the tool it found in use by alien tribes once its advantages were manifest, but much less likely that it should have the opportunity of learning or be prepared to revolutionize its industrial technique in order to reproduce the precise method of manufacture practiced by the original users of the tool."

I notice, incidentally, that this also brings in the problem of the dynamics of diffusion. But we also have here a suggestion of convergence. You will find others in this and other papers by the prehistorians.

This, I said, has not only theoretical but also methodological importance. Those of you who are at all acquainted with Graebner's *Methode*[3] and Father Schmidt's[4] subsequent enlargement on that work know that they pay a great deal of attention to the criteria by which historical connections are established, the criteria of form and quantity. This indication of convergence obviously strikes at the criterion of form. The same form of implement may be arrived at independently by distinctive processes. I am not saying that we have to reject the criterion of form, but it may suggest the desirability for modification of that formulation of the form criterion which has been popular within the culture-historical school in Germany and Austria.

MCCOWN: Implicit, I think, in the vast majority of the writings of the prehistorians who deal with the Paleolithic has been the underlying conception that similarities in the phenomena which you find are primarily the result of diffusion. Careful investigation of the phenomena, so as to obtain suffi-

[3] F. Graebner, *Methode der Ethnologie* (Heidelberg, 1911).

[4] W. Schmidt, *Handbuch der Methode der kulturhistorischen Ethnologie* (Münster, 1937).

cient data to decide what is in every case, I think, the more difficult problem, whether you are dealing with convergence, is rare. This is due in part, I think, to the lack of any clear conception as to the techniques and general methods that are applicable to Paleolithic sites.

Dr. Movius has mentioned what is now a well-known case of possible convergence, i.e., the appearance of bifacially flaked tools in a cultural context which is very heavily weighted, both qualitatively and quantitatively, in favor of a tradition which is rather different in both its formal and its technical aspects. These tools are what he has called the "proto-hand-axes" of Java. Here, I think, you have a beautiful case of the sort of situation which, on the face of it, gives every evidence of being one in which convergence can really be demonstrated. However, the underlying assumption is either that Java was screened by the purely nonhuman, physical environment or that there was some sort of barrier of culture, which resisted the possible diffusion of the ideas of making this particular kind of bifacially flaked tool from its center in India, which is, in relative distances, not very far from Java.

Now this is precisely the sort of situation in which, as Dr. Movius has pointed out, the investigations that are needed are lacking, and it is not very obvious that they can be carried out in the immediate future. We have only the fact that Dr. Movius' own careful survey of the gravels of the Irrawaddy River in Burma failed to turn up anything that could be construed as even a proto-hand-ax, apart from real hand-axes.

MOVIUS: . . . On this question of convergence, I might mention that the whole assemblage in Java is more similar in its fundamental pattern to what is found in the Belgian Congo than it is like what is found in India. Now what does this mean?

We have two tropical regions. We have adjustment in each case to a certain type of environment. We are on approximately the same time horizon in both cases. But from there, where do we go when we are left only with the imperishables of man's handiwork? All we can say, and all we are ever going to be able to say, I think, is that under these parallel types of conditions . . . man as a thinking being is capable of arriving at the same solutions to the problems that face him in his daily life and that this can happen in widely separated areas.

The question of distribution brings us in the Pleistocene, of course, face to face with the natural sciences; for, unless we can keep on the same time horizons, we cannot do anything with distributions. Until we get further advances in the natural sciences which will make possible more refined subdivision of our long periods of time, we are not going to be able really to adopt and employ the distribution method that is so successfully used by students of human culture in the later periods; but at least we can think about it and hope that the day will come when we can do so.

RATE OF CULTURE CHANGE

LOWIE: Another point which I think some of the papers on prehistory bring out clearly is that in certain circumstances there is remarkable stability. Here I should refer back to what Professor Schapiro mentioned yesterday afternoon, namely, the great stability in the art style of Egypt.[5]

MOVIUS: [An example of the opposite extreme—rapid culture change—was provided yesterday by Clark's mention[6] of the "tremendous acceleration in the rate of cultural development" which seems to have taken place

[5] For further examples of cultural stability see chap. iv, section on "Stability."

[6] In chap. iii, section on "Relation between Physical Type and Culture."

during the Upper Paleolithic.] The great significance, in an over-all sense, of this tremendous speedup in the rate of development is that it seems to have been necessary before the next stage in human civilization could be attained. There probably would not have been such a rapid spread of farming and of the new food-producing economies, had people not adjusted themselves to responding at a more rapid rate to the problems which they encountered in their daily life and which are expressed in the extinct—well, we call them "cultures," but they are assemblages of imperishable objects of man's handiwork.

LOWIE: The question that arises here for the general culture historian is an obvious one. Why this extraordinary stability in some cases, why the fluidity in others?

ECONOMIC INTERPRETATION OF PREHISTORY

MCCOWN: I think one of the valuable parts of Professor Grahame Clark's background paper is that the application of what I regard really as a sort of human brain power to materials which, twenty or twenty-five years ago seemed very limited, has made it possible to do a great deal in the way of understanding the kinds of complex relationships involved in the cultural elements that exist in the Mesolithic. . . . Twenty-five years ago, I think that the majority of prehistorians would have considered as being absolutely out of the question the writing of a book such as Professor Clark has recently produced,[7] which lays out fascinatingly and in great detail the characteristics of the economic life in prehistoric Europe following the Paleolithic.

It seems to me that prehistorians whose main concern is with the Paleolithic have, again and again, neglected opportunities to infer from their data, quite legitimately in many cases and in other cases with a very much larger margin of speculation, phenomena which reflect the economic and other relations of Pleistocene peoples.

Part of this, I think, is due to the fact that the methods and the techniques that are employed in the excavation of sites by prehistorians interested in the Pleistocene have been essentially homegrown. There has been little attempt to work out any sort of consistent and well-thought-out methodology involving a multiplicity of techniques. . . .

MOVIUS: It is certainly true that . . . the techniques we have used in bringing the materials out of the ground have left much to be desired. I think, though, that we are going to have to work somewhat differently from the people of the later periods, who have building levels, etc. We are going to have to adopt techniques of the natural sciences, such as chemical and soil analysis, [which] might yield very valuable information indeed. . . .

DIFFUSION AND THE AGE-AREA CONCEPT[8]

BIRKET-SMITH: The question of the origin of a culture element tapers down to one of independent invention or diffusion from some other place, in which case it may have spread by tribal migration or simply by stimulus from other peoples. Much speculation has been wasted in the discussion of invention versus diffusion, but the only way of arriving at a solution is by painstaking investigation in every single case. Obviously, we should not jump at conclusions. I am far from denying the possibility of trans-Pacific connections between Oceania, Mexico, and South America, but we must not ignore the fact that some data may be interpreted in terms of independent origin in South America, for instance, of the divine kingship in Colombia, the slit-drum,

[7] J. G. D. Clark, *Prehistoric Europe: The Economic Basis* (London, 1952).

[8] For a previous reference to the age-area concept see chap. iv, section on "Ethnobotany."

and the blowgun. Only exceptionally do we get some assistance from archeology, as in the case of maize cultivation. We have positive evidence of the cultivation of corn in America at such an early date that a spread across the ocean from Asia seems to me an impossibility.

I can only touch the numerous problems arising in connection with the diffusion of culture. We know for certain that migrations have taken place in historic times. I need only mention such peoples as the Arabs, Turks, and Mongols in the Old World and many Plains tribes and the Tupians in the Americas. It is safe to infer that the spread of the Bantu, the Athapaskans, and the Eskimo—to mention only a few examples—must be due to similar migrations. A migrating tribe will, of course, carry its culture along to its new habitat. If the geographical conditions there differ essentially from those of its original environment, it must try to adapt itself to the new surroundings, either, like the Maori of New Zealand, by dropping some of its old elements and inventing new ones[9] or by taking over more suitable elements from the aboriginal population. The latter procedure is seen among the Eskimo of the Northwest Passage, who came from the interior in rather recent times and adopted from the Thule culture a considerable number of elements pertaining to sea mammal hunting.[10]

I presume, however, that by far the greater amount of culture elements have been diffused not by tribal migration but by gradual transmission from one tribe to another and usually not in complexes but rather separately. As mentioned before, a complex will sooner or later tend to disintegrate. Geographical factors may sometimes form insurmountable obstacles to certain elements, or they do not fit into the established pattern, or they are simply ignored for some obscure reason which can only be characterized as a kind of mental inertia. I had the opportunity of observing an example of the latter kind among the Aivilik Eskimo at Repulse Bay, who repeatedly saw our Greenland companions employing the well-known cotton shooting screen when hunting seals basking on the ice. They expressed their profound admiration for this useful little implement, but, as far as I know, they never even once tried to imitate it.

One of the most important, but at the same time most difficult, problems in historical approach is chronology. Unfortunately, it is only exceptionally that we have direct evidence of the adoption of new elements, setting aside, of course, those which are due to European and American contact; but sometimes we do find such cases. When last year I spent some days among the Bontoc Igorot of northern Luzon, I saw both the ordinary rice-harvest knife and the heavy Philippine wooden plow of Chinese type. Neither of them occurred among the Bontoc fifty years ago, when Jenks wrote his great work on the tribe, and I need not point out that they cannot have spread through contact with the Americans.

Ethnology has several means of establishing the relative age of culture elements, but here I can mention only one of them, namely, their geographic distribution. We are all aware that this is a very dangerous method, and when employing it we have to take all sorts of precautions. On the other hand, if we acknowledge the reality of culture diffusion, there is bound to be some sort of relation between time sequence and distribution. Burrows has enumerated a number of elements characteristic of

[9] Te Rangi Hiroa (Peter Buck), *The Coming of the Maori* (Wellington, N.Z., 1950).

[10] Kaj. Birket-Smith, "Recent Achievements in Eskimo Research," *Journal of the Royal Anthropological Institute,* LXXVII (London, 1951), 150.

western Polynesia.[11] When I visited Rennell Island, one of the Polynesian outliers in Melanesia, I found that very few of these western elements occurred there; but, whatever the final result of an analysis of Rennellese culture may turn out to be, there must be some connection between this fact and the history of the population.

It is too simple to say that the greater the area of distribution, the greater the age of the element. If that were so, the Ghost Dance religion would be one of the oldest elements in aboriginal North America. I should rather believe that great distribution combined with a number of locally developed subtypes is an indication of considerable age. A special case occurs when a distribution area is broken, because the element in question has been partly replaced by another one, which, in this place at least, must be later. The same thing may happen also to culture complexes. Thus, the so-called "neo-Eskimo" culture, characterized by whaling with harpoons and floats, sealing with nets, the abundant use of polished slate, etc., once spread from Bering Straits to Greenland but is at present to be found only in Alaska, the Mackenzie delta, and east of Hudson Bay.[12]

The safest way to establish cultural sequences is, needless to say, the archeological method, and the most complete and reliable reconstructions of the past have been obtained where ethnology and archeology have worked together. Thus final proof of the occurrence of neo-Eskimo culture in the regions of the Northwest Passage was acquired through excavations. Archeology is far from being infallible, and even at best it can give nothing but a fragmentary picture, but nevertheless I consider an intimate collaboration between ethnology and archeology as of paramount importance in historical approach. (I share this view with Irving Rouse, cf. his inventory paper.)

From what I have said here it will appear that the idea of culture elements spreading in beautiful concentric waves is fallacious, as already emphasized by Dixon many years ago with regard to the Sun Dance complex.[13] Among the Eskimo, for instance, diffusion is strictly linear, along the Arctic coast, and the main center of cultural development is even situated on the outskirts of the area.

DEVELOPMENTAL CLASSIFICATION

BIRKET-SMITH: . . . In his inventory paper Julian H. Steward tells us that Gordon Childe's "postulated cultural sequences are so general that they are neither very arguable nor very useful."[14] I am afraid that very much the same thing may be said with equal right of the terminology adopted by the Chiclin Conference and the Viking Fund Peruvian Symposium. Of course, both systems do characterize the cultural sequences in the areas in question in terms of development. The danger is apparent, however, as soon as the Peruvian scheme is transferred to what is now called Mesoamerica. First, some unsettled soul may easily believe that the stages are synchronous, which is extremely doubtful, as pointed out, for instance, by Gordon Willey in his inventory paper. It is much worse that we do not know whether the same cul-

[11] E. G. Burrows, *Western Polynesia* ("Etnologiska Studies," Vol. VII [Goteborg, 1938]).

[12] Birket-Smith, *op. cit.*, p. 146.

[13] Roland B. Dixon, *The Building of Cultures* (New York, 1928), pp. 167 ff.

[14] Cf. Gordon Childe, *What Happened in History* (Harmondsworth, 1942). Cf. also Kroeber on sociologists: "By the time he finds a formula that no one can cite exceptions to, it has become so essentially logical, so remote from phenomena, that no one knows precisely what to do with it" (A. L. Kroeber, "History and Science in Anthropology," *American Anthropologist*, n.s., XXXVII [1935], 561).

tural forces have been at work in both areas. This is even more flagrant if the same system is employed in both the New and the Old World, as has been attempted by Steward.[15] If, for the sake of argument, we tried to apply Childe's system to Peruvian and Mesoamerican cultures, it would at once be obvious that the conditions which gave rise to what he calls the "Urban Revolution" must have been quite different in the two hemispheres.

ROWE: I was very much interested in Dr. Birket-Smith's comparison between the functional developmental classification set up by the Virú project and applied later on in Central America, and Childe's and others' theories of stages in Old World archeology. There is something of a parallel here to the business of the Old World chronology being condensed while the New World chronology is being extended,[16] because I think I see some tendency on the part of Old World prehistorians to abandon the sort of stage interpretation of culture history in their field at the same time that American archeologists seem to be turning toward a rather similar set of stages. In this particular case, I personally feel more sympathetic with the abandonment tendency of the older prehistorians.

The functional-developmental classification, like the stage theory in the Old World, is used by archeologists for a very particular type of historical reconstruction. Archeological evidence tells us something about material culture, but, since potsherds do not talk, it does not tell us very much about nonmaterial culture. One of the reasons why, it has seemed to me, some American archeologists have turned with enthusiasm to functional-developmental classifications is that these classifications imply an association between material and nonmaterial items of culture such that, if you find certain types of material culture, you can infer the nonmaterial culture that goes with it, at least in a rather broad fashion. The correlation between material and nonmaterial culture items, however, is not something which has been arrived at empirically from examination of detailed evidence but is something which is arrived at a priori on the basis of what the theoretician thinks is reasonable.

The curious thing is that we have seen in American archeology a rather considerable uncritical acceptance of the associations implied by the functional-developmental classifications at the same time that the archeologists have neglected the existence of a very considerable body of ethnological information in which you can find plenty of cases where the associations assumed in functional-developmental classifications do not hold, where you examine both the material and the nonmaterial culture and find that the material culture is not associated with the kind of nonmaterial culture that the classification demands.

WILLEY: . . . It does seem to be the case that Old World archeologists are now going away from synthesis, whereas the New World archeologists are beginning to take it up, at least in places like Peru, but it seems to me that this is only natural. . . . We got off to a slower start.

The Peruvian developmental classification is a beginning synthesis, and I believe that it recapitulates the configuration of historical change in Peru which has very striking parallels in Middle America. What the similarity in these two configurations means, we do not know. That is a problem frame of reference, we will work on it, and eventually, I am sure, the scheme will be greatly modified or discarded. . . . I

[15] Julian H. Steward, "Cultural Causality and Law," *American Anthropologist*, n.s., LI (1949), 1 ff.

[16] See chap. iii, section on "Comparison of Old and New World Chronologies."

am sorry if the terminology is so cryptic, but remember that in almost every area, at least here in this country, when synthesis began, as, say, in the Pecos classification of the Southwest, that was one of the greatest criticisms: much too complicated, too many curious words, etc. But I do not see how you can get around that. Probably in the beginning, all such syntheses will tend to be clumsy.

THE "KULTURKREISLEHRE"

BIRKET-SMITH: Another system of universal evolution and terminology, although very different from those mentioned, is the *Kulturkreislehre* of German and Austrian ethnologists. I do not intend to discuss the great merits and shortcomings of this school, since it has been done by Lowie and Kluckhohn far better than I could do,[17] and Professor Koppers will speak about it here. I only wish to add, as I have pointed out on previous occasions, that, owing to geographical, psychological, and other factors, most culture complexes will sooner or later tend to disintegrate when they spread over long distances, and for this reason alone, if for no others, we cannot expect to find the same *Kulturkreise,* for instance, in Melanesia and in the Americas.[18]

KOPPERS (Lowie translating): The very close relationship between prehistory and ethnology, which seems to me essential, has always been more obvious to American scholars than to European ones.

Even a considerable number of years ago, in a critique of Menghin's great work which I myself published, I protested against the absolutization of a culture-historical scheme. In other words, I rejected the adoption of a scheme to fit all possible situations.

Furthermore, as a result of studies made largely by Russian scholars, I have come to reject one very essential feature of the scheme of Father Schmidt which I put on the blackboard in a very elementary way (Fig. 2). The pastoral *kreis* is no longer considered one of great antiquity . . . and it appears to me and to my chief assistant in Vienna, Dr. Haekel, and to others that the sequential part of the scheme, namely, the theory that the reindeer was the earliest of domesticated species, that it was followed by the horse, and that, finally, other species such as cattle were domesticated, has to be abandoned. The evidence, largely from prehistoric material, shows that cattle came first of these three, the horse definitely later, from about 2500 B.C. . . . to possibly 3000 B.C., and that the reindeer is last. In other words, we are by no means inclined to uphold Father Schmidt's original scheme against the obvious testimony of the facts.

CANALS FRAU (submitted in writing after session): Con relación a lo que acaba de expresar el Dr. Koppers, quisiera yo llamar la atención sobre un punto que está contenido en el trabajo de W. Duncan Strong. Al referirse a la *Kulturkreislehre* dice Strong que la misma "has also many advocates in Argentina and other parts of South America." Es este un dato que es discutible.

En realidad, y al hablar de la *Kulturkreislehre* debemos diferenciar claramente entre lo que llamamos el método histórico-cultural, que en el fondo no es otra cosa que la utilización de métodos históricos en antropología o etnología, algo así como un "historic approach," y la formulación de los

[17] Robert H. Lowie, *The History of Ethnological Theory* (New York, 1937), pp. 177 ff.; Clyde, Kluckhohn, "Some Reflections on the Method and Theory of the Kulturkreislehre," *American Anthropologist,* n.s., XXXVIII (1936), 157 ff.

[18] Kaj Birket-Smith, *Kulturens Veje* (2d ed.; Copenhagen, 1948), pp. 74 ff.; Swiss ed.: *Geschichte der Kultur* (2d ed.; Zurich, 1948), pp. 53 ff.

Kulturkreise propiamente dichos, esto es, círculos o áreas de cultura primeramente establecidos para Oceanía, y que tendrían valor universal.

Si Strong se ha referido a este ultimo, es decir, a los *Kulturkreise,* entonces puedo decir, que en la Argentina, tal vez con la única excepción de Imbelloni y ahora de Menghin, la doctrina carece de adeptos. En cambio, la mayoría de antropólogos argentinos está de acuerdo en el empleo de métodos históricos en sus trabajos.

MIDWESTERN TAXONOMIC SYSTEM[19]

BIRKET-SMITH: Before concluding these critical remarks on terminology, I shall refer very briefly to the so-called "Midwestern Taxonomic System." Here, I believe, the danger is its arbitrariness. Terms like "focus," "phase," "aspects," etc., have now required a more or less well-established meaning in the archeology of the Middle West, but it is extremely difficult to transfer them to other areas. Dr. Helge Larsen and I have both, independently of each other, tried to use them in Eskimo archeology, but we had to give them up, because the definitions are entirely subjective. It may not be out of place to call to your mind that the study of the European Neolithic, which is at least as complex as that of the Middle West, has arrived at very far-reaching and well-founded results without such a cumbersome taxonomy.

As a little boy coming from Hans Andersen's country, I sometimes feel inclined when confronted with elaborate terminologies to say, like the boy in the tale of "The Emperor's New Clothes": "He has got nothing on!"

LINTON: I have asked for one minute apropos of the McKern or Midwestern classification. In judging approaches of this sort, I think it is necessary to take into account both the time and the place. At the time that the current classification was developed, middle-western archeology . . . was still in a very rudimentary state. The average middle-western archeological site was about 8 inches deep, without stratification. The McKern classification was not an attempt to reduce a mass of organized data to some sort of new order but an attempt to bring some kind of order into a great mass of discrete accounts of digging in more or less uniform sites in different places.

Taxonomically, it corresponded exactly to the attempt to classify birds in a period when there was no paleontology. It must be regarded from this point of view. It is now being replaced by the sequential classifications wherever possible. However, it was the first attempt to bring some sort of order out of chaos in the area. . . .

USE OF LITERARY SOURCES

BIRKET-SMITH: When trying to reconstruct the history of any one culture, we are at once confronted with two questions: What is the origin of its individual elements? And what is their age? Implicitly we will obtain the answer to a third question: How did the synthesis take place by which the culture acquired its individual stamp? An attempt at pointing out the methods by which these problems should be approached would result in a complete description of ethnological methodology, and that is, of course, out of the question here. I shall only refer to the books of Graebner, Van Bulck, and Wilhelm Schmidt, which, in spite of certain deficiencies connected with their particular views, are still the most comprehensive and thorough works of their kind.[20]

[19] For a previous section on the Midwestern Taxonomic System see chap. ii.

[20] Graebner, *op. cit.;* Gaston van Bulck, *Beitrage zur Methodik der Volkerkunde* ("Wiener Beitrage zur Kulturgeschichte und Linguistik," Vol. II [Vienna 1931]); Schmidt, *op. cit.*

There are, however, one or two things [concerning the use of literary sources] which I want to emphasize; for, although everybody seems to agree with them in theory, they are all too often neglected practically. . . . First of all, if possible, *all* firsthand sources should be consulted. It is self-evident how extremely defective our knowledge of, say, the Indians of the eastern United States or of the Amazon area would be if the early sources of the sixteenth, seventeenth, and eighteenth centuries had not been taken into account. . . . Another point is that translations should be avoided if at all possible, because they may sometimes contain fatal blunders. . . . For instance, Erland Nordenskiöld once told me that in the French translation of one of his books a throwing board had been converted into a blowgun!

Finally, the reliability of the source should be submitted to scrupulous trial. An ethnologist working with the North American Indians must know that the extent of John Lederer's travels in the Southeast is open to doubt and that great parts of Hennepin's *Nouvelle découverte d'un très grand pays situé dans l'Amérique* (1697) are spurious. Another source of mistake is the fact that some early authors have freely copied their predecessors. Thus we find in the work of De Vries a description of the Mohegan which is nothing but a repetition of what Megapolensis wrote about the Mohawk. . . . I mention these instances just to show how careful we must be when employing written sources.

HISTORY AND PROCESS

BIRKET-SMITH: . . . . I hope to have made it clear to you that I do not swear to any one "school" or any one method. There is no such thing in ethnology as a master-key that opens all doors, and strait jackets in the form of rigid systems are more than dangerous. In historical reconstructions we must, as in any science, proceed from the known to the unknown. In the light of our present knowledge, cultural evolution has been multilinear, and whether it will be possible to find universal "laws" sometime in the future is as yet doubtful. Physical philosophers tell us now that even in the inorganic world there are only statistical and no absolute laws, and the same thing applies to biology in a still higher degree. It is highly probable that this will be even more true in cultural history, where the human mind is concerned.

FORDE: The papers, I think, and the discussions of today show some lack of recognition of a point which our president presented very clearly in papers a generation ago, when he showed the application to anthropological studies of the recurrent dichotomy of understanding between analytical and synthetic presentation. It seems to me that we tend to fall into an inadequate and dogmatic position if we treat historical or evolutionary studies and studies of recurrent process or of minor necessary relations between events in the human world as antithetical, and present the position as one in which it is proper and correct, as I understand Professor Birket-Smith to think, to make historical studies of the total development of human civilization and yet to say that we are not going to sell our souls for messes of sociology and psychology.

Now, in fact, no historical study is ever undertaken without both a sociological and a psychological system in the mind of the person who deals with the records which he handles. I do not think that needs to be elaborated. The point is a fairly straightforward one, and, in fact, the advance in culture history looked at over a large field is not only an advance in the sense of recovering more material, it is also an ad-

vance in the analysis of concepts or in the formulation of concepts based on a much closer analysis of culture process and social relations in the universal sense.

Just to take a simple and obvious example which will be within everybody's historical memory, clans were regarded as solid objects in the late nineteenth century. They were thought to diffuse in the same way as handaxes. It is true, you could not see them, but you treated them by a false reification, as though the name "clan" referred to a tangible object.

I would suggest that the work of the last thirty years on the factors underlying, first of all, the formation of corporate groups in societies of all levels and, second, family systems in the human species . . . gives us a basis for analysis which makes the question as to whether clans diffused from one area to another very largely irrelevant. What diffuses from one area to another are certain formal symbols, which are, for the given population, diacritical or symptomatic of clan organization.

The antagonism of culture historians toward studies of process also seems to me to derive from a misreading of the position of evolution in biology. In the biological world, evolution is recognized as a historical study, but the refinements of evolutionary hypothesis have depended on the advance of knowledge of process. We have even had examples in our own field, at the human level: many evolutionary theories of the nineteenth century have been discarded as a result of genetic advances.

In culture history we are not so far forward, broadly speaking, as the biologists, in cross-fertilizing continually between the analytical and the synthetic or historical researches. To put it in a metaphor, we cannot tell bats from birds, and, because we cannot, we still wrangle indefinitely about problems of convergence, parallelism, or unilinear evolution, where we should recognize the fact that, until we have sociological and psychological techniques for distinguishing identical processes from similar processes, we cannot solve these problems. In other words, I think we tend to try to settle things much too fast.

STEWARD: My remarks will be more or less along the line that Dr. Forde developed. . . . Dr. Willey's paper brought out, I think, the general fact that you may approach history, and particularly archeology, from two points of view: (1) chronology, the time-space arrangements of the materials, or (2) what he calls the "processual point of view," which I would take to mean an interest in problems, perhaps of a sort that Dr. Kidder elaborated yesterday, in function, history, dynamics, explanations, or whatever you wish to call them.

Dr. Willey points out that a knowledge of the time-space relations is necessary to the processual relations. I would agree with that but also say that the latter is necessary to the former, because there are many problems of independent invention, diffusion, and the nature of culture growth in different centers which are left in doubt so far as the conventional methods of the archeologists are concerned, but which, I think, would be illuminated if a greater knowledge of process is introduced. . . . I have the feeling that, if archeological studies and, let us say more broadly, historical studies could be made more comparative by (1) taking hunches about process and development from other studies in archeology and (2) taking them from ethnological studies, then our understandings would be tremendously enriched.

I must say, in reply to the intimations of Dr. Birket-Smith and John Rowe, that I do not quite see where the data

automatically present you with theories or interpretations. It seems to me we have to proceed with hunches. They may be very bad ones, but they have to be tried out. Gordon Willey indicated a great many. For instance, in a cross-archeological approach, there are problems of the relationship between demography (the sheer size of population in connection with its mobility) and the settlement pattern and food supply; and of those, in turn, to the structure of the society. . . .

Again, to bring in an ethnological comparison . . . in the Southwest one notes that special forms of decorative pottery occur throughout the sites and in all burials, but in a high-culture area like Peru you find a contrast between status burials with precious objects and beautiful pottery and the plain ware in commoners' houses. I think you might make a hypothesis of a processual nature there; from that distribution of pottery, class structure could be suggested.

To come back to the point of how far you are going to generalize, yes, you will produce an abstraction, but either we retreat completely into the relativistic or we begin to abstract. I think we can do this in archeology, aided greatly by the understandings we are now getting from ethnology.

FETALIZATION

VALLOIS (Lévi-Strauss translating): There is a difference between what takes place in *Amblystoma* and what takes place in the anthropoid. In the case of the first-named animal, it just happens that the reproductive organs and the body reach maturity at the same time. In some cases the maturity of the body and of the sexual organs is reached at a much earlier stage, and this is what is called "neoteny."

In the case of man and the anthropoid, the situation is entirely different, because in both man and the anthropoid the sexual organs reach maturity much earlier than the body. The difference between the anthropoid and man in this respect is that the time differential is greater in the case of man than in the case of the anthropoid.

The additional time of growth in man can be explained because man is not able to reach full maturity in all his organs, and this is what is called "fetalization." But, if this is true for some organs, it is not at all true for the others. For instance, the brain is much more perfect in men than among the anthropoids, and if, in the case of body hair and teeth, there is an incomplete development in man as contrasted with the anthropoids, this is not true in the case of the brain or in the case of the premaxillary bone. This bone in man becomes fused to the maxillary bone much earlier than is the case with the anthropoid.

Formerly it was considered that the evolution from ape to man should be understood as a gradual transformation; and there were many consequent features of a "missing link" which were built up between apelike character and manlike character. This is so true that, when *Pithecanthropus* was first discovered, it was thought that the femur did not fit with the picture but belonged to another being. Then there is another case—that of Piltdown man—if we contrast the jaw with the skull.

In man the different bodily characters did not develop at the same rate and in the same way. For instance, the evolution of the femur was already achieved before *Australopithecus,* while the brain evolved between *Pithecanthropus* and *Homo sapiens,* and, in the case of teeth, there has been a continuous, gradual process of alteration.

HALLOWELL: Just as a matter of information, I would like to point out that Geza Roheim, the psychoanalyst, has drawn from the older work of Bolk and made a great deal of the point of

fetalization in man, taking this as one of his biological bases for certain psychological inferences. I cannot quote directly, but I think that what Roheim says at one point is that man is a fetal primate who has achieved sexual maturity.

THE DEME, RACE, AND LINGUISTICS

WASHBURN: I would like to go back to the point that Dr. Forde made, that, if we know something about function, we see these things differently in process. Let us take the premaxillary suture. Here is a small, detailed anatomical point. What good is it? Well, when the canine tooth grows in a monkey at a rate that Dr. Sarnat has shown, the premaxillary suture grows very rapidly in an exceedingly restricted period of time, making space for this big tooth. Dr. Oakley has already called our attention to the interrelation of tool-using and the size of the canine tooth. I would argue that when you see the condition of this suture in man, you are seeing a frozen osteological result of the sort of thing that Hallam Movius has been talking to us about: that the presence of the suture (the time of closure of the suture) is impossible to consider without knowing archeology and thinking of the archeology the second you look at the tooth.

The argument for the unified application of distinct branches of anthropology to this sort of problem is this: Who is going to think that the growth in this suture, as worked out by a plastic surgeon, is related to tool-making unless some group of people makes a real effort to bring these diverse lines of evidence together?

I would like to make two other points. If we take the idea of the deme as a key unit concept in physical anthropology, several of the concepts of race which are outlined in Dr. Vallois's paper are wrong. Dr. Vallois points out that some of these races are based on populations. He points out that sometimes the same worker uses two different concepts. Only the concept of race based on population can be reconciled with Dr. Carter's paper. This is an issue that the conference should face, and it has this implication for anthropology:

If you take the population concept, you are then concerned with a list of things which Julian Steward has already outlined—size of a settlement, the arrangement of the settlement, questions of breeding, food, disease, and ecology. If you take the type concept, you can run physical anthropology with no relation to the rest of anthropology. Carter's paper is the link between a genetic concept of race, based on population, and social anthropology, and it needs to be considered in that light. It relates even to linguistics. In teaching physical anthropology this year, the most helpful idea that has come to me is Greenberg's classification of the languages of Africa, because they show the interrelationship of a group of languages in eastern Africa, which goes contrary to the traditional thinking of physical anthropology and which fits a whole block of the information better than the physical-anthropological classification did. The incidence of sicklemia, a hereditary disease in East Africa, fits Greenberg's linguistic classification and not the traditional physical-anthropological classification. Who is going to know Greenberg and human genetics and anatomy except the anthropologist?

CHAPTER VI

Problems of Process: Methods

Chairmen: A. Irving Hallowell and Alfred Métraux.

Papers discussed: Lewis, "Controls and Experiments in Field Work";* Paul, "Interview Techniques and Field Relationships";* Henry and Spiro, "Psychological Techniques: Projective Tests in Field Work";* Lounsbury, "Field Methods and Techniques in Linguistics";* Greenberg, "Historical Linguistics and Unwritten Languages"; Murdock, "The Processing of Anthropological Materials."*

Speakers: Hallowell, Mead,† Nadel,† Fejos, Birket-Smith, Washburn, Eiseley, Henry, Beals, Eggan, Spoehr, Linton, Caudill, Voegelin,† Greenberg, Lowie, Métraux.

FIELD METHODS IN ETHNOLOGY

HALLOWELL: It was a major step forward in anthropology when, by the end of the nineteenth century, there was substituted for travelers' accounts an attempt to go into the field in various parts of the world and bring back fresh material. This well-known fact need not be elaborated, but I happened to run across a passage from the famous eighteenth-century writer, Jean Jacques Rousseau, who, in "A Dissertation on the Origin and Foundation of the Inequality of Mankind," raised points which might be worthy of attention, particularly in view of the climate of opinion in the eighteenth century, when all sorts of generalizations were made about man without specific investigations and certainly with a great deal of oversimplification. Rousseau writes:

"The inhabitants of Europe have, for three or four centuries past, been employed in running over the other parts of the world, and are constantly publishing new collections of voyages. I am nevertheless of the opinion that the people of Europe are the only men upon earth we are as yet well acquainted with. . . .

"The cause of this is manifest at least with regard to countries far distant. There are but four kinds of people who make long voyages; there are seamen, merchants, soldiers, and missionaries. Now, it is hardly to be expected that the three first should be very good observers; and with respect to the last, even were they not, like the rest, subject to the prejudices of their occupation, we may conceive they are too much taken up by the immediate duties of their sublime vocation to descend to engage in researches which may seem calculated merely to gratify curiosity; and which would interfere with the more important labors to which they are devoted. Besides, in order to preach the gospel with success, zeal alone is sufficient, the grace of God does all the rest; but to study mankind with success, talents are requisite which God hath not engaged to bestow on any man, and which do not always fall to the share even of the saints.

"We can hardly open a book of voyages without meeting of some description of character and manners, but it must seem surprising that these travellers, who describe such a multiplicity

of things, say hardly anything that any reader is not previously acquainted with, and that they had not sense enough to observe more at the other end of the world than they might have done without going the lengths of a street, while the really distinguishing features of nations, which must strike every judicious eye, have almost always escaped them. . . .

". . . I can hardly conceive how it is, that in an age wherein useful and polite literature are so much affected, there are not two men properly connected and rich, the one in money and the other in genius, both fond of glory and aspiring after immortality; one of which should be willing to sacrifice 20,000 crowns of his fortune, and the other ten years of his life, to make a justly celebrated voyage around the world; not to confine their observations in such a voyage to plants and minerals, but for once to study men and manners, and, after so many ages spent in measuring and surveying the house, to make themselves really acquainted with those who live in it."[1]

I should like to focus discussion on general questions which I have arranged in three categories: the aims of field work, the actual techniques of field work, and what happens to the material.

We first need to ask for what purpose field work is undertaken. What kinds of problems does the anthropologist wish to investigate? To what specific questions does he want answers? What kind of data must be obtained in order to answer these questions? And what are the best ways of getting them? Is his aim merely to secure a fresh body of facts, or does he have some problem in mind, some hypothesis to be tested? Can he or should he always formulate a research design? Is he an experimentalist in any sense? What theoretical model, if any, does he have in mind, and how does this model influence the selection and collection of data? Is it better to have a team of specialists, or should one investigator attempt to collect a wide range of diverse material? What is the significance of the sex of the investigator? Is a wife-husband team desirable?

Concerning techniques, we may ask how far field methods and techniques are a function of specific aims, problems, and interest in certain kinds of data. To what extent are there techniques common to the linguist and the cultural anthropologist or, perhaps, those who are interested in psychological problems or even to others? What are the techniques now in use? Are there new techniques that should be experimented with, or old ones that should be refined? What do we know about the validity of the data we use in relation to the techniques that have been employed to collect it? Is this something we should be more concerned about?

Problems of the handling of material I might put in the form of a simple question: Are there any ways better than those used in the past to present ethnographic material where we emphasize a tribal group as a whole? There is the old scheme in which you split everything up into a succession of topics; and a great deal of material is organized on this basis—Is this something that needs reconsideration? How can field data in cultural anthropology, for example, be ordered and processed so that it may be used with more effectiveness? Can we make use of sampling methods in the handling of ethnographic data? If we do not use quantitative methods, what are the techniques for handling large masses of

[1] *The Miscellaneous Works of Mr. J. J. Rousseau* (5 vols.; London, 1767), I, 292 ff. French edition: C. E. Vaughn (ed.), *The Political Writings of Jean Jacques Rousseau* (2 vols.; Cambridge, England, 1915), I, note on 211–12.

cross-cultural data? And to what aim are our processes related? . . .

MEAD: I would like to mention first what appear to be the grossest lacks [in the papers and the program on methodology under discussion. First, there is the complete absence of any paper on] physical anthropology in its broadest sense, including the collection of dietary, disease, and growth data. . . . [Likewise, there is lacking a paper on archeological methods for the ethnologist in the field. Second, the subject of field-work training for anthropologists is entirely neglected. Field work may be done in primitive societies or modern societies, but I wish to limit my discussion to methodology pertaining to the collection of material on primitive societies.] . . .

The first and most important requirement of any field method is that it result in material in a form as permanent as possible in terms of the changing theoretical interest and knowledge of successive generations. . . .

If we accept this requirement . . . how much information about how many aspects of the society do we have to bring back? We also have the difficulty of not knowing when we go into the field that there is an aspect that will be recognized years later. Obviously, analysis and collection of material in analytical categories will never adequately provide for these unknown bits of analysis, so that the problem reduces to collecting material in the largest chunks convenient, with the largest number of dimensions that one can include.

Practically, that means, if it is verbal material, very large masses that have not been directed and cut up by the questions that the ethnologist asks. It means that you want 50,000 words from one person, letting him talk about what he wishes, a large part of which may seem totally irrelevant for a hundred years or more and then may solve a problem that we are not yet able to conceptualize. You need a big chunk, not material carefully cut up, as when you ask an informant to tell you riddles, or another to tell you folk tales, and another to give you curses, then carefully put away somewhere so that it is impossible ever again to reconstruct the interrelationships that would occur in the mind of a single person if he were simply allowed to talk.

It means that when we deal, for instance, with a ceremony, we want as many different aspects of that ceremony as possible. We want a dimension of time, of food consumed, a spatial arrangement of the participants, the sequence of ceremonial events, the sequence of informal events.

If we insist on presenting a certain proportion, at least, of our unanalyzed ethnological work in chunks, we can have the best guaranty that it will later be amenable to analysis and meet this main basic requirement.

Now, Dr. Lewis suggested as a figure of speech that the ideal of field work at present seemed to be a sound moving-picture camera, moving throughout the field work. I would add that the ideal is not one but two sound cameras, because you want at every point as full a record as you can get of what can be seen and heard and, ideally, smelled and kinaesthetically and rhythmically realized, depending upon the special abilities of the field investigator, and you want an equally good record of the position of the observer at the time that he heard, saw, and smelled, which you can do only by *two* cameras. . . .

Ideally, one would add a machine to record what goes on inside the heads of all the participants and the anthropologist. Short of that, it would be most advisable if every field worker were to record his dreams. Probably all good field workers using the native language dream about the culture and use the material of that culture symbolically in

their own dreams. If a certain number of those are adequately recorded, we may be able to do something with them. . . .

The best discussion I know of the specification of the position of the observer has come from a political scientist, Lasswell, who has not himself done much field work.[2] We anthropologists have been exceedingly lax in our specifications of the point from which we observe something, how we got our information, and our own state when we got it. For example, it makes considerable difference in what he sees whether the field worker has a temperature of 104° or not. Another example: Iatmul natives never tell an anecdote of any sort without saying how the mosquitoes were biting, and this with reason, because the point of greatest importance at any moment on the Sepik River is how the mosquitoes are biting; hence any account by an anthropologist of any episode—a marital quarrel, a raid, or a ceremony—which specifies his position and specifies the native's position but does not include the mosquitoes is therefore incomplete. Such full specification will depend upon whether you are working by yourself, how many mechanical means you have for such specification, whether you can train native assistants to do things like recording of temperature—but all will increase the usefulness of our material.

In his paper Dr. Lewis considered to what extent an anthropologist can use devices worked out in the other sciences and how they can be fitted together. He did not discuss intensively the question of internal controls, that is, the extent to which we could use the method that the psychologists use when they split a systematic mass of data in half and analyze one half, using the other as a control. Anthropological material lends itself remarkably well to such a method. . . . [A sample could be divided], given to two separate analysts, the material analyzed in as many complicated ways as one wished, and cross-compared. That can be done with synchronically collected material. We do not have to go back later, we do not have to assume that one community will be like another community. It never will be, and I think that most of the methods that Dr. Lewis discussed are subject to the difficulty that these communities change so rapidly. . . . All anthropologists, archeologists, and linguists use this method of internal checking, but we have not raised it to consciousness as a principal method.

The whole question of projective techniques is of great importance at present in the field of ethnological or cultural-anthropological work, and ought to be looked at from more angles than those of Dr. Henry and Dr. Lewis.

First, one can regard projective techniques as a method of training anthropologists not neurotic enough to need psychoanalysis. Present experience suggests that psychoanalyzing people who would not be psychoanalyzed except for training reasons is exceedingly unsuccessful—people not correctly motivated for undergoing a therapeutic process sometimes can never put themselves together again. At the same time, it has proved very valuable for field workers to have access to some of the concepts of the Freudian school. Projective techniques are a way of getting them, whether or not one subsequently emphasizes the use of projective tests in the field. Much of the current respect for projective tests has come from the fact that anthropologists who have used the tests have shown familiarity with and an interest in a level of psychological research that they would not otherwise have been able to handle.

Second, can one find out anything

[2] Harold D. Lasswell, "Person, Personality, Group, Culture," *Psychiatry*, II, No. 4 (November, 1939), 533–61.

by projective methods that one cannot find out in other ways? That is still a very moot question. Dr. Klopfer, for instance, does not believe that you can find out with projective tests anything that a thoroughly oriented sensitive field worker cannot find out in other ways while also collecting material that is of some value to history.[3] Conceivably, the protocols of projective tests may be historically valuable, but they represent a very disproportionate expenditure of time among primitive people who are priceless to us in our whole handling of culture history. The most serious criticism that could be made of projective tests at present is that they take hours and hours, and after all these hours one has a series of responses highly formalized by the test itself. It has been demonstrated that some people have used projective tests who have not shown any signs of finding out the same things by other methods, but no one has yet demonstrated that you could find out anything by projective tests that you could not find out in other ways.

There is the further difficulty, raised by Dr. Lewis, that projective tests have not been validated on our own society, and anthropologists have used techniques which psychologists are not willing to support as testing the things that they are supposed to test.

To the extent that anthropologists publish only the results of projective tests, they are throwing away the only thing about the test that is probably permanently valuable, since it is hard to conceive that a thousand years from now the information that Klopfer's or Beck's methods of scoring Rorschach led to a certain sort of frequency for people of a given island is going to be very valuable. One point not raised in any of the papers is that if you are going to use projective tests, you must save the protocols and save them in a form such that they can be examined over and over again. . . .

My fourth point takes me to that made by Professor Birket-Smith this morning concerning the relationships between psychology and sociology, with which I most heartily do not concur. He said we ought to borrow from psychology but not have it in the same room, so to speak. We have got to communicate with psychologists and sociologists as a necessary form of the advancement of this kind of anthropological research, and projective tests provide an excellent way of communicating with psychologists. They are one form of language, of ways in which we can bring back our results, so that they can be understood by a group of people who cannot go into the field, cannot experience primitive society in any way. . . .

NADEL: . . . About twenty-five years ago, when I was a student of psychology, three books came out at the same time by different writers, all entitled *The Crisis of Psychology,* indicating what psychologists felt about the stage reached by psychology. We have now in anthropology reached the stage that might justifiably be called a crisis. . . .

The crisis has to do with the end of an era, a twofold end or the end of two different eras. The first (the era of discovery) refers both to what the chairman implied in his quotation from Rousseau and to what Margaret Mead implied when she talked about "chunk ethnology" or "chunk field work." I think the era of "chunk field work," when we were still very much the voyagers and the explorers with lots of blank spaces on the map to fill, is over. That does not mean that there are no

[3] B. Klopfer, Personal communication in the course of analyzing Rorschach protocol. See Margaret Mead, *The Mountain Arapesh,* Vol. V: *The Record of Unabelin with Rorschach Analyses* ("Anthropological Papers of the American Museum of Natual History," Vol. XLI, Part III [New York, 1949]).

blank spaces left or that some phenomena are not adequately known; but we have switched from exploration in the spatial sense to exploration in depth, analysis of a given culture in terms of underlying mechanism, processes, perhaps regularities or laws, certainly not simply discovery, accumulation, and faithful description of new facts. Of course, they are again new facts, but of a different kind—not "chunk facts," but facts that emerge from analysis. The best example of this is the need felt today for restudy, whatever the purpose: to verify, to incorporate changes, or to test our very methods and techniques. . . .

The second disappearing era is that of single explanatory concepts and of single methodological principles. We have had our share—evolution, diffusion, and then functionalism. We all realize now that different approaches and methods are possible and must be tried out, because each may offer some contribution which another method might not offer to the same degree. I agree with Margaret Mead that we must borrow techniques from other disciplines . . . varied and numerous as they are today. Besides our own techniques of life-histories, there are the T.A.T. and Rorschach tests, ordinary learning theory and perhaps experiments of the kind I carried out on memory; sociometrics, and small-group experiments; at least some group therapy and its methodological principles; and a great deal of flirting with social field theory and with communication studies and cybernetics.

Considering all these plus all the techniques and methodological principles that have grown up in anthropology, the situation is seriously bewildering.

Anthropologists today seem to be alienated from one another almost completely in viewpoint and interest; for example, social-structure studies by Evans-Pritchard or Fortes, Firth's *Malayan Fishermen*, and Mead's *Balinese Character* simply do not belong to the same universe of discourse.

What is our choice in this situation? We could specialize in one field and ignore the rest. Quite obviously, the whole tenor of this meeting is against that kind of attitude. A second alternative is eclecticism. That is a word that no one likes, perhaps because eclecticism is essentially unsystematic. The answer is that we must be more systematic about it, since, among all these different approaches, there is some common denominator of relevance. We spend a great deal of time, in our published papers and meetings, on trying to discover this common denominator, and, unless we discover it, I think the advance of our discipline will seriously suffer. . . .

This is precisely what I mean by today's crisis. And although we cannot possibly hope to resolve it, surely we can try to note the critical points and perhaps deal with them. Roughly, the situation has become too complicated, and we must try again to simplify. I do not think that this is a particularly pessimistic view or that we should lose heart. I recently attended a meeting of theoretical physicists and was struck by Professor Oliphant's statement that physicists are at the moment almost as bewildered as we are in anthropology and feel that something is shortly going to break to simplify a machinery and conceptual framework grown just too complicated to be handled.

To rephrase these critical points in very practical and realistic terms: I think we can reduce our crisis to three simple, although somewhat disquieting, propositions: (1) no single person can now master all the aspects of anthropological research; (2) no single person can now master all the techniques we have to employ, even when he has decided on the particular aspect of

anthropology; (3) no single person can even hope to master all the literature available in his own field of research.

As regards my first point, the first difficulty arises from an accepted premise, almost an axiom in modern social anthropology, that we must study society or culture as a whole . . . taking a broad view, that is to say, including a large number of phenomena and being very catholic in our assessment of relevance of the various facts met with in our field work. It follows that we must examine all the classes of behavior which we have come to separate off as relatively autonomous provinces in social life, such as economics, religion, political science, and so forth. . . . In addition to these broad sectors of culture or society, we have also certain pervasive aspects or dimensions, such as group structure, idea system, value system; we have the longitudinal approach of socialization and infant training, as well as the deeper-level approach, the type of analysis which psychologists have evolved. In fact, most of our studies are uneven, and even the great prophet of studying culture as a whole, Malinowski, has never done so. The most holistic anthropologist usually omits language, which he leaves to the linguist; music, which he leaves to the musicologist; often art, dance, literature, and other things as well. This is not to say that we have grown clumsier. The fact is that when, in earlier days, one monograph covered everything known about a culture, gaps still occurred—you very rarely had any treatment of economics (only technology, as Herskovits has pointed out), and, instead of a treatment of law, there were crime and custom or similar things. You certainly had no discussion or analysis of child training or the periods of socialization. All this seems to imply that there is a certain limitation to our intellectual factualism. . . . If one has to cover all these fields, one obviously has to be equally qualified to deal with each of them.

The second difficulty emerges from the orientation which I referred to as an analysis or exploration in depth. Formerly, we were interested in describing the typical behavior of a group, the qualitative character of the customs and institutions, the norms which would summarize a social organization. The resulting picture satisfied the anthropologist and his public. This is no longer true. The emphasis today is on the variability of any kind of norm that you might think you have extracted, and problems like sampling become of paramount importance. Margaret Mead's previous reference to the many observations that are necessary and the split half-proof that psychologists use for testing surely implies the same thing—numerous observations out of which statistically you get some sort of regularity, with deviations, of course. I do not intend that every book should be full of statistical tables, but the preliminary study going into any kind of written formulation must be essentially of a statistical nature that takes into account proper sampling. Also there must be some kind of numerical and quantitative analysis which enables us to say of a particular mode of behavior that this is typical to such-and-such a degree and there is such-and-such a range of variability.

A further very realistic difficulty is the time factor involved in anthropological study. For ideal field work the investigator must have adequate linguistic familiarity with the culture he studies, and for at least three months much of the information obtained will be useless because his linguistic qualifications during that period are inadequate. Also, I have just referred to sampling and statistical problems; the preliminary survey that is necessary, even in order to think in terms of samples, takes a very long time. . . .

Then there is the justified demand that the anthropologist should observe at least a full year's cycle, after he is sufficiently qualified in linguistic and local experience to profit from that year, in order to see all the various agricultural and technological activities —and quite often a year's cycle is inadequate because there are rituals which do not take place every year. . . . But the most important point in doing field work, I feel, is to work in terms of hypotheses, after a brief period of familiarizing yourself in a broad survey manner with the facts that face you. Here I disagree with Margaret Mead's "chunk" ethnology. Although I agree that every field note that you ever take must be kept, I believe in organizing ethnology from the beginning around problems and hypotheses; but it takes time to formulate hypotheses and even longer to verify them. Sometimes they are never verified, and you have to start from scratch. Add to this a point on which I feel strongly, the desirability of field work in more than one area. I know that this is much more the custom in the United States than it is in England, where a great many field workers have worked fully in only one area. Evans-Pritchard and I agreed in discussion, once, that two contrasted areas are the absolute minimum, four are the ideal, and three are desirable. . . . Well, what does it all add up to in years? I do not think you can do it under four or five years. Work out for yourself the additional time needed to write up the material. This is only one of the difficulties to be considered when we as social anthropologists are asked also to concern ourselves with archeology, prehistory, paleontology, and the rest.

What is the solution? The first and obvious one is teamwork in the field, so that people with different biases or interests and certainly different training can cover some of the fields—economics, religion, political organization —at the same time. . . . This is not a bad solution, provided that it is kept in mind that the members of the team must have a common briefing and training so that they may talk the same language and see things and problems in a common light. . . . Another alternative is conducting studies of limited scope, such as of political organization or religion. Such studies need not lose in value simply because they are partial. A well-trained anthropologist who keeps the other things in mind can get enough information and cross-confirmation not to distort the picture of his sector of concentration. But this kind of specialization quite obviously runs counter to anthropology's main contribution to the social sciences, in that it is a synoptic science and constantly mindful of the interrelation between the various sectors or aspects of culture.

I am now trying out on the highlands of New Guinea a plan which is, perhaps, a compromise between the two alternatives. It begins with a rough preliminary survey of the culture distribution and the degree of uniformity that might be expected. Then, one area will be studied intensively, and from that study certain hypotheses should emerge which will demand further analysis or comparative data, which would then be obtained by other people working in adjacent areas, still within that broad cultural unit. The advantage is that a great many of the aspects of culture can be studied in the one sample community, while in the other communities you can concentrate more on the specific aspects, which you can deal with much more briefly. If there is any doubt, the sample area may be referred to again to find out whether it is really the same or whether you have to branch out into further studies. . . .

There is a fourth method, repre-

sented by the Navaho project from Harvard, where a battery of inquiries and a whole army of inquirers are let loose on one community. The advantage of this study is that it operates with a sample, or a model—that is the only way I shall use this term—on which different kinds of approaches may be tried, thus resulting in different hypotheses which might prove to be leads in studying entirely different communities. The same, of course, holds true for techniques and methods. For training anthropologists and working out techniques, this method, which avoids wasting much time, is probably the most economical.

To my second difficulty—about different techniques—teamwork is, again, to some extent the answer. This is especially true, and here I perhaps disagree with many people, with regard to psychological techniques. I am still very much impressed by warnings voiced some time ago that the Rorschach administrator and the interpreter should be a single person, doing the field work in as short a period as possible. So much in the Rorschach approach demands the clinician's particular experience that it cannot be reduced to any standardized techniques, while an anthropologist who goes out, gives the Rorschach, and brings back the report for blind interpretation is in danger of losing a great deal of valuable information. Much of this is true of any ordinary projected test, where you want a trained psychologist to give the test and assess it, appreciate all the features which we very rarely get in our protocol of the reaction, of a diffused reaction, of the experimental subject, and so forth.

So much for the technique. Now, another point: Do we really know precisely what we want projective methods for? Margaret Mead has clearly stated that we are still not very certain about this . . . and I think we cannot afford to disregard the inquiry. It may be that projective methods will only offer confirmation of what the anthropologist can, by means of his customary field techniques, discover. They may, however, offer us the kind of information that the anthropologist cannot discover, and I am thinking here of two different things. One is the distribution of personality types in the strictly psychological sense. I am not talking about the basic or modal personality, but the distribution of different types obtained from a battery of tests applied to various aspects or fields or areas of the personality. The second point is that projective techniques might show you facts entirely opposite to those inferred from your cultural analysis. Margaret Mead has said this possibility has not yet been established. Well, it has not been established in print, but I have established it in a series of psychological experiments of my own, only part of which were published, at a time when I simply did not see that problem. I shall say only that in the reanalysis I have been struck by the fact that there is certain information of a psychological nature which simply does not tally with the anthropological information. In Henry and Spiro's paper there are twenty-four summaries, and in fourteen of these summaries it is stated that these particular tests showed the ethnographer's observations to be confirmed by the test; five are conflicting; five are doubtful. The comparison is not a very even one, because the numbers vary quite widely, but what does this conflict mean? Is the test no good? Is it extraordinarily good? That is to say, does it lead us where ordinary observation does not lead us? Let us hope for more cases where tests, properly administered and supervised by a psychologist, will bring out material which conflicts with what we as anthropologists have found. Then I think we will get somewhere.

This, my last point, is in many ways the most serious. I have already said that no one can possibly follow existent literature on the field. . . . What are the remedies? One is the cross-cultural survey, but I think even Dr. Murdock will agree that it cannot solve all the problems. It is a most valuable means for getting quickly and easily at cross-cultural data, . . . but it will not help very much in keeping abreast of theoretical and methodological discussions going on all the time, quite apart from contributions of a monographic character. Then, again, the era has gone when somebody could write seven volumes on the Trobriand Islanders or a 700-page volume on Tikopía. No publisher would take it; and, anyway, that kind of leisurely documentation is no longer as relevant as it looked then. We are gradually drifting toward a situation when we shall have to present only the bare bones of our findings, keeping out notebooks locked away somewhere. I regret that this means a sacrifice of the aesthetic aspects of writing, but I see no way out. Our present way of describing a society is dangerous, in that synonyms are often mistaken for new concepts . . . and wasteful in the sheer time that must be spent reading all the books that appear.

Now how can we do this? I envisage a situation in which we shall work with symbols like those used in symbolic logic. Actually, the relationship is a very close one. I might point out only that some of the basic categories of symbolic logic are the same as those we would use in any cultural or social analysis—for instance, inclusiveness or exclusiveness, the concept of class, the "if . . . then" formula, the "if . . . then" clause, which give you all the correlations you want. Modal logic deals with probability, the likelihood of a thing's being according to a particular formula. Whether this or another is used, the time must come when we reduce description and analysis of culture to some such simplified and nonaesthetic terminology and some similar system. . . . I am sufficiently optimistic to hope that out of this present gathering might come a body of men ready to try to work out some such bare-bones system. It cannot be done in the abstract but must be done around a problem which serves other interests at the same time.

I am going to make two concrete proposals which I have been making in vain for the last three years. My English colleagues did not bite. . . .

One is a comparative study of a problem as concrete as we can make it, to be undertaken by a number of people, each of whom will consider a particular cultural area or regional area. The problem I thought of was age-sets or age-grades—concrete in the sense that it has a definable biological basis—studied from a comparative angle to help us solve once and for all that constantly recurring and, to my mind, completely meaningless question, Can we discover regularities? I think we can demonstrate what the regularities are, in this case with regard to the organization of age-grades in their various types as correlated with other social phenomena. But we would also take the opportunity of technically presenting our material in an abbreviated, succinct, and symbolic form.

My second proposal is this: we badly need in anthropology something along the lines of the yearbook, *Psychology of 1937*. We need a book that, year by year, will summarize the ethnographic work that has been carried out—the mere factual material, the theoretical advances made or our hypotheses, and the conclusions that have been suggested. I have no objection to giving the physical anthropologists and archeologists a look-in, since I still think that we shall not be able as cultural anthropologists to think at one and the same

time with three or four different brains.

If either or both of my proposals are accepted, then I think we shall stand a good chance in that frightful struggle for survival which modern social anthropology has become. The sacrifice will be considerable, but I see no alternative, short of decreeing a periodical conflagration of all the libraries of anthropological books, so that we can start from scratch.

FEJOS: Dr. Mead has mentioned that it would be a good idea to use two sound cameras in field work. I do not believe the number of sound or silent cameras is a deciding factor in the quality of the evidence which the ethnologist will gain. Cameras do not necessarily record truthfully but are very great liars if the men behind them cannot operate them properly. More important is whether or not the native is conscious of being photographed; possibly 98 per cent of the photographic material we have today in anthropology and ethnology is posed. If the anthropologist wishes to record worth-while evidence with motion pictures, then he will very likely not only photograph ceremonies, dances, and pot-making, but also stress situations, interrelations of people, etc., none of which can be truthfully depicted if the native knows he is being photographed. Photographing in this fashion is not so impossible as it may seem. Hollywood long ago discovered that a piano crate with a tiny hole in it, on a truck, is an excellent way to "shoot" people on the street, without their knowledge, and for at least some fifteen years patients in psychiatric institutions and elsewhere have been photographed for the benefit of the medical profession with the use of two-way mirrors—a technique also used by Hollywood. Similarly, one could hide a microphone in a native house for, say, eight hours, to try to get an example of the total content of conversation during a limited period. If we were lucky enough, we would even learn what the natives think of the ethnographer, which would not be a bad idea. The most important point is not to falsify the subject's attitude by putting him under tension. It is very important that for a true record, total interviews—including the words of field worker(s), interpreter(s), and informant(s)—be recorded. These would expose the great number of leading questions which we direct at the natives, to which we always get assentive answers. . . .

BIRKET-SMITH (statement revised after session): I am sorry that Dr. Margaret Mead has misunderstood me. I do not object to being in the same room with psychologists. We can get valuable assistance from the psychologists, but I want to emphasize that we should not turn ethnology into psychology.

Although Dr. Nadel calls himself a social anthropologist and I call myself an ethnologist, I agree with him in his realistic and somewhat pessimistic view regarding the collecting of data. If for no reason other than time, it is necessary to distinguish between what is essential and what is not. Just imagine what the reading of the history of the United States for the last century would be if *all* data were collected. The reading would occupy one hundred years multiplied by the number of individuals at any time.

WASHBURN: . . . On the question of publication, one does not want to forget microfilm, microcards, and all these other techniques. If it is necessary for the printed volume to be a brief skeleton, still it is possible inexpensively to make available to the profession all these other things more extensively than they have ever been available before.

I would like to disagree with Dr. Nadel very strongly in his general position. I think he has confused the

position of the profession with that of his speciality. Medicine, for instance, is infinitely larger than anthropology. There is an infinitely greater amount published. No doctor feels that he must read everything that comes out in a year or, if he cannot, that medicine must subdivide. You cannot even do that in a specialty. Nevertheless, there is a basis of general education in medicine, there is a series of common problems, and there is a constant shifting back and forth—anatomy may be more important at one time, biochemistry at another, psychology at another. Here is a profession larger than ours, more subdivided, with more techniques which are not understood throughout the profession; nevertheless, it gains a great deal by maintaining certain general professional organizations.

It is at that level that I view the unity of anthropology as extremely important. Specialty problems must be solved by specialists, but the way should be kept open for synthesis at a later date.

EISELEY (submitted in writing after session): Regarding Dr. Nadel's suggestion that symbolic logic be substituted for "literary presentation," anthropology, if it is to be known to a public increasingly willing to support its activities, cannot afford the obscurantism that so often results from overelaboration of technical jargon.

HENRY: I wanted to say a few things about the problems of projective techniques. Of interest about the work that has been done with the Rorschach test is the variety of methods that have been used, and the clinical psychologists become very much concerned about this. The Rorschach test is sometimes administered in the native language, sometimes in English, sometimes through an interpreter. Sometimes the test is interpreted by the man who gave it; sometimes somebody else does a so-called "blind" interpretation; sometimes the ethnologist sits with the Rorschach interpreter, the clinical psychologist, while he makes the interpretation. These various methods bring very different results, and the purists (and clinical psychologists are quite purist) raise considerable question as to the comparability of the data. With respect to the relationship between the findings of the ethnologist and the Rorschach findings, Dr. Nadel said that 14 of the summaries indicate that the Rorschach findings confirmed the ethnographic findings. This, rather than reassuring me, causes me considerable concern, because . . . I simply do not understand their great mutual confirmatory character except in terms of communication between the ethnographer and the Rorschach technician. . . .

Neither the Rorschach nor any other psychological technique can substitute for direct observation of the human organism functioning in its natural environment. The more we turn over our anthropological observations to simple use of clinical techniques, the more we surrender one of the most powerful instruments we have for discovering new things about human behavior.

HALLOWELL: . . . A little more checking has been done than has been indicated. Dr. Klopfer and I long ago made a rather systematic comparison —never published—of the cases in which I used an interpreter and those in which I did not, and, although I do not say this is conclusive, we found no difference.

BEALS: . . . It happens that about three days ago I had a conversation with Klopfer in which he was very definite that the Rorschach will at present tell you nothing that you cannot learn by other techniques. I think he looks upon it as an instrument which, once perfected, may provide some useful shortcuts in getting certain

kinds of things. But I do not think he would say that Rorschach technique was anywhere near perfected at the present time.

In a more general vein, it seems to me that much of the discussion, particularly by Dr. Mead and Dr. Nadel, really points to the problem that we need many kinds of anthropologists. . . . We are going to have generalists and specialists and people who do all sorts of different jobs. This raises the question of training. For instance, many of our people need to have statistical training, but they do not need to be statisticians. They need to know enough to know the resources and possibilities of statistics as well as the limitations, which sometimes are much greater than the layman thinks.

The problem of the team versus the individual in research is simply a question of how you orient problems. There are problems that individuals can do, there are problems that take a team. The kind of team you have depends on the kind of problem you have. These are not insurmountable obstacles to our progress but are simply the recognition of increasing complexities in science and the developing of new techniques to meet them.

EGGAN: . . . Perhaps we ought to go back to Rorschach himself, who was interested in his test as an instrument of experimentation. The clinical psychologists use it because it seems to work, but recent experiments by some psychologists have suggested that somebody ought to do a good deal more experimentation. A psychologist at Chicago recently separated out the Rorschach outline, the plain black filled-in forms, the regular forms with the color, and regular forms in black and tried them out on a controlled hospital sample; he matched them according to the degree of abnormality and so forth, and got practically no statistical differences. Color shock, for example, does not seem, on the basis of this controlled experiment, to be what the clinicians think color shock is, so that, if the clinical psychologists will not experiment with it, why, perhaps some of the anthropologists had better.

SPOEHR: . . . What is the impression that Dr. Henry and Dr. Beals have as to whether the use of the Rorschach tends to set up an actual barrier between the observer and that which he observes? In reading a number of accounts where the Rorschach has been emphasized, I sometimes have a feeling that attention tends to become misdirected, that the talents of the observer get into the wrong channels, and that there is an actual methodological impediment set up.

HENRY: . . . Possibly what Dr. Spoehr has in mind is a tendency to put one's interpretations into pre-existing categories. This is probably a real danger. If one goes into the field with only this instrument, one comes out with only what the instrument yields, and the great wealth of day-to-day activities of the human organism in its natural environment is lost.

The other problem is whether you break rapport. Any ethnologist knows that he should not give the test until he has pretty well established himself with the people.

LINTON: We are missing one important point, that is, the old question of whether anthropology is to be regarded purely as an art or whether it is also to have certain scientific pretensions. I do not doubt for a moment that any sensitive individual can get a rapid picture of the personality norms of the members of a particular society. The purpose of the people using the projective tests and other types of exact checking is to bring back the sort of evidence upon which someone else can also work, and to see whether they arrive at the same conclusions. There is no question that the personality of the indi-

vidual is going to have a considerable effect on what he thinks the people are like. Even if you have a certain individual psychoanalyzed and have a complete record on his personality, you must remember that the people you deal with in the field are not abstract symbols but people, and a particular individual is going to make a particular set of friends through whose eyes he will see the culture and from whom he will draw conclusions as to what the people are like. . . .

CAUDILL (statement revised after session): 1. Nadel misunderstood Mead's "chunk" ethnology. She meant the need for detailed study and reporting of interaction of specific people at specific places and times *over* time (cf. Chapple's paper). If we are to learn about *process,* we need this direct observational material with *exquisite* attention to detail.

This does not, in my mind, mean getting everything. It does mean that, if you are interested in kinship, your basic field documents consist of a complete reporting of what *you* said and did and what the informant said and did, etc., *and* detailed reporting of *actual* interactive behavior related to kinship over time—i.e., over minutes, hours, days, etc.

2. On projectives: First, the term "test" is misleading. Projectives (and particularly the T.A.T.) are, or can be thought to be, *interview* techniques. Among Ojibwa and Japanese Americans I found that the T.A.T. would, after it was over, get people to talking freely on topics, and at deep levels of material, which would ordinarily take 10 sessions to get to, *and* without much anxiety or trauma on the part of the subject because, in part, of the convenient fiction (known to both the subject and you) that it was the pictures he talked about, and not himself. This has complicated psychodynamic reasons, which cannot be considered here.

Second, projectives are *not* that time-consuming. They allow you to gather a type of material, *and very importantly,* in a systematic and hence comparable fashion that would otherwise take much longer.

Third, the anthropologist has to *learn* how to give and interpret these things—and, beyond this, his interpretations will be only as good as his *theoretical* psychological training and his clinical insight.

Fourth, the anthropologist does not *have to* interpret the protocols psychologically. He can—and it is a good way to get much material—use the record to get at values, roles, etc. I have devised a series of pictures of everyday life-situations in the hospital occurring on the patient level, the nurse level, the doctor level, and levels in interaction (e.g., pictures of nurses and patients together). I interpret these protocols at three levels—go through the record three times if you will—for psychological dynamics, for the subject's role on the ward *and* how he sees other people in theirs, and his *values* regarding various aspects of the hospital. These protocols serve as a *check* on my *very* detailed observations on the ward over time (e.g., 400 hours of detailed patient-patient, patient-nurse, patient-doctor, on a disturbed men's ward). Concretely, if you observe that nurses are *not* on the ward much, you look at what nurses and patients say to pictures of nurses and patients, and you find out many reasons *why* nurses are not on the ward much, much about their attitudes toward patients, and patients' attitudes toward nurses.

3. To my mind, we no longer study simply personality in culture, but the interactive small group in the culture over time; we study, as Strodtbeck does

in three southwestern societies, husband-wife interaction *in* the culture.

MEAD: . . . The point I made about a chunk of material has been totally misinterpreted through most of the discussion. I meant that we want as many simultaneous records as possible on material *in situ,* not that we should have a complete record. I used the sound camera as a figure of speech, not to recommend that we do that but to say that our ideal was to record the observer as well as the observed. When I said that we could get our information as well or better by methods of historical value other than by projective tests, I did not mean subjective methods, as Dr. Linton's remarks would perhaps lead one to believe. I meant by moving pictures, a kind of multidimensional observation of behavior—what Dr. Henry referred to as detailed, precise observation of behavior—which also has historical value.

LINGUISTIC FIELD METHODS AND TECHNIQUES

VOEGELIN: This year I gave a course in field methods for the first time. I remembered beginning the course with some nervousness. I started by telling . . . about my first field trip. I described how I went to Dr. Kroeber quite playfully and asked him about field methods. Dr. Kroeber spoke of the different makes of cars and concluded that his preference for field work was a model-T Ford. I realized he was teasing, so I thought I would catch him and I said, "Not to get there —but after I get there, then what do I do?" Then he recommended certain kinds of frying pans. . . . In other words, you did not have the kind of interest we have now. Now we give courses in field methods. But what do you give in such a course? What do you say specifically on field methods in linguistics? Lounsbury in his paper did not quite know how to start. . . . I feel with him. He started with orthography.

Orthography seems a perfectly natural thing. How do you write down what you hear? I will give just one example of the orthography used for writing the initial consonant in words like *ship.* In English orthography we use two letters (*sh*), three letters (*sch*) in German, and the American Indian tradition, when I was in Berkeley, was to write one letter, *c* (so *ship* in this latter orthography would be written *cip,* with an undotted *i*). This looked queer to a pleasant Dane who was teaching us phonetics, and he said, "This is nonsense of course. You write a long *s,* the way Daniel Jones does. That is the way Daniel Jones does. That is the IPA system." So I started writing thus.

Then, postdoctorally, I was to go to Yale with Sapir, and Sapir was corresponding with me and I gave him some forms from an American Indian language, using the IPA long *s.* He simply blew up. There was correspondence, and I reverted to *c* for a little while, but I felt guilty about deserting my Danish teacher.

At Yale we had a conference—Sapir, Herzog, Mary Haas, Newman, Swadesh, and myself—and we changed from *c* to *š.* Before then, the *s* wedge was associated only with Slavic languages. Now, *š* has won the day, and I wish to report a sort of personal triumph on this trivial matter. I have persuaded Gladys Reichard to use *š.*

This has nothing to do with phonetics. The question is, what shall we do with field methods? Shall we tell true stories like this or play around with other irrelevances?

Lounsbury gives methods and techniques, starting from the simplest and crudest and proceeding to the finest and most complex, and says that any degree of linguistic proficiency other than absolute zero is provided for in a

series of techniques. I do not know whether we ever really succeed in giving techniques in a field-methods course other than saying what we do. The advantage in giving the course is that we categorize the things we do.

The first thing we do is to PHONETICIZE, and I would go just a little further than Lounsbury and say: we can have absolute zero in linguistic competence and still do some things. . . . To begin with, as Lounsbury points out, phoneticizing can be done relative to English consonants, relative to Italian vowels, or something of the sort. But, of the three ways of phoneticizing now followed, the articulatory one is most followed. You can write down the sound *s* in terms of the motions of the muscles which produce the sound *s;* hence articulatory.

The second way of doing it is acoustically. . . . In effect, you can count the actual vibrations and have an analysis made.

The third method, which Dr. Lounsbury did not mention, is what Eli Fischer-Jorgensen, of Copenhagen, told us about a few weeks ago—perception phonetics. How do sounds appeal to the native speaker, whatever the articulatory or the acoustic phonetics may be? These three ways of phoneticizing, or at least two of them, are absolute, rather than relative to a given language.

Then you have PHONEMICIZING. Many of the problems that were considered very important at one time turn out to be less important. Phonemicizing makes hearing much easier. You listen to the things which are relevant in that language. Much of the fuss and bother, for instance, in California as to whether Dixon heard the Maidu implosives now seems unimportant because it turns out that the implosives are not phonemic. They are not distinctive.

The method of stating the phonemes, whether they are stated as uni-phonemes or components, such as this component of implosiveness, is an important point in phonemics. . . . If you miss a component that occurs as a distinctive sound of the language, but is associated with a whole series, you are obviously missing quite a lot of that language.

MORPHEMECIZING is described under various headings—under two headings by Lounsbury. It is a very tricky business. For example, missionaries, to whom Lounsbury is so generous, have, as he says, preserved many languages for us phonemically. But I have not as yet found any missionary work that really morphemecized a language. It is very hard to communicate this. Much of the mysticism of form and meaning goes away when you morphemecize.

Next is SEMANTICIZING, and this point I will discuss later in another session.

On GENETICIZING, I repeat what I mentioned yesterday, and that is, you can do it without proof with what may turn out to be probability—merely probability—such as Greenberg has done. Lounsbury describes geneticizing not in terms of Greenberg's work but in terms of Swadesh's earlier work. Swadesh also introduces chronology—this I . . . think it is premature to discuss. . . . This method works chiefly with probability; it does not give you anything on process, and I think it lacks a mathematical sort of proof, although that can be controverted.

Then you have geneticizing where you get process in comparative grammar, the comparative method; this is the sixth method. In the list on the blackboard, you will note that I put a cross by the side of this kind of GENETICIZING to indicate that apparently it is considered in some quarters to be a dead duck. But if one is interested in process, this does give you process.

Finally, the label for my seventh and last point is a word analogized from

communication—COMMUNICATIZE. . . . This is called, in Lounsbury's paper, "dialect distance.". . . I do not know who invented this term. As long as I can remember, I have heard of some sort of testing the informant as opposed to asking the informant. Before the last war, I read a paper at one of the Linguistic Institutes concerning a count of 150 native languages in North America north of the Rio Grande, where we have good records on them. Sturtevant was furious. He said: "We cannot tell how many European languages there are when we have hundreds of linguists studying them, and then along comes one of these anthropologists and tells us how many American Indian languages there are." We kept kicking that around all summer. Bloomfield was on the side of counting them. I did not know how to handle dialect distance, but everybody spoke of testing it in various ways. I did not even know how to discuss it then, but there was really nothing complicated about it. It was counting languages by asking native informants, "Do you understand the language over there?" The count was made on the basis of native speakers judging whether or not other languages were separate languages. Harris and I proposed testing for dialect distance and sent some students out for Algonquian and for Iroquois languages.

There are two ways of seeking dialect distance by testing: one, by testing dialect distance by transfer information. This requires no linguistic competence. The second way to test dialect distance is to measure by morphemic sameness, that is, to see whether the morphemes of two dialects are quite the same. That requires some linguistic competence. You have to be able to identify a phoneme, a morpheme, and so on. After sending the students out, the idea of checking occurred to me—comparing the results of dialect distance testing against the judgment of specialized personnel who have worked in the same general area in the normal way.

One set of students who went to the Iroquois came out with a result which agrees with Lounsbury's own feeling as to how the Iroquois languages are divided between dialects and languages, and disagrees, for example, with my account made on the "ask the informant" basis, published in *American Indian Languages Still Spoken.* The second set of students' results agree with Michelson's and my specialized linguistic judgment on Algonquian languages. Plans are now afoot to apply this transfer of meaning in dialect distance to people learning English from a foreign country. If the information is transferred to a certain percentage, then you are that close in dialect—or in learning a second language. You do not need to take a related language; you could take, for example, Hungarian and German. If you had a common scientific vocabulary with Hungarian, you could then say that you had a certain percentage —however low a percentage—of mutual intelligibility.

Hungarian and Cheremis are related, but, inasmuch as there are no scientists among the Cheremis, you might actually have lesser intelligibility with Hungarian and Cheremis than with Hungarian and German; so this cuts across any genetic classification.

It might even work in the matter of learning another language. If, for example, you want to test my knowledge, of, say, Turkish, you can take an arithmetic average and say my phonetics are this good, my syntax is this good; but this does not say anything about how well I am doing. The holistic approach, saying I have transfer of information of 60 per cent of Turkish into English; then, after I have lived in Turkey for two months, I have 70 per

cent transfer; and so forth gives a tangible measure to correlate against cultural participation—it gives something interpretive.

GREENBERG: With regard to some of Dr. Voegelin's remarks on Dr. Lounsbury's paper . . . stage 5, he says, is to geneticize, that is, to set up a hypothesis as to which languages have a common historical origin. Stage 6 would be historical reconstruction of a protolanguage, I assume.

VOEGELIN: They do the same thing.

GREENBERG: To my mind, 5 and 6 are very intimately connected. The linguist does essentially only 5, because he has to do that before he can possibly do 6. Unless you decide that French and Italian are very probably related, you cannot begin to construct a protolanguage from which they both descend. Now, then, why say there is no process in 5? It is no defect of this method that it does not involve process. It is like blaming a bat for not having legs. When you have set forth your hypothesis of relationship, when you proceed to reconstruct the protolanguage and, by getting this process of change from one to the other, we discover what the processes of linguistic change are. . . . I submit that any empirical proposition is probable. The question is the degree of probability. . . . Again, . . . it is assumed that I am a partisan of fundamental vocabulary. I am somehow tied up with Swadesh in this matter of vocabulary counts. I want to make it perfectly clear that I have never decided that two languages are related because they had a certain percentage of vocabulary resemblance. When I worked on the more than eight hundred languages there are in Africa, I did not make a single vocabulary count, and that is also true for other areas in which I work. I did not arrive at a hypothesis on that count. . . . At a certain point, I begin to be intuitively certain. Perhaps other people are not intuitively certain that a relationship really exists. What is the sensible point at which to assume we have proof? . . . What I did was to decide what is the logical point, and I think it is: significantly more than chance relationship. What I have set forth in my paper is simply an attempt to determine what chance relationship is.

PROCESSING OF MATERIALS

LOWIE: . . . Dr. Murdock has made an excellent summary of the procedures to be followed in utilizing anthropological material. I should merely like to add a few details.

As regards the utilization of materials in foreign languages, it is obvious that a great deal depends on the quality of the translator, and we have some grievous examples of how one can be mislead. . . . [For example, an anthropologist reviewing a translation of a document on the Boxer Revolt in China found the English words "building-mast." When he referred back to the original, it was apparent that the translator had arbitrarily split the German word *Baumast.*]

It is not only with translators who are ignorant of the language in question that one has difficulty, but also with a person who knows very well the language which he is translating but who is not acquainted with the subject matter. . . . I might also say that, so far as existing translations of important ethnographic works go, very curious things occur [one of which is complete omission of parts].

The difficulties that confront the student because of the numerous languages he has to control or at least utilize in some fashion may be illustrated by the case of anyone who wishes to make a thorough study of the Lapps. It so happens that the Lapps fall not only into Norwegian and Swedish territory but also into Finnish and

even into Russian, since there are Lapps on Kola Peninsula. The consequence is that, if anyone wants to become a specialist on the Lapps, he ought to control all this literature, as well as what is written in more generally known languages. . . .

In the conclusion of his paper, Dr. Murdock refers to the rapidity with which it is possible to do work with the aid of the records now accumulated at Yale. I think the example he has chosen is probably a very good one. On the other hand, completeness of information would be necessary in order to make this kind of approach really satisfactory in the long run. If you have only one sample, let us say, for a large area, you could never feel that this was definitive. I understand that you intend to enlarge your accumulation of materials, so that they would be more and more representative as time goes on. This is simply a temporary deficiency.

MÉTRAUX: When I was listening to these different presentations of what our field methods should be, I asked myself this question: Why, after all, do we discuss these methods, if we are not clear in our minds about the purpose of anthropology? I am exceedingly naive, perhaps, in asking this question, but as long as we do not know or are not very clear about what the use, the purpose, and the contribution of anthropology are to our civilization, I do not think we can be clear about our field methods, and I would like to see during this conference at some time a definition, an exposé, of what the aim and purpose of anthropology are. I think, perhaps, we could collect very different opinions.

On the other hand, I disagree strongly with Professor Nadel when he berates some of his colleagues for wanting to go into the unknown. The point of view of the anthropologist is colored by the field in which he works. If Professor Nadel, instead of working in Africa or Australia, had worked in South America, where we have so many tiny tribes that are vanishing very rapidly, he would see that there is still a margin for the kind of exploratory anthropology which is, I am afraid, no longer fashionable.

Moreover, during the discussion of teams, no mention was made of teams consisting of natives of the countries in which the anthropologist works. More and more our field work is concerned with acculturated people, and among them we can find people who have no training in anthropology but who are technicians. . . . In my recent field work in Haiti, where I could not find any trained anthropologist, I found all kinds of technicians who were invaluable. I had an agronomist, who spoke the language perfectly, who was a native of the country; I had a social worker; and so on. I even had doctors. These people can form an excellent team. The role of the anthropologist is to tell them what to look for, what kind of questions to ask. And the anthropologist learns a great deal from them because, in their respective fields, they know more than the anthropologist does.

I think it is good to give all these warnings to those who go into the field, but one has to remember that, after all, one has to adjust to the circumstances of the field and make the best of conditions that one finds in the field. One must have only one goal, which is to get the material back—whatever the methods, whatever the circumstances.

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

that 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 naive 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 micro-

scopic 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 dis-

covered 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

continuity of thought and development.

Now somewhat the same applies to models. I may be mistaken, but it seems to me that the statistical model that Professor Lévi-Strauss talked about is not so very different from a certain type of mathematical formulation. It is an equation, or can be put into an equation or at least something similar mathematically. Put in that form, it loses all the glamour, of course, of being as of 1952.

My past being greater than my future, I feel sympathetic toward the good old solid accomplishments of the past. Consequently, when, as is true of every 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.

CHAPTER VIII

Problems of Process: Results

Chairmen: Julian H. Steward and D. N. Majumdar.

Papers discussed: Beals, "Acculturation";* Hallowell, "Culture, Personality, and Society";* Thompson, "Advances in Folklore Studies";* Mead, "National Character."*

Speakers: Majumdar, Redfield,† Nadel, Hallowell, Henry, Steward, Linton, Clark, Beals, Lowie, Movius, Dias, Spoehr, Caudill, Lewis, Bidney, Mandelbaum,† Mead, Forde, Sangvichien, Lévi-Strauss, Ishida, Thompson.

ACCULTURATION

MAJUMDAR: Indian anthropology, although inspired by British anthropology, has not developed along British lines but rather along lines that you find in the United States. This may be a case of selective acceptance or convergent development. [In India, growing recognition is being accorded anthropology both in the universities (in one there are 225 students of anthropology, 15 of them at the doctoral level) and also by the creation of a seven-section anthropology department within the government.] India has unique scope for the study of cultural dynamics. The constitution of India provides for the gradual transition of 30 million tribal people to rural status, and 60 million of backward castes are waiting to be leveled up within the short period of ten years. . . .

Professor Beals shows the processes and the possibilities of the study of culture change or acculturation, and says that the full acceptance of acculturation, which he equates with cultural contact, is quite recent. In India, systematic study of culture change had been initiated as early as 1931, under the auspices of the decennial census, and Indian literature abounds in references to culture change and culture contact. This literature, untranslated from vernacular languages, finds no mention in Western publications.

Most significant in Professor Beals's paper is his emphasis on the need for precise definition of terms like "assimilation," "adaptation," "acceptance," "symbiosis," "commensalism," "acculturation," and "transculturation," and for reduction of the processes into standard categories. His stages are adaptation, syncretism and reaction, in varying degrees.

I feel that adaptation is not a process of acculturation, for acculturation is a grafting process, like the grafting of tissues. Any physiological change must be effected with complete knowledge of clinical conditions; otherwise we increase tension potentials and produce friction. A toxic effect of acculturation is evident in many parts of India today as "contra-acculturation." The renascence of Indian culture, through modernization of Indian folk art and culture, is a sign of contra-acculturation. Although, theoretically, acculturation may be both one- or two-way traffic, it is in practice the passing of traits from a higher culture or conquest group to a backward or conquered group,

through tacit acceptance by the latter. A native people in contact with technological civilization will acculturate or perish, but contact with a nontechnical, nonconquest group may lead to transculturation. . . .

Acculturation studies must be correlated with the time factor or mobility. Indians once looked to the West for traits which they incorporated into their culture; today, the terminals and voltage are both changing. The Chotanagpur tribes, Santhals, and Mundas, who live in compact blocks of millions, have not acculturated much, while the scattered tribes have detribalized. A quantitative approach to acculturation would be possible if we could develop a technique of ethnometry, of measuring ethnographic phenomena, and we are trying to develop formulas. . . .

Professor Hallowell's paper expresses very neatly the Indian reaction to the personality structure approach. He says that, although culture, personality, and society may be conceptually differentiated for special types of analysis and investigation, they cannot be postulated as completely independent variables. We have to deal with two types of personalities: (1) the personality of the culture, (2) the personalities within a culture. The second helps assess the first; but has the first no significance with respect to the second? There is definite interaction among the three—culture, society, and personality.

REDFIELD: Professor Hallowell's paper coherently sets forth points with regard to the nature of man in society and culture, a study substantially unrecognized in anthropology twenty-five years ago and one which has been more explicitly adopted in the United States than in any other country. If it is an essentially American contribution, this is a good place to expose it to international criticism.

It has been successful, in that it offers a fairly coherent body of propositions and stimulates a number of people to find out more about the things we are interested in and to relate them to these propositions. The contrast with Dr. Beals's report is notable, for he concludes that recent studies in acculturation have mainly followed lines laid down in early literature, with little theoretical contribution. Please note that it was acculturation with which I was early connected, not this field of personality and culture.

We are not required to choose between the way of looking at things that Professor Hallowell's paper expresses and that expressed in, let us say, the writings of Professor Radcliffe-Brown. We do not rapidly move in our kind of science in the direction of a choice of final truth, but at this point in our work we welcome this viewpoint and learn from it because we are called upon to ask new questions and to see relationships which before we did not see so plainly. It is sometimes said that the personality and culture people have added nothing new, that the study of culture and society, of course, involved what went on in individual personalities—to which one answer is that, of course, it did; but not much was said about it then.

I suppose the essential characteristic of this viewpoint is that the human individual is the center of the study of process, conceived as a creature in formation and always undergoing change. This seems to bring us much closer than was true in my early years to a systematic consideration of what is more nearly a basic observable, a thing from which all the other inferences were derived. The processual nature of the phenomena is emphasized by the fact that there are a number of human careers alike in some respects and different in others. Society, culture, and other phenomena now take a

slightly derived or peripheral position from the point of view of the examination of this developing entity, the human personality. We look at them all again, but in a new relationship. So the questions change—How does a society persist? becomes How do human individual personalities persist? The question of social change becomes one of the formation of human personality in a being becoming human. . . . Instead of looking for a frontier between two entities of societies or of cultures, we look at people with organized personalities entering into situations in which these organizations undergo some alteration. We find ourselves studying the same thing, but from an enriching viewpoint.

Consideration of this somewhat altered viewpoint with regard to our problem stimulates my thought in several directions, of which I should like to suggest three. The first is a recognition of culture as an expression of human creativeness. Because of this viewpoint, I find it easier to see man creating, expressing himself, representing his inner state, and I come to see that man not only receives stimuli or submits himself to the constraints of custom or adapts or adjusts to others in a network of social relations, but he is also a dreamer, a mythmaker, a fantasy-producer. Symbols are seen not merely as the clues to social control and the mechanism of communication, but also as products of the symbolizing nature of man—something accomplished by man, not merely acting upon him. . . . I should like to hear more than the brief statement in Thompson's paper that the students of folklore are beginning to ask questions on how folklore is created and modified.

The second direction is attention to human nature. It now becomes interesting and possible to characterize human nature in positive statements that may be true of all human beings, developed and wrought into whatever society or culture. Replacing an early confused and misunderstood conception of psychic unity, we begin to see a sort of triangle of aspects of the whole reality of humanity: culture, which is the expression of humanity in persisting traditional groups; personality, the expression of humanity in the persisting organizations of the human individuals; and human nature, the persisting characteristic of all who undergo the experiences characteristic of human beings.

In this point of view the important and useful universals may have reference to human nature rather than to cultures generalized upon. For example, all human beings apparently know that they exist, and they are able to look inward upon themselves. Their look inward upon themselves is something that goes on in that opposition or dialectic which G. H. Mead and others have described.

Other illustrations of this kind of question are the universality of the Oedipus complex or the way we must modify it in order to make use of it; the common-sense assertions with regard to man which sociologists such as Charles Horton Cooley used to make much of; and our offhand insights with regard to shame and guilt and the like. In this territory we find ourselves characterizing human nature as a recognized and respectable subject matter of our science, and it is no surprise that certain branches of psychology and sociology have anticipated us. What is creditable to anthropology is that we learn from them. What is creditable to them is that they often modify their views under our influence. But conjointly this area of investigation is now opened—I do not think it could have been opened without the development of the viewpoint.

The third direction of thought is expressed in the concept of personality type, which I want to speak of in connection with the light it throws on the classification and typing of culture. This is connected with universals of culture.

If you begin to classify mankind by divisions of activity or sets of institutions and custom in the Wisslerian manner, you come to an end pretty soon. But if you make a fresh beginning, you find new understandings emerge. Goldschmidt's attempt (see Hallowell's paper) to show that the Yurok-Hupa Indians and the emergent capitalistic Europeans had in some sense the same personality type is stimulating in its boldness and, though it may be quite untenable, illustrates the kind of question you might ask and how it helps you to put together things which you feel ought to be together. A young man, having studied the Lapps for a number of years, became interested in readings on the Bedouin and, although the Lapps have totally different customs and environment, felt that there was something about the Lapps' notions of individualism, hospitality, and so on, which were so similar to those of the Bedouin that a Lapp would feel quite at home among the Bedouin, once he got over the trouble with the climate, diet, and other fairly superficial things. Looked at from the point of view of the concept of personality type which emerges here, this begins to make some sense. If you recall, Professor Hallowell found among one of the Ojibwa groups an extraordinary persistence of something below the level of the ordinarily observable, which he would identify with a type of major personality. Professor E. K. L. Francis has written some interesting papers on the nature of the peasant personality, suggesting that by comparing Hesiod's account of early Greek peasant life with ethnological accounts of peasant societies, we arrive at a peasant personality which seems to be typical and which has peculiar stability in certain kinds of historic situations. I find all this helpful.

Dr. Tax and some of his associates have recently enunciated the perfectly outrageous hypothesis that acculturation never occurs, which is very surprising in view of Dr. Beals's review of the literature on the subject. I am all for outrageous hypotheses, and I think this is a good one, although it just is not so if you understand it in the ordinary literal sense of acculturation. Dr. Tax and his associates seem to be saying that the Indians that they are working with listen to the radio and wear clothes like ours but, if you talk to them longer, seem to be thinking and feeling just about the same as they did in the past. These investigators say that acculturation there does not occur. What is meant is that something persists and this is, perhaps, more easily understood if you center your interest upon the conception of human personality and in this case look at its typical form.

This way of looking at it should prove stimulating for the students of prehistory. If you emphasize, as archeologists apparently are coming to do, not so much the change in the way a tool was made as the manner of life that may reasonably be inferred from such artifacts as are preserved; and if you go on and say that what really matters was the acceptance of a sedentary form of life with neighbors constantly jostling around, generation after generation; that this could come about even among food collectors, given the right situation; and that what matters beyond that was the transition of the village into a city, then what becomes important in understanding these transitions is not so much the particular tools that were used or even the particular material resources—necessary conditions as they are—but

the kinds of men that were made by these forms of living.

I should suppose that transition from the nomad to a townsman form of life would be easier under certain conditions, and certain kinds of human groups living today would be better or worse adapted to such a transition. It would be harder to make a peasant out of a Comanche than out of a sedentary food collector; and, if you think of it from the point of view of the kind of personality and the stability that Hallowell and many others have shown for these personality types and think about human history in those terms, we might be able to deal more realistically and uniformly with the problems of the human career considered now as a species rather than a single individual.

NADEL: The emphasis in the studies of personality might fruitfully be shifted to the plural of personality—a study of personalities—in any given society, group, culture, and so forth. . . . I mean covert, obscured deviations or variations in personality features which only projective tests and the like might reveal.

The connection between that and acculturation is this: Assuming that you have in a society an influence from outside, which brings to bear on the society, apparently and overtly, features in contrast, or at least in sharp opposition, to the traditional pattern of life, then it is very possible, if we assume that there is a variability among the existing personality types, that some of those obscurely deviant personalities might be attracted by the new chances of realization offered by the new culture, and that these people who had previously overtly lived successfully in their own society but had, in fact, suffered from all sorts of frustrations, suppressions, and so forth, would then find a direct channel of expression in the new culture. Here, I am referring to Dr. Hallowell's paper, where he says that we must always understand the relationship between culture and personality in a twofold way—its condition and its expression.

I was led to this thought by the fact that certain projective or near-projective techniques applied to the Nupe revealed character traits quite in conflict with the accepted ideal pattern of behavior. The ethos as formulated from observation as an anthropologist was not the one that appeared in these tests. In these experiments I did not get a uniform personality but a scatter, which, roughly, can be grouped into majority-minority types. Now the minority type, apparently successfully living in this culture but not internally adjusted to it, might easily become the carriers of any new culture which would be the opposite of that they were living in. Some of the processes of acculturation, including acceptance as against rejection, might well be correlated with that internal diversification of personality types which we can assume to exist at any moment in any given culture.

HALLOWELL: In 1941, Homer Barnett, without the benefit of projective techniques, advanced the hypothesis that it was the people who were not getting along so well and did not have opportunities in one culture, who, when an opportunity came, were the leaders.[1] A test of that hypothesis by Richard Adams did not entirely verify it.[2] These are specific examples of this real problem. I agree with you but would also like to comment, with reference to the Rorschach, that here a systematic sampling of a population by a projective technique might possibly give you clues which you would not

[1] Homer G. Barnett, "Personal Conflicts and Culture Change," *Social Forces,* XX (1941), 160–71.

[2] Richard N. Adams, "Personnel in Culture Change: A Test of a Hypothesis," *Social Forces,* XXX (1951), 185–89.

get otherwise and then might become significant in terms of following up the particular individuals who are deviant or not deviant, by getting a pattern of what the variables are.

HENRY: In view of the [systematic] work done by psychiatrists . . . [and by sociologists and anthropologists], I think it is quite improper at the present time to talk of observation as if it were merely impressions. Observations, if done on a careful sampling basis, can be grouped in a statistical way and analyzed.

The problem of acculturation is exceedingly vast, once one begins to think of the relationship between personality and culture and acculturation. For example, is an individual acculturated until his autonomic system reacts in the way our autonomic system does to life-stresses? It is important to understand that acculturation goes far beyond some of the formal categories we have been used to examine. . . . How does the acculturated person think in comparison to the way we think? Does he think of the body as a machine, as we do, subject to stresses and strains, in the Cartesian sense? Is he a properly acculturated individual until he can think like this? Does he think of the individual as divided into dual cells in conflict? Is he capable of assimilating intangible notions to tangible, so that he can talk of a waste of time, saving time, and so on? Does he think of the mind as being aware and unaware of things? Does he think of thought as random ideas passing through the mind, or as a very concrete thing—recollection and the specific image before him? Does he think in terms of what I call "psychic migration," so that he can regress and get ahead as a personality? Is he capable of introspection?

STEWARD: This problem is beginning to look as if there may be two quite separable aspects of acculturation. For instance, is there a culture of the forecastle on a ship? The crew turns over rapidly and may come from all walks of life and include individuals who have been acculturated or, rather, socialized for all kinds of ways of life and attitudes. And yet, in that situation, they conform to the cultural demands, whether they like it or not. It even poses a problem of what we mean by "culture." The personality, the built-in, learned personality that the individual acquires in the process of socialization, may train him for one way of life but these days he generally finds he has to live a little differently. Do we not still face the problem: Whence came the change in those external circumstances to which he must adjust himself? and the corollary problem of how far and in what way the built-in personality will affect the external circumstances bearing down on him.

LINTON: Two aspects of acculturation have got somewhat scrambled here. One is the adjustment of the individual to a new culture and a new society, and the other is the adjustment of societies to one another through the exchange of patterns of behavior, etc.

In this second phase at least, you cannot understand the role of individuals simply on the basis of their personalities, aberrant or otherwise, because there immediately enters the factor of prestige and influence. You may have an individual of very low prestige from a serf group within the native culture who immediately accepts the European culture at every point that he can, but he thus not only does not hasten the acculturation of the new society but even delays it. Among certain immigrant European groups, the people who resisted acculturation or Americanization most vigorously were members of the lower nobility, who occupied a much higher status as Europeans than they would occupy as Americans. In all these studies of personality and culture, also, where

you are dealing with anything except very small, homogeneous communities, it is necessary to have a social breakdown. After all, larger societies are always made up of subsocieties and subcultures. . . .

CLARK: Regarding the effect of acculturation between two or more societies, this conception is a pretty old one in archeology, and it is rather significant that one of the best illustrations—the studies on the genesis of La Tène art—resulted, as it were, from a cross-fertilization of one discipline to another. [Only because of his knowledge of Greek and Etruscan art forms did Dr. Paul Jakobstal find the key to this genesis in the influence of this civilization on the Celts.]

Dr. Beals says that acculturation is dynamic in character and concerned with process. In this case of the genesis of La Tène art, that works out very clearly. Dr. Jakobstal and others have shown that one can trace two successive phases of this impact of Greco-Etruscan civilization onto these much simpler peasant people living north of the Alps, and these two phases are reflected in archeological terminology. We speak of a Hallstatt period and a La Tène period. In the Hallstatt period, objects . . . were imported but with no real effect [on existing artistic traditions. In the La Tène period the chiefly class imported bronze wine vessels which the native craftsmen tried to copy. Their lack of understanding of the designs led in some cases to bad copies, but in others to an entirely new art style—the direct result of acculturation.]

We have innumerable instances of this, and the study of acculturation has been the main inspiration of the study of the Bronze Age and the Iron Age in Europe. . . . In the study of prehistory, these concepts have played an extremely important part, and, conversely, by studying prehistory, one can see, as it were, the fossils of this historical process.

BEALS: One of the questions that Mr. Majumdar raised about the size of groups having some relationship to the rate or character of acculturation immediately struck me as raising some very important problems, one of them having reference to the job of acculturation that was done by twenty thousand Spaniards in the first eighty years of the postconquest history of Mexico —a very extensive and very rapid acculturative process.

The literature of acculturation tends to become confused between talking about the cultural processes of acculturation and talking about what happens to an individual. . . . We can look at a body of phenomena in terms of cultural interpretations, in terms of societal interpretations, or in terms of the culture and personality approach. These are related ways of looking at these phenomena, and they are in a constant and dynamic relationship. The situation one finds in acculturation is of particular interest because processes which may not be unique to acculturation are here highlighted and intensified and hence, perhaps, easier to perceive and study.

The question of the relation between acculturation studies and the personality approach is very real. . . . We have cultures which are no longer Indian, which have undergone varying degrees of acculturation in which the content may be very largely European in its origins, and yet which we who have not worked in the personality field vaguely feel have somehow retained a different kind of patterning, of looking at the world . . . which apparently leads Dr. Tax and his group to suggest that maybe no acculturation has occurred. This again is confusion. Are you discussing acculturation or acculturation of individuals?

Actually, I think our evidence is far

from complete. Spindler's work on the Ojibwa seems to indicate that the group has become completely acculturated from the personality standpoint, while the control white group, a marginal group, more closely approaches in personality some of the Indians in the group who are not yet completely acculturated; in other words, this dynamic relationship seems to work—and to work both ways.

The importance of knowing the range of personalities for understanding some of the dynamics of acculturation is extremely great, but I would add to this that we know very little about the range of adaptability of a given personality structure to various situations.

LOWIE: I speak partly as a native informant and partly as an anthropologist. Dr. Henry and Dr. Linton have both raised very important issues. With regard to the question of when an individual is acculturated, I, having come from Austria at the age of ten, still have strong reactions [against certain American forms of address as over against German forms].

And I am entirely in agreement with what Linton says about the necessity of having regard to the social status of the people. . . .

MOVIUS: I should like to make one observation on this question of the individual versus culture and acculturation from archeological parallels: on the one hand, the points raised by Dr. Hallowell, and the social factors involved in culture growth, which were commented on by Professor Redfield, on the other. I think Redfield had in mind the question of the transition from one revolution to another, as conceived by Childe, but we must remember that the new traits did not come into being all at once, in one place. There were many new developments over a considerable area, which in large measure reflected the foresighted thinking of given geniuses, given people, who had the concepts. It was the individual who foresaw the skyscraper; it was not the social growth that itself made the skyscraper. . . .

DIAS: With respect to acculturation in other times, we must devote attention to the influence of fine arts. Thus in Portugal and Spain [the peoples who came to the peninsula from the outside are seen to have had the strongest influence on the surviving culture in regions with environments similar to those of their points of origin].

SPOEHR: With regard to Oceanic prehistory, one reads in the literature that the Oceanic migrations were caused by Mongoloid peoples moving from southern Asia and that, as a result, Polynesia and Micronesia were populated. This has always seemed nonsense to me. We can, I think, reconstruct the cultural and societal conditions that were necessary for movements out into the Pacific, but anyone who has been in an outrigger canoe out of sight of land, with a tropical front approaching, knows that it takes a particular personality type to cope with these conditions. We could very possibly reconstruct the personality type that was a necessary and sufficient condition for the migrations into the Pacific. Furthermore, today, under conditions of acculturation, say in the Marshalls or Carolines, you get a very remarkable differentiation in the community between those still willing to take an outrigger canoe five and six hundred miles across the sea, and those who would prefer to remain and become schoolteachers.

PERSONALITY IN CULTURE

CAUDILL: "Personality in culture" is an awkward term. [The locus of personality] is more likely to lie within small groups in a culture, through the mediation of which individuals derive their culture. . . . [We need a frame-

work which will show such things as] how adaptive mechanisms are differently combined among different people to give rise to distinct personality structures—these mechanisms, of course, being to some extent given by the culture.

Also relevant to Dr. Hallowell's work is the fact that much of this goes on at a nonverbalized and perhaps in this sense unconscious level, so that we need projective techniques to get at it.

In American culture there is the idea that you can occupy Germany and Japan and fairly easily make some significant changes, whereas such work as Dr. Hallowell's, about the persistence of personality over time would suggest that culture is, as Boas said a long time ago, a stubborn persistence of configurations underlying this, and that this somehow goes in part against an American value system.

There are two general sets of propositions in psychology today. One stems from Freud and talks largely about personality attributes located inside people. The other, stated largely by H. S. Sullivan, looks at personality as growing out of and, in fact, resident in the interactive situation itself. . . . These bodies of thought need to be related to the whole problem of culture and personality, and, in the Sullivanian sense, to the study of small groups and the effect of the situation upon behavior.

HALLOWELL: To say simply that a personality has persisted does not quite cover the point. There has been an adaptive process going on, and this is the most important aspect of the whole thing. While there are certain features in personality which could link with the past, yet in functional terms it has really been breaking down under certain conditions, and you have an interrelated series of variables in a dynamic process.

LEWIS: Almost each time culture and personality have been discussed here, personality studies seem to have been equated with the use of the projective tests . . . and there are many outstanding psychologists who simply do not go along with the results of these tests. Many anthropologists, I am afraid, have gone into the business of studying personality just with these tests—without very careful preparation in personality studies—and this is dangerous. If we could get trained psychologists to help us in the field, we might come up with even more meaningful materials.

HALLOWELL: While it is true that certain psychologists, when they view these tests from a certain frame of reference, are not altogether satisfied, it is also true that in hundreds of clinics in the United States psychologists are being trained to use projective techniques and are using them.

BIDNEY: Redfield began by making a very hopeful statement that studies of personality and social anthropology are really complementary and that there need not be any opposition between them. I think that, as a thesis, deserves much more emphasis than has been given it heretofore.

When he outlined some very stimulating suggestions, he referred to culture, human nature, and personality and left the aspect of society rather in the background. If we are to bring about a kind of complementary situation in which both personality studies and studies in social anthropology as represented by the British anthropologists exist, we must definitely recognize that personality should be understood as a function of culture, human nature, and society and not explain away the element of society simply as a function of culture. We must reckon with it as an independent kind of invariable. This would be the only possible way of reconciling the kind of social-anthropological approach in England with the

kind of cultural anthropology and personality studies we have in this country.

STEWARD: I am wondering whether the quasi-psychological approach that is involved in the personality approach may not be very pertinent to some things and less pertinent to others. It seems to me that there are many changes of culture, perhaps ultimately of far-reaching importance, which occur initially not for any reason involved in personality but for purely rational reasons. Dr. Kidder has pointed out that the acceptance of the machete in Mesoamerica and a lot of Latin America ultimately revolutionzed, in certain respects, the native culture. The initial acceptance was a clear-cut case of usefulness under prevailing circumstances, and personality was certainly not involved, although in the end the effects on the Indians of their use of the machete were quite profound.

In contrast to this, we might cite cases in which people have come into conflict with one another, requiring postulation of some psychological mechanisms—hostility, identification, projection, etc.—to explain possible manifestations of witchcraft, which, without these psychological mechanisms, could not possibly be explained. I would suggest, then, that a problem is to ascertain those aspects or categories or segments of culture where one or the other kind of explanation is not necessarily exclusive but predominant, and the method thereby would be a little bit different.

NATIONAL CHARACTER

MANDELBAUM (statement revised after session[3]): I believe that we all understand national character studies to be studies of contemporary peoples who live in politically defined groupings and we need not belabor the point that these national units are real units. People react to certain stimuli as nationals, and these are real social and cultural entities as we conventionally use these terms. In studying such contemporary peoples, we try to study the cultures as wholes, and we stress field work.

Moreover, as was pointed out in Dr. Mead's paper, we take the comparative point of view. We do not confine ourselves to present observations but use historical information, when available, to gain understanding in time depth. We also study subcultures, class and kin groupings of various magnitudes within the larger unit. We seek help from culture-personality studies to determine the influence of such factors as perceptions and emotions and also the limits and variations of these factors. These may tell us in what ways a people are amenable to certain changes or tenaciously cling to a certain set of patterns. There is no necessary conflict between the cultural and social kind of analysis and the analysis of factors of emotion, perception, and memory which must usually follow the cultural and social analysis.

In addition to the limits that are culturally set, there are other limits about which we should know something, as demographic and ecological limits.

The study of national character is still in a fledgling state, and it is to the great credit of those who have worked in this field under considerable handicaps that they have been bold enough to make the preliminary and tentative formulations. It has certain important advantages and certain notable disadvantages. Some of the disadvantages are these. There is the matter of size and complexity of the subject. When you study the Russians or Indians and try to make some sketches of the national character of a large segment of humanity, it is a sizable job, though not necessarily an impossible one. Then

[3] An expanded version of these comments will be published elsewhere.

there is the difficulty in assessing a particular national character against a range of generally human attributes, where the nature and scope of the general range is far from well known. Then there is the unfounded accusation that this study is a new form of racialism, disguised by a veneer of fresh jargon. And, since these studies may deal with subjects of great interest to many and under hot dispute, the anthropological analyst may be strongly castigated by all parties in the dispute.

Some of the advantages are these. It enables us to fill in very large and serious gaps in our comparative data. We can make use of the codified, systematized wealth of information that the historians and other specialists have given us for various areas. We can tap the resources of these specialists in the field and, perhaps even more important, recruit co-workers and colleagues from the cultures themselves, so that we can complement one another's knowledge or cancel out one another's biases. We can utilize such good work as that which has gone into the compilation of folklore archives and folklore indices, and which have scarcely been tapped for questions other than those of a very limited range.

There are three major points in Dr. Mead's paper which seem to need comment or correction.

The opening statements in that paper note that national character studies not only are applied studies but take their form and method from the exigencies of the world political situation. Applied anthropology is of importance, but I see no reason for this insistence that national character studies are only applied studies. If. Dr. Mead means that most or all of the studies done so far have been applied studies or have been stimulated by requests from governmental agencies, that is another thing.

The problems which are set in these studies need not necessarily take their form from immediate political problems, nor should they deal mainly with inaccessible societies. It would be well to avoid any implication that administrators have been ready to consult with and take the advice of anthropologists on political issues. The state of our knowledge in this field is not so advanced that we can give clear-cut recommendations on all major political issues, nor are the administrators prepared to accept sound advice which anthropologists can give. A few anthropologists have had some hand in important policy decisions, but rarely, if ever, in their capacity as anthropologists. What we do know can be crucial in certain situations, but our problems should not be set by any external agency, nor should anthropologists be the handservants of an external agency.

The second point concerns Dr. Mead's saying that culture-personality studies and national character studies are distinguished by the presence of a psychological theory. She lists such examples of psychological theory as simple associationalism, Freudian constructs, the eclectic constructs of social psychology. These formulations give us some definitions, a certain limited knowledge of process, and direct our attention to certain spheres of inquiry. But they do not provide theoretical principles which may be applied readily. The findings of these theories have usually come out of investigations of phenomena which were limited in scope and sometimes in number. Hence they have limited applicability to holistic culture-personality studies.

Third, Dr. Mead makes the point that anthropologists do not take samples in the way, say, that social psychologists do. I entirely agree with her—social psychologists as well as others are much too prone to begin to measure without knowing what they are measuring, or what good the measurements will do. I would also agree

with Dr. Mead, Dr. Schapiro, and others that we must have a certain sense of relationships, of form or style, but also that, once we have it, we have to test it, we must count in order to appraise the validity of the formulated pattern. I do not think that the anthropologists' sampling is of a different order from that of other scientists. It is true that the linguist may be able to get a grammar from one informant, but that procedure is based on many assumptions which may not apply outside linguistics. There is a great deal to be gained from examining the contributions which linguistic analysis can make to general ethnology, but we must also recognize that there are certain ways in which language is different from the rest of behavior, and, if we shut our eyes to that, we will not only misunderstand sampling but also a good many other things.

MEAD: On the question of national character as an applied subject, I meant applied research, too; it is research in an applied field, not application in the sense that you go in and fix anything. I call any research "applied" which makes you do something that you would not do next in terms of your tools and the state of your theory; and most of us who have worked in this field are not doing research in the place that fits best for the sheer advancement of personality in culture theory. It is only in that sense that I mean it is applied. . . . To make that clear will mean rewriting a good many spots in my paper.

Second, I did not think it necessary to lament that, if you could not do field work, you could not do full anthropology. In the further sense, when I say "the exigencies of the world political situation," I mean that we are in a very unstable world situation in every respect today . . . and that information organized nationally appears to be useful; but if somebody would guarantee that everything would stay nice and comfortable and no research would be needed for the next twenty-five years, I would not touch national character for the next twenty-five years, because I think it is more important to go back to New Guinea. . . . That is all I mean by "applied"—choice of research area in terms of a value external to the research itself but concerned with the safety and future of mankind. Your statement that out of some kind of fervor people dropped other things and went into national character is true, and almost all of us who have devoted our time to it in the last ten years would much rather have done something else.

On the point about psychological theory, that is a question of inept writing, combined with a fair number of typographical errors. I said that personality and culture study involved a psychological theory. By that, I did not mean that it involved a particular school of psychology. . . . Then I descriptively outlined those particular approaches which had been most used. I did not mean that, in order to work in culture and personality you had to be a Freudian or a Gestaltist or a Hullian psychologist. We have to have our own body of theory in culture and personality, using new developments and tools and conceptual schemes as they come out of psychology and sociology.

On this question of sampling, I did not at any point advocate having one sample. I mentioned that in certain instances formal properties of language could be gotten from one informant, if that was all you had. Certain formal properties could be gotten if you had a large enough corpus. The distinctive aspect of anthropological sampling is a very large amount of information about a small number of individuals, and this is not peculiar to anthropology. . . . We know how to place our informant in terms of the social structure, in

terms of his past, his capabilities, his perceptual behavior, so that we can extract a very large number of discrete observations from a single individual. I was not advocating having a single individual, and you ignored the stages that I outlined at the end of the paper, where I suggested that the first stage of national character work is the construction of hypotheses from intensive study of small amounts of data, small in the sense that they are collected from single individuals or based on highly patterned behavior of some sort, then the validation of those hypotheses to the point where you know they are worth testing on a wider scale; and at that point the sociological type of survey testing, which would test incidence and prevalence over a wide population, demarcate your regions, demarcate your class positions accurately, would in any case come in where it is possible to have access to the society and where money is available. . . .

This question of sampling is a question of order of sampling and of which kind of sample we as anthropologists, with field work in primitive societies, have been best trained to use and have so used to construct our initial hypotheses.

STEWARD: Might I ask why you feel that a smaller subcultural group would be better for scientific procedures than the national level? Because in the former the uniformities are clearer? There are less complicating factors affecting individual behavior? Because in the national group the core of shared features is smaller?

MEAD: Partly, but also we are very much concerned in this field with the relationship between politically determined relationships and human personality. We have no complete study on a small scale of the way in which political forms are incorporated into personality. Procedurally I would like to see a study of a small community that had political form before tackling units of the size of the United States. This would be much better than going directly from structures like the British Empire to its impact on individual human beings, as they learn within it.

STEWARD: You mean by "political forms" that the impact of the political forms would be from a larger unit—the national British political forms on some subculture of Britain?

MEAD: No, I mean that I would like to study the smallest possible nation-state that nevertheless could be represented on the national level before I tackle a big one.

FORDE: Does Dr. Mead mean that national character studies are intended to be studies of the political value systems of peoples? The national character studies I have read have laid a great deal of stress on basic interrelations at the familia level and sexual maturation and so forth, but almost none on political values.

MEAD: The national character studies with which I am familiar have been entirely concerned with attempting to describe those regularities which are common to the individuals who live in a politically defined nation, even though that may represent a cluster of cultures. . . . The fact that we study the way in which the family mediates those shared attitudes which are manifested when a group of Americans form an army and go abroad—that we discuss the family and discuss the maturation of the individual—does not mean that that is what we are concerned with. We are concerned with the ways in which the shared attitudes which characterize the nation, when seen from the outside, are inculcated in the course of maturation.

FORDE: They may be politically significant, but they are not political value systems.

SANGVICHEN (statement made at later meeting): Dr. Mead gives me the impression that [in studying Thai national

character] she would class me as typical of the culture of my people. I am not considered typical of my people, even though I am not and, even if I stayed here twenty years, would not be incorporated into the American way of living. To study Siamese national character, Dr. Mead would have to go to Siam and get familiarity with the life of the common people there.

LEWIS: How would it be possible to relate to this discussion some of the suggestions for control in field work which Dr. Mead gave us yesterday. . . . How can that type of methodology be related to the kind of methodology which has to be used in national character studies? . . .

MEAD: We have developed a very detailed methodology of quite a different order. What we discussed yesterday was what one field worker or a husband-and-wife team or its equivalent does when confronted with a living network of interrelations, a whole community embodying a culture. If we could have a perfect national character expedition, we would do the same thing, except that we would have economists, historians, archeologists, etc. When we have had to work with a few informants at a distance, we have developed a ratio between the number of expert people and the amount of data shared and used, so that in the last piece of research I directed this winter we had three people doing research and 14 top-level experts working on the material the three people collected. Sometimes you would have 17 people working over three interviews, from the standpoint of different disciplines, different knowledge of the history of the country, different types of experience. . . . Whereas, if you have a single field worker or a pair of field workers, you get as many simultaneous measurements as possible on the behavior of a group of people, and you have in this case a group of people, trained to act as a team, operating on a small amount of material.

LÉVI-STRAUSS: A few years ago, I had access to the carbon copy of a chapter of a survey on the French national character done by Dr. Mead, and, though this was not for publication and rather reckless on my part, I used it in my seminar in Paris, which included about twenty students, half of them French, half of them belonging to five or six different nationalities. Among the French students, no clear reaction was found. They were not indignant at all. They were not convinced either. They remained, if I may say so, neutral, while all the foreign students were absolutely enthusiastic about the chapter and stated that this was exactly how they were seeing the French people. Now this agreement worried me a little, because, after all, they had very limited experience of the French people and for a very limited amount of time, so, according to scientific standards, the agreement should not have been so enthusiastic as that.

This should be related to the problem of projective tests because, by all reasonable standards, projective tests should not work for intercultural study, and the amazing fact is that they do work to a very large extent.

The question I am now putting to Dr. Mead is whether the explanation is not that the national character studies are a kind of projective test where the patient is the observer—that the anthropologist gets his observations as the pieces of a puzzle and projects on the puzzle to reconstruct the national character. If this hypothesis is right, we would have the more or less satisfactory explanation that national character studies do not teach us the objective characters of the culture under study, but a certain relationship between the observer and the observed phenomena, and that this relationship can be con-

sidered true, while not necessarily reflecting the properties of the phenomenon under consideration.

MEAD: . . . I agree that very often these things are projections from the observer. However, your foreign students were of different nationalities, which suggest that we had said something about the French that could be recognized and understood by a variety of non-French people, which is already a step beyond simple projection. Second, we never succeeded in getting a completely French person to be a member of our group . . . as we got from the other national groups. That, in itself, we regarded as significant. Third, the French material is the most tentative of any that we had, because we felt it would be unjustified to do a great deal of work on France when we could get only rather meager French cooperation, when there was no enclave of French people here to observe so that we had to treat them all as individuals, and when France had its own social scientists who could do studies in France; we only tried to get enough to show comparatively what we were getting at if we worked at a distance, when we had to, as compared to accomplishments in the field.

We do not feel that we have solved the problem of describing the national character of a people until we can say it in a way that they accept. But we expect that the willingness to accept formulations of the self that include unconscious and inarticulate elements of the personality will vary from one culture to another, and we are just beginning to learn how to say something about a people which they will recognize and which will, at the same time, be scientifically viable and communicable.

MAJUMDAR: I have not the courage to express an opinion on the analytical processes, but I wonder if Dr. Mead does not deal with an artificial population by the word "nation," which is not today an anthropological concept. We have seen the processes of fission and fusion of national groups. By introducing national character as a new category, I feel that we may fall again into the trap to which Chamberlain, Galton, and others committed the Nordics. Would it not be better to use "regional types" for "nation"?

The recent Japanese reaction to Benedict's brilliant characterization of the Japanese is an example in point. I refer to the special volume of the *Journal of the Japanese Ethnological Society*. A study of national character might be safe in competent hands but might not be so in others. . . .

MEAD: . . . There is always the possibility that anything may be misused. . . . But we are saying that any people of any race, who are for a long enough time members of a political nation, will display certain characteristics. It is the exact opposite of racism, and it is so far opposite that one of the principal objections by Slavs to our Russian studies is that they do not recognize the Slavic soul.

ISHIDA (statement revised after session): As Dr. Majumdar has just mentioned, we once published some reactions to Ruth Benedict's *The Chrysanthemum and the Sword* by several Japanese scholars under the heading "Problems Raised by *The Chrysanthemum and the Sword*."

Their criticism was directed, among other things, against her way of treating historical materials. It seemed that Benedict used almost on the same level data from different periods of Japanese history, e.g., seventeenth-century and the prewar or wartime phenomena, in order to find the patterns of Japanese culture.

Is not a national character formed historically and does it not change through history? To what extent can we speak of a constant, persistent national

character or pattern which can survive great social changes?

These were, I remember, the main points on which all Japanese scholars could not agree with her methods. Nevertheless, they acknowledged that her insights, though more or less intuitive, did point out many correct points which even the Japanese had not noticed.

LINTON: I wonder whether the introduction of personality, personality concepts, and the psychological explanations for these in terms of, shall we say, swaddling, interrupted nursing, and so on, is not introducing an unnecessary hazard into the situation? The statement was made that this is a new development, but there is a long series of what amount to national character studies which relate to the Byzantines. . . . However, this was certainly not done in terms of the infantile experience of these people. It was done in terms of their observed cultural characteristics. Until we know very much more about interrelations of infantile training and adult personality—and I may say that the information that is now coming in from the few places in which we have been able to follow individuals from childhood on raises increasing doubt as to whether the personality is the result of a specific factor of this sort—is it not possible to get everything that will really be useful along this line directly on a cultural basis? Culture . . . includes a whole series of concepts, of values. The individuals in any society show, on projective tests or on analysis, a wide spread of personalities; yet, in a great many situations, all the members of a particular national culture group will behave in very much the same way. People everywhere learn how to do things that are not congenial to their basic personality configuration if they are sufficiently rewarded.

As to the cross-cultural study of the effects of one of the systems of infantile treatment—swaddling—this is rather widespread in Europe, and I suggest that one compare his results in the case of the Russians and of the Sicilians.

MEAD: We have compared in detail the results for the Russians, the Poles, the Rumanians, and the differences check with the rest of the culture. Dr. Linton's statement might suggest that this method is based on the study of single traits of infantile behavior as if they were causative in the cultures. This is not a method that is used by any member of this group. With the adults we take interviews, projective tests, and observe a variety of kinds of behavior; and we obtain records of the treatment of children because we do not have the children to construct initial hypotheses. Those hypotheses are then tested against historical material and the behavior of adults in a variety of other situations. This is not at all a method that attributes national character to single isolated treatment of children.

STEWARD: One question that always bothers me is whether the national uniformities, except in countries with developed mass-media communications, are relatively greater than the subcultural differences between the segments of the population; and that, it would seem, is a purely empirical thing to be determined before anything else.

THOMPSON: I do not know whether, coming from such an interesting, stimulating, and controversial subject, I can do more in a few minutes . . . than mention the desirability of work similar to what many of the European countries are carrying on in what they call the study of folk life; that is to say, the study of peoples of one's own culture, who carry on older traditions. That is one thing that the American anthropologists as a whole have done very little with, that something might very well be done with in the future.

One other point: This morning, when we were talking about universal concepts, it seemed to me that I have been reaching over into that field when I talk about types and motifs in folk tales, and especially in the very tricky area where we try to establish so-called archetypes of folk tales.

COMMUNITY STUDIES

STEWARD: This leaves me to say just a word on the community-study approach. If you consider the community-study technique essentially one of studying subcultures then you can approach the community from the point of view of personality and culture, of culture and of a great many other things. But I think most of us who have worked on community studies have felt that a larger dimension is necessary to understand the subculture of cultures represented in the community itself. There is needed, particularly if we are interested in the dynamics of change, a good deal of attention to over-all features that are not primarily understandable on a local basis—the government, church, law, trade, cash-crop production, export crop, etc.

Anthropology in the future will be faced with finding some means of understanding these larger wholes of which subcultures are parts. We have already mentioned the national character aspect, but there are other aspects, too. There is work by the economist; the political scientist; the students of the history of religion and all kinds of special institutions; the students of the humanities, which often do not express the local subcultural arts and music and so on but national cultural achievements and which are available to and used by the subcultural groups in varying degrees; etc.

This poses two problems to which I do not now see a wholly satisfactory answer. One is the nature of interdisciplinary collaboration necessary (if you could finance enough people to carry on all this) to understand the community. The other is the more difficult problem of how to understand the relationships among these features at the national level so as to have a truly holistic approach to the national culture as well as to its subdivided internally specialized parts. . . .

CHAPTER IX

Problems of Process: Theory

Chairmen: Ralph Linton and S. F. Nadel.

Papers for discussion: Bates, "Human Ecology";* Redfield, "Relations of Anthropology to the Social Sciences and to the Humanities";* Northrop, "Cultural Values"; Hoijer, "The Relation of Language to Culture"; Washburn, "The Strategy of Physical Anthropology";* Boyd, "The Contributions of Genetics to Anthropology"; Vallois, "Race"; Carter, "The Theory of Evolution and the Evolution of Man."

Speakers: Spoehr,† Kroeber, Oakley, Nadel, Linton, Henry, Mead, Majumdar, Clark, Movius, McCown,† Straus, Boyd, Senyurek, Teilhard, Washburn, Lowie, Lévi-Strauss, Voegelin, Bidney, Tanner, Mandelbaum, Haas, Hallowell, Bates, Redfield, Caso.

ECOLOGY AND ANTHROPOLOGY

SPOEHR: Dr. Bates has organized his paper around four principal topical divisions: the analysis of the environment, populations, the community, and ecology in evolution. My own remarks pertain to the third of these topical divisions primarily, and expand somewhat on the discussion of the ecology of human communities. By "human communities," I mean communities of men alone rather than the biological community of man plus the congeries of his domesticated plants and animals, his parasites, and his plant and animal hangers-on.

The benefits to be derived from an ecological point of view are, to my mind, principally two: First is the element of holism, the viewing of a community or society in its total environmental setting. As Dr. Redfield remarks in his paper, the holistic approach is characteristic of anthropology, and it therefore fits well with this ecological point of view. . . . Nevertheless, I would submit that the holistic point of view of ecology and its stress on relationships is not always completely compatible, for the analysis of relationships in the field demands the isolation of relevant data for special study.

The second benefit I would suggest is that the ecological point of view places emphasis on relationships for field analysis and field research and not merely on a categorizing of discrete sets of data. . . . In cultural anthropology there are outstanding examples, of course, of what is essentially an ecological point of view. I may merely mention Dr. Kroeber's study on cultural and natural areas of native North America. The application of an ecological point of view in field research appears to be most profitable when specific facets of culture and social organization are analyzed, such as the relationships between features of social organization to adaptive techniques, to habitat, as exemplified by Steward's classic work in the Basin, and a number of other studies. It is significant that an application of the ecological point of

view in studies of this type has been most fruitful when applied to societies possessing relatively simple technologies; yet the relationship of community to habitat should remain of fundamental concern to that branch of anthropological study that is concerned with forming and testing propositions of general fact.

In any anthropological study that attempts to state valid relationships between or among entities, the distinction between the affirmation of a relationship and its demonstration must be kept carefully in mind. . . . Supposedly related variables should be examined over a period of time, to see whether, in fact, covariation is present. Here, the ecological point of view and the focus of its interest are again hopeful. . . .

I have one further comment to make on the ecological point of view in relation to the study of prehistory. This point is touched on briefly by Dr. Bates. Here, I think, is a most important field waiting and ready for a much more sophisticated treatment than it has so far been accorded. As a brief example from one of my own fields of interest, I would like to cite the point made by Dr. Linton a few sessions ago, that probably in Southeast Asia we have an old center of food cultivation based on taro and yams, probably supplemented by the banana and breadfruit, with protein food undoubtedly coming from the sea. When to this combination there was added a developed form of sea transportation, we had the stage set for the migrations out into Oceania.

Available carbon 14 dates suggest that the first form of ocean-going transport was developed by at least 2000 B.C., that the migrations of man westward into Micronesia and Polynesia had commenced by at least 1500 B.C., and that, consequently, we have a framework of some three thousand years of prehistory in Micronesia and Polynesia together. It seems highly doubtful to me that in this relatively long period of time there occurred any really remarkable changes in culture, any really remarkable cultural growth comparable to those of continental land areas. I think a partial explanation lies in the relationship of man, of communities of men, to the limited resources of their endowment, of their habitat, to the fact that the adaptive techniques were very well adjusted to this habitat, and that few others would really work. . . .

A final remark concerning Dr. Bates's comments on the study of disease in relation to the ecological approach to this subject: It is certainly of great importance that we have a clearer understanding of the part played by disease as the selective biological factor, and as an influence affecting the distribution and migration of prehistoric man. It seems to me that progress in this area of research depends to a large degree on examining prehistoric populations, for it is really the relation of disease to populations, not to individuals, that is the crux of the matter.

KROEBER: I conceive of cultural history as merely one special department or version of natural history in the ultimate. . . . Since the natural history point of view [now transformed into ecology, remains a potent influence in biology, it will continue to exert its influence in anthropology]. . . . I agree also with Dr. Bates's suggestion that man almost certainly had a society before culture. . . . All the evidence we have—from the whole animal kingdom as well as the primate order—indicates that effective culture begins with true man, whereas society extends much farther back into the animal world. [The cultural level, by contrast, is emergent]. . . . Because he tends to be so impressed with culture, the anthropologist [regards the relation of culture to environment in a rather sec-

ondary fashion, often failing to perceive the importance of the natural setting in affecting cultural history. . . . I see an analogy between a culture and an ecological community, in that both are, in a sense, composite accidental associations of traits, in the one case, or species, in the other, which have worked out a certain adjustment to one another and are in ecological balance. The resulting integration is secondary, a kind of mutual tolerance. Integration of a higher and more lasting order in culture might thus arise only under conditions of stability and be partly a function of time. This hypothesis is to be broached only tentatively; it is a view suggesting that cultural integration is not predestinate, an organic growth in a Spenglerian sense, but rather containing some of the fortuitous quality of the biocenose].

DISEASE AS A SELECTIVE FACTOR

OAKLEY: In connection with human ecology, I have heard it said that the aboriginal population of Australia was reduced from something of the order of 500,000 to 50,000 in the course of a century or so after the coming of the white man, and one factor suggested is the spread of disease through clothing, which, before contact with the white man, was, of course, not used. It struck me, when this suggestion was made, that it might conceivably have some bearing, for instance, on the very rapid disappearance of Neanderthal man as far as one can tell, through contact with the Cro-Magnon, with higher culture and tailored clothing.

NADEL: One point which it seems to me was not sufficiently dealt with in Dr. Bates's paper and which is of very great importance to the cultural anthropologist, especially in connection with disease, is the factor of immunity to some of the local varieties of disease, which would affect people migrating into a new territory.

Today, especially, with movement being entirely free, this problem is strongly exemplified in West Africa, where tuberculosis in a severe form appears in areas where it formerly had not existed. Individuals or small groups moving in the course of modern occupations into another territory where there was no tuberculosis now show it very rapidly. It was one of the problems that faced the Nigerian government when it moved teachers from the humid zones in the south to the dry zones in the north, and vice versa.

Of extreme importance in the study of disease is another point, one in the investigation of which I was involved in West Africa and in the Sudan, and which bears directly on social organization. In consequence of the free movement today, the spread of venereal disease, especially of gonorrhea, with its effect on fertility, has already, in some places, led to depopulation, and the social structure no longer fits a population for which it was originally designed. . . .

LINTON: . . . In Madagascar, you have the Malayo-Polynesian type confined to the high plateau, where, until recently, there were no mosquitos. This group was completely surrounded by a zone of Negroid peoples who obviously infiltrated very slowly, because they took over the Malayo-Polynesian languages and cultures practically *in toto*. We also have, in our own Southeast—this is one of the cases where the studies of immunology should get together with archeology—evidences of some catastrophic situation which followed the first Spanish invasion, which fundamentally changed the settlement patterns and apparently greatly reduced the population. The most likely explanation seems to be diseases that were introduced by the Spaniards. . . .

I feel that this primitive and unconscious bacterial warfare has been one of the most important factors in con-

trolling the direction of migration of human groups and the dominance of particular human groups. It is not simply a case of the stranger coming into a region where the local population is immune; it is also a case of moving a population such as West African Negroes, thoroughly impregnated with malignant malaria, into a place like the Tagus Valley in Portugal, where there was a vector already present, and which resulted in a rather prompt elimination of practically all the whites in the region.

HENRY: In considering evolution, one ought to take into consideration recent developments in psychosomatic medicine. The issue is this: that a culture places strains on the alimentary tract, on the respiratory mechanism, on the circulatory tract, which begin to become operative very early in life. One can imagine, although we have no proof for it at the present time, that those organisms that could stand the stresses set up by culture . . . survive, and those that could not died off, so that what we have at the present time are individuals who . . . can withstand some of the stresses of civilization on the soft parts of their anatomy.

It is important to bear in mind the work that has been done on the Norwegian rat. This shows very clearly the changes between the size of the endocrine glands in wild rats and domesticated rats, and I think this would be important also, in determining selective factors in evolution.

MEAD: I think we ought to add to this discussion the points that were made in Dr. Northrop's paper and in Dr. Hoijer's paper; that is, more ought to be included about the way in which the culture organizes the perceptions of the individuals who live in it, in regard to the selective use that they make of their environment, especially under conditions of migration. . . .

We have a certain amount of material that suggests that one of the factors in the mental health or balance of a human individual is his perceptual relationship to the environment, which can be very narrow or very deep. There is, however, a possibility of a balance between them, and when that balance is disturbed, so that there is almost no relationship to the physical environment in people who previously have had such a relationship, as in urbanization, for instance, you may get a disturbance of mental health. This does not mean at all that human beings cannot live in cities, but that it is difficult for them if they come from a peasant or rural background which has prepared them culturally over a very long period for a different relationship to the environment.

PRESTIGE AND FOODS

HENRY: I want to say something which follows somewhat along the lines of discussion by Dr. Mead. The first point has to do with what might be called, roughly, the value systems that arise with respect to the kinds of foods eaten.

One of the things striking to me, in studying southern South America, is the tremendous emphasis of the native tribes on flesh food. It would seem that where there is a long history of interest in flesh foods and the granting of prestige to flesh foods, it becomes very difficult for the people to adopt agriculture or horticulture even where the soil and the climate make this possible.

Another problem that has struck me in studying the Pilaga is the enormous change in social organization that must take place before a tribe will shift from a hunting economy to an agricultural one; certain changes, for example, in the division of labor are very essential in taking over an agricultural way of life. So these two factors of value—views of food and social organization—seem most important when one con-

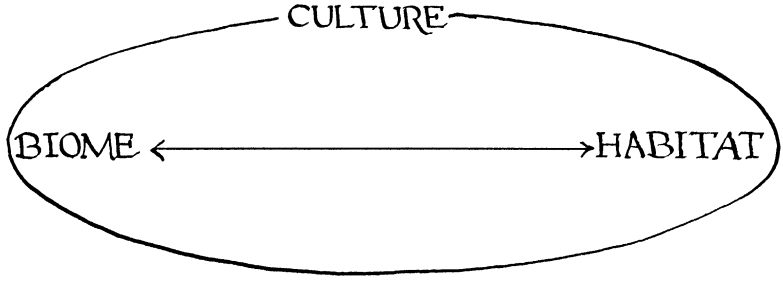
siders the relationship of man to his environment.

MAJUMDAR: Our experiences compare with those of anthropologists in other regions, but in India people who take vegetable food look down on people who take nonvegetable food. Particularly in northern India, there are castes and communities that would not sit at the same table with those who eat flesh foods. . . .

ECOLOGY AND PREHISTORY

CLARK: I want to make one simple point about the continuity from natural history to human history in ecological terms.

My diagrams express the nature of an eco-system in prehuman, precultural times, and we can imagine that in precultural, prehuman conditions, an eco-system would result from a very simple interplay between the biome and the habitat, between all the forms of life found in soil and so forth. On the other hand, as soon as we get into the Pleistocene, we have a curve factor which we can call "culture," and between culture and biome we have reciprocal relations; between culture and habitat we have reciprocal relations.



Those diagrams appear to be rather different, but one must imagine that there really is a continuous development between them. It does seem to me also that these broad stages in human culture that White and Childe and others distinguish—savagery and barbarism, hunting and farming, or whatever you want to call them—do express in a graphic way the gradual magnification in the role of culture in relation to the other elements in the eco-system. After all, Movius' hunter-fisher people were comparatively small fry, particularly in the beginning. The effect of culture on the biome and on habitat was relatively slight; in so far as men domesticated the elements in the biome and perfected a technology which enable them to shape their habitat, this factor of culture became more and more prominent, until, of course, in our own day, with natural science, the leading factor in ecological change is man, human society.

Now, by pollen analysis, for example, you could study in very great detail changes in the vegetation aspect; you could see that in the earlier hunter-fisher stages, the proximity of human beings exerted very little influence indeed, though you do get slight indications. When farming develops or farmers come into a region, however, you at once begin to get transformation. Notably in the areas which are forested, you get the clearance of forests and a total upset in the biome. By studying the biome purely by biological means, you can discover a registration, in fact, of economic change, which, of course, is being produced by developments in the sphere of culture and is very neatly demonstrated, I think.

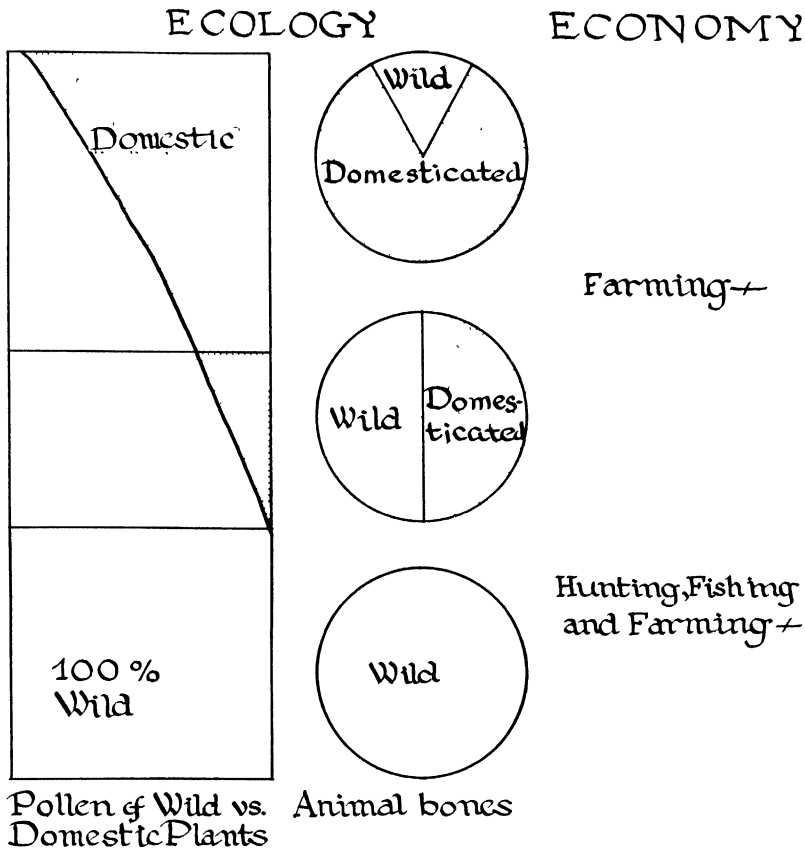
MOVIUS: . . . In the prehistoric pe-

riods, we are tied very intimately in with the natural sciences, and it seems to me of significance that we can only go as far as the natural scientists themselves have gone in their own investigations. If you go to northern Europe, there is nothing older than the Late Pleistocene, and therefore the great effort to make investigations of a geological nature on the part of the natural scientist has been devoted to the Pleistocene.

If we are going to extend out of such an area, we are going to have to take the initiative as students of human culture and organize teams of natural scientists to come and work with us in the field. It is not enough to bring back samples and say, "Here, you are an expert; process these for me." These people have to see the materials and the places where they are collected, under the conditions in which they exist in nature.

THEORY AND PHYSICAL ANTHROPOLOGY

McCOWN: Dr. Washburn's paper is the takeoff point for my remarks, and I want to bring your attention particularly to those statements wherein he says, "Physical anthropology needs a consistent proved theoretical framework." I think what I will say is essentially a difference of phrasing rather than a difference of substance, but I would say that physical anthropology has, and has had, a consistent and a proved theory, and that is the theory of organic evolution. It seems to me that, in so far as Dr. Washburn's dis-



tinction between the old and the new physical anthropology is concerned, the old physical anthropology suffered because it got away from this main, master-theory.

I refer to it as a master-theory because, obviously, any discipline is going to require a theoretical framework which you have appended to and interlocked with a series of subsidiary theories or hypotheses. But the main one has been given us for a long time, and I think that the difficulty has been always that, as far as evolution was concerned, this was interpreted rather narrowly as concerning, primarily, the matter of fossil man. There always has, however, been a minority view, that evolutionary theory was the acceptable major framework within which you could relate problems concerning the living races of man.

Dr. Washburn says that the necessary evolutionary and genetic theories are available. I would say that this is clearly true; this is widely recognized by physical anthropologists at the present time. The main difficulty with the theory of organic evolution . . . lay, I think, primarily in the weakness, for a considerable period of time, of that particular part of the main theory which had to do with the origin of variations. Today there is an understanding of the major ways in which variations in living populations arise. This we owe to genetics.

But another part of the main theory has to do with selection, and, while I do not want to minimize the importance of genetic drift, it is nevertheless perfectly apparent that selection is one of the operative ideas that it is essential to understand with respect to organic evolution. It is here, I think, that the physical anthropologist always has turned, and is turning now very strongly, to the ecologists, because the understanding of selection, so far as it has impact on human populations, involves precisely the kind of things that ecologists have been concerning themselves with for a considerable period of time.

First of all, there is that part of selection usually called "natural selection" that applies to human populations and is thought of as involving the interrelations of a living organism with the physical elements of the environment in which man lives. This is, from the standpoint of biologists, usually not spoken of as "natural selection" any longer; it is simply called "selection." But you have this problem: that man lives in a complex environment, one which has been variously designated, for which I will use the phrase "cultural or social selection."

The result of this is that you have a spectrum of probable selective forces, ranging from the essentially inorganic to these organic and cultural ones, and it is here that ecology and the people working in ecology are valuable allies of the anthropologist interested in problems of the nature and the character of both living and extinct populations.

It seems to me a perfectly sound hypothesis that selection, as far as it applies to human populations, very largely consists of selective factors that operate within human societies rather than in the province of purely nonhuman factors. I think you can almost say that the old natural selection of the zoölogists and the botanists is, for man, to some degree an unnatural kind of selection and that, for man, the natural selective forces are those which he, in a sense, generates within his own societies.

To come back to this problem of theory, the presentation which Dr. Washburn has made so very well concerns primarily the additional subsidiary theoretical frameworks that we need to attach to this major framework, and here we have, I think, the main

goal and the reason that physical anthropology needs a new orientation. . . .

IRREVERSIBILITY OF EVOLUTION

STRAUS: With the main points in Dr. Washburn's paper, I agree quite fully, but I would like to consider a couple of them.

First, the question of adaptive and nonadaptive characters that he discusses is not simply one of choosing adaptive or nonadaptive characters but, as he points out, of finding out whether some characters are nonadaptive. But, even admitting that there are such distinctions, it becomes very difficult to know just how to use them. This was clearly appreciated as early as 1873 by St. George Mivart.[1] It was quite clear to him that this business of separating adaptive and nonadaptive characters and using them in assessing relationships or evolutionary change, or as Gregory and Wood Jones later used the term, "characters of habitus," as opposed to "heritage," overlooks one thing: that you have such things as gene complexes in the body. In gene complexes there would be characters of various sorts and, assuming for the moment that there is a sharp distinction between adaptive and nonadaptive, many so-called "nonadaptive" characters might very well be carried along in the same gene complex with adaptive characters, therefore making analysis of the situation very difficult.

The second point is Dr. Washburn's concept of orthogenesis. It distresses me a little because his interpretation of the meaning of orthogenesis, and its relation to irreversibility, has rather gained currency among some of the anthropologists, as witness the treatment of it by some of the people in the Cold Spring Harbor Symposium.

Orthogenesis, I would agree with Dr. Washburn, is a vitalistic concept for which there is no scientific evidence, and it does probably imply irreversibility. A belief in a modified irreversibility, however, such as some of us entertain, does not necessarily imply orthogenesis, and it seems to me that Dr. Washburn is confusing those two issues. The concept of irreversibility, in a limited sense and not in the sense in which it has been overworked by some of the paleontologists, is still an extremely useful one, but it has no implications about vitalism, internal drive of any sort, or orthogenesis, as is commonly accepted.

BOYD: I agree with Dr. Straus about this question of irreversibility. The point is that, as Dr. Washburn says, an arm could become long or could become shorter by evolution, but it probably would never become the same arm. I think we agree on that, but you would probably agree with me that it could go either way, so in one sense it is reversible and in another sense, of course, it is irreversible. I tried to bring that out in my paper when I discussed orthogenesis.

Then there was this question of the nonadaptive and adaptive characteristics. Now, if we are talking about inherited characteristics, I think there is considerable doubt whether there are any nonadaptive characteristics. I know that people like J. B. S. Haldane believe that practically all human genes are "transients," that is, they are on the way out or on the way in, either they are going to increase to a hundred per cent in the population or they are going to go down to zero, one or the other. They are not going to remain stationary. This is a very fundamental point.

SENYÜREK (submitted in writing after session): I agree wholeheartedly with Dr. Washburn's general point of

[1] St. George Mivart, "On Lepilemur and Cheirojalens, and on the Zoölogical Rank of the Lemuroidea," *Proc. Zool. Soc. London*, 1873, pp. 484–510.

view. In regard to irreversibility, I think two things should be kept separate: one, the changes that affect the proportions, such as the example of arms, given by him. Now, if a character is lost, that is a different thing. I give as an example the postorbital bone which was lost in evolution and has not come back in the evolution of primates. The other item concerns the proportions, and there are some features where reversion has occurred. As an example, consider the pulp cavities. This is only a question of size. Originally they were small in early primates, then became large, and now they are reduced again in most recent men. I think those two should be kept separate; in changes affecting proportions alone, we can have reversibility, provided that the selection has changed.

ORTHOGENESIS

TEILHARD (submitted in writing after session): I strongly object to the habit, recently developed, of rejecting as a matter of fact, both in animal and in human evolution, the word (and even, to a certain extent, the concept) of *orthogenesis*. This word may have been used originally in an old-fashioned and untenable "vitalistic" way. But it has not been replaced so far to express the unquestionable cumulative ("additive") development of characters along certain definite directions, which (although the fact may be somewhat forgotten by the geneticians) is the essence itself of life. The term "ortho-selection" covers a part of the phenomenon—but a part only, since in the case of man (who has become able to control and plan to a certain extent his own biological structures) we certainly have to deal with an "ortho-elective" process of evolution.

CRITIQUE OF RACIAL CLARIFICATION

WASHBURN: I think the whole discussion comes back to Dr. McCown's original point—the question of whether physical anthropology had or had not any theory.

I qualified my remark carefully by saying "a consistent experimentally verified theory," and this is a very different thing from having a theory. It is true that physical anthropologists believe in natural selection—they all tell you that—but they then specifically state in many cases that they base their classification on nonadaptive characters. If one consistently carries out the idea of natural selection and adaptive radiation, it is not consistent to base the classification solely on nonadaptive characters. . . .

There are, of course, theories of race, but some of the theories are based on types, individuals pulled out of populations, and others are based on frequencies of independent traits in populations, whether eye color or hair form or blood groups, and these happen to be inconsistent ideas.

As Professor Vallois pointed out in his paper, the same individual frequently uses two different concepts of race, and this is why I say physical anthropology as a science had no consistent theory. If it is possible to use a race concept based on type on Monday and on population on Tuesday and be back on type on Wednesday, you do not have a consistent theory. That is the state that physical anthropology has been in. . . .

The point is that, after having shown that the form of the skull was very dependent upon, we will say, a muscle, what did people do? They measured the skull and acted as if it had no relation to muscles whatsoever, and, in this sense, whatever result came out of the experiment was thrown away, because it did not go into a useful, consistent, theoretical framework. The same problem comes up in terms of human constitution. If genetics shows that the items which go into the evalua-

tion of the type are, in fact, independent variables, then genetics is contrary to a classification such as Sheldon's. You cannot say on page 1, "I like genetics," and on page 2 use a type which includes, in the definition of the type, a variety of things which vary independently genetically. Therefore, I say the key issue in the whole discussion is a consistent theory which is experimentally verified and which is used.

One final point on irreversibility: We are all told that irreversibility cannot take place because somebody says, "Well, something disappeared and it hasn't come back," but they do not take the illustrations where something has disappeared and has come back. These probably come back because of different gene complexes, in this sense not genetically identical—nobody is going to argue that. That is all we have. Early mammals had four molars—so have some baboons. There is no indication that the intermediates had four molars. Early guinea pigs had more digits than the present guinea pigs. Sewall Wright has a strain of guinea pigs with more digits. So the whole digits, whole teeth, whole things which, from the point of view of the phenotype, have completely disappeared for millions of years of time come back. I do not care what you want to call it, but I call it "reversibility."

ANTHROPOLOGY, HUMANITIES, AND THE SOCIAL SCIENCES

KROEBER: Redfield remarks that humanity as a subject matter provides the peculiarity that human, cultural material is more easily treated by the method of descriptive integration than that of generalizing science. . . . When I have expressed myself on this point I have used the term "historical." However, the idea of sequence of time as the essential factor in history is so deeply implanted that, no matter how often you affirm that the historical approach is not necessarily chronological, the impression remains. The term "descriptive integration" is much more effective, including also the idea of integration, which I have expressed by putting emphasis on context. I find myself in complete agreement with Redfield.

What he says about the different kinds of models which have been attempted—the natural science or causal model, the functional model, the symbolic, the aesthetic, and the logical model—raises the question whether all but the first are not wrong, if anthropology is to be a science. I have met that issue by saying that anthropology is not or does not have to be a science or is not wholly a science. I find at least this corroboration in Redfield: "The dominance in anthropology of models associated with the natural sciences is not matched by corresponding success in executing studies based on these models," another way of saying that the integrative approach yields more ready results. . . .

Important also is Redfield's assertion of the several polarities which exist in anthropology and that these result in certain tensions. Anthropology is certainly one of the most centrifugal of the sciences taking in an enormous territory. Sometimes we are amicably laughed at by people in fields with five or ten times as many members working at a much narrower range of problems. As Redfield says, our disposition to become concerned with questions marginal to any sector of the immense and variegated study of man nevertheless makes anthropology the freest and most explorative of the sciences. I think that is a claim we can justly make. Lately I have differentiated between cultural anthropology and sociology in terms of the clientele recruited by each: people with well-established centrifugal exploratory tendencies for the remote and marginal become anthropologists,

whereas those who have strong centripetal tendencies, who want the familiar and the repetitive, who do not want intellectual adventure but who want a certain tightness of what they operate with, become sociologists.

What gives us a great deal to think about in Redfield's paper is that, although in university curricula anthropology is almost always classified first as a social science, that practically does not come into Redfield's discussion. He sees a natural science element in anthropology, and he recognizes something in it related to the humanities. The question is whether we have two ends without a middle. The further question is: Is there a social science, and what is it?

LOWIE: There are certain aspects of cultural anthropology which permit of a rigid scientific approach. . . . Kinship terminologies, for instance, can be studied with the same logical processes with which one studies a natural phenomenon. There are other aspects which simply are not amenable to that type of treatment. I am personally very sympathetic with the type of work which has been done by people of artistic temperament, who nevertheless have given us a marvelous picture of the totality of cultures. . . .

LÉVI-STRAUSS: . . . We overlook the fact that in anthropology we start with a nonexperimental situation. We have no experience of the foreign culture, do not understand it, and common sense says, "This is meaningless, it does not belong to us, this is repulsive." So the first stage, which corresponds to the descriptive-integrative stage of Professor Redfield, is to make a nonexperimental situation experimental. Once it has been made experimental, we must try to find out invariant relations or laws. The humanistic side of anthropology corresponds to a first and absolutely indispensable stage which does not preclude, after this stage has been reached, the natural science side.

VOEGELIN: I was struck with Redfield's good judgment in not putting anything about language in his paper. I have annoyed some linguists by saying linguistics is humanistic and belongs with the humanities. It is a sort of personality preference these days that linguistics belongs to the social sciences; in general, the operations of linguistics would seem to be more of the scientific sort, and that is why people do not want to have it called humanistic. . . .

If you have an open corpus, you are constantly being analytical, and you can never close it. If, however, you want to close your corpus, you close it in order to get a descriptive integration, or holistic effect. I like to close the corpus, and therefore I like linguistics to be humanistic.

TEILHARD (statement revised after session): As in the case of "orthogenesis," I wish to react against the spreading fashion of separating from one another (as merely analogous) *zoölogical* and *social* evolutions. Of course, the irreversible groupings of millions of individual human brains into a single physico-psychic superunit is not of the same order as (but of a *higher* order than) the grouping of billions of neurons into a single human brain. But in both cases we witness the same basic process of an increasing arrangement of matter resulting in a higher level of indetermination and psychic awareness. This identity of process is not only a sufficient, but a *cogent,* reason to decide scientifically that the two phenomena are of the same physical nature, representing two successive steps in a single fundamental drift of the universe toward higher stages of associated "complexity" and "consciousness."

NADEL: Professor Lévi-Strauss said the difference between the scientific approach and the anthropological approach is that we have to start by describing. Two rigorous sciences—geol-

ogy and astronomy—have to start the same way. So that is not a criterion which rules out anthropology.

On one page of Redfield's article, he speaks of the "immediately apprehended real whole which is typical of the anthropological approach." I do not believe that we ever do immediately apprehend the whole. It is precisely the difference between anthropology and, say, the aesthetic approach that we do not give way to this impressionism. The whole grows out of a very careful analysis which uses precisely the criteria which Dr. Redfield so ably analyzes, such as the functional model, the model of logical consistency, and the causal model. The whole is the end-result and not the immediate effect of our just observing.

MEAD: To go back to what Professor Kroeber said of us as the widest and most exploratory of the sciences or disciplines . . . it looks as if we might reasonably say that the present separation is between the humanities as a historical discipline that has never discovered that man was an animal, the natural sciences that have been unwilling to discover that he had consciousness, and the social sciences that have been busy copying out-of-date physics. That situation is one that it is our task to continue to bridge. . . .

BIDNEY: . . . I think that there has been a lot of lip service paid to anthropology as being the most integrating or integrative of the sciences and, in practice, a tendency to treat culture as if it were an autonomous kind of entity which can be treated by itself. The most encouraging aspect of the discussion today is our rediscovery that, unless we integrate within anthropology itself the study of culture with the various other aspects of man, we will not have the integrative discipline we are looking forward to.

CLARK: I read with very great astonishment [Redfield's statement] about the hierarchy of status "wherein the 'harder' natural sciences occupy the uppermost positions and the humanist is farther down." This may be true in the New World; it certainly is not in the Old. With humanists [at, say, Oxford] the university is a human institution into which scientists have intruded themselves. . . . In my own university, anthropology is classified as a science by the bureaucrats, except for one small section which deals with the Dark Ages and has literature and [its devotees] are on a lower pay level. There you see the Cambridge emphasis. It is interesting that the Royal Society will admit a few anthropologists but will not admit physical anthropologists and usually puts their work in some other field. The British Academy will admit archeologists easily but anthropologists very selectively. It is obvious that neither the Royal Society nor the British Academy knows what to do with anthropologists.

TANNER: . . . I was rather moved to say this morning that it seems strange to be talking about the strategy of physical anthropology, because I do not think physical anthropology exists. It is the application of a series of techniques and methods to the solution of problems concerning man in some way, but all its problems and all its techniques are used in other fields, more so than in physical anthropology. I think it would be a hindrance to the subject to think of it otherwise than as a congeries of techniques applied in a particular way.

STRAUS: . . . I do not think that anthropology exists as a distinct entity as physics does. It exists merely as a meeting ground of people interested in man. Therefore, it inevitably must borrow from the techniques of various fields—from humanities and the natural sciences. . . . I should not call it a discipline but rather a field in which diverse people come together because of their interest in one rather baffling creature, Homo, and his predecessors.

LINTON: I think the outstanding difference between anthropology and the other sciences—and I class it as a science—is that here you have an attempt to bring together various disciplines of research for the elucidation of one species, whereas the other sciences are always interested in particular orders of phenomena and follow these across species lines. Anthropology is a focal point for other sciences. It can very easily be torn limb from limb, or it can be made a real focal point of research.

LOWIE: Geography would be part of it.

LÉVI-STRAUSS: . . . We all agree that anthropology has a close relationship with the humanities, the social sciences, and the natural sciences. Another point of view is that of the depth at which those sciences work. It seems to me that both the humanities and the natural sciences work at a deep level, while, so far, social sciences remain quite on the surface level. In most cases the so-called "social sciences" are not sciences at all. They are, rather, arts that are trying to use the very little we know about social phenomena for a better kind of living. . . . Anthropology tries to jump to a deep level, while social sciences have been so far unable to do so, and the result is an extremely difficult situation for co-operation between anthropology and the social sciences. This should be the first problem overcome.

LINTON: I have been repeatedly puzzled by how to distinguish between cultural anthropology and sociology. It seems to me that the best test is a pragmatic one. Anthropological techniques for this type of depth research actually are applicable only to small organized groups, the *Gemeinschaft*. You cannot use them on *Gesellschaft* situations unless you analyze the *Gesellschaft* into its component subsocieties and subcultures and follow by a careful selection of the individuals or *Gemeinschaft* groups within the *Gesellschaft*. If you want to find out the things sociologists want to find out, you go questionnaire in hand. If you want to find out the things anthropologists want to find out, you go with a packet of cigarettes and infinite patience.

MANDELBAUM: I wonder if Dr. Lévi-Strauss would define what he means by a "deep science," and whether Dr. Linton would agree with that definition?

LÉVI-STRAUSS: I feel that Dr. Linton was quite in agreement with what I tried to say. I did not use the word "a deep science," but a science working at a deep level, not limiting itself to empirical observation of the data but trying to build up out of these data what may be called "scientific facts."

MEAD (submitted in writing after session): Professor Lévi-Strauss characterizes anthropology as working "in depth" as contrasted with sociology. One way of amplifying this statement is to say that anthropology uses methods of analysis from different levels (embryology, geology) with different units and different time scales, cross-cutting all disciplines relating man to nature, on the one hand, and in history, on the other, while sociology is an analysis on a single level, leaving psychological and biological analyses to other disciplines.

HAAS (submitted in writing after session): The best of the humanists are usually well versed in a single culture or even a single period of a single culture. It cannot be denied that even a lifetime of study is insufficient for the proper understanding of a culture. On the other hand, the anthropologist has been characterized by the breadth of the scope of his studies, and he thus gains a perspective that is denied the humanist. Both breadth and depth are needed, however, and it is precisely in this aspect where the anthropologist and the humanist need more contact with each other. Each has much to learn from the other.

HALLOWELL: I would like to combine the remarks by Professor Linton and Dr. Bidney. For a long time, we looked around for some distinctive phenomena on which we, as anthropologists, could center our attention. The phenomenon we found was culture and in a certain kind of interpretation of levels, if you look upon culture as a special emergent, this is a phenomenon for anthropology. The emphasis we have laid on culture is in line with the historical developments in other sciences in terms of phenomena. On the other hand, I agree with Dr. Bidney. There is some kind of shift going on, and in the kind of discussion we are having here our real focus is man in respect to all sorts of phenomena. I think this is actually going to show that, if we confine ourselves to the study of culture and only culture, we will limit our future. By making this shift, I think something very interesting is going to result.

BATES: We get lost in words very easily. It seems to me that all science is essentially an art form, so that how we distinguish the one thing from the other has always puzzled me. . . .

We have to have labels—anthropology, ecology, history, humanities, social sciences. . . . But we ought to look at specialization and labeling as an unfortunate necessity rather than as a desirable thing in itself. The trouble is that when we get our label we freeze the pattern. . . . We have to specialize; that is, we talk about anthropology becoming so broad that no one can understand it all. . . . But how do we narrow it down? It seems to me that each of us should be at liberty to pick a point of interest and work at it without worrying that we are getting involved with humanities or sociology or histology or anthropology or what not. . . .

LINTON: There is a story that in the revolution in Germany which followed World War I, a group of workmen were chasing a Prussian officer in uniform. They came to a park with "Keep off the Grass" signs. The workmen, having been properly trained, stopped at the edge of the grass. The officer, from a more liberated stratum in the society, sprinted across the grass and got away. Repeatedly, in the pursuit of scientific problems, one comes to "Keep off the Grass" signs; perhaps whether you pay attention to them or whether you run your problem down is a question of the individual's early conditioning.

BIDNEY: . . . Historically and at present, one of the major theoretical issues concerns the relationship of anthropology and sociology. The issue is whether you are going to take your culture as an ultimate level of reality and integrate everything with reference to it or whether you are going to take society in your social structure. This is basic and is not to be dismissed simply as a pragmatic issue that we can resolve.

A second point, on the question of synthesis: Anthropologists do not need to be told that theirs is an integrative and synthesizing science; our whole discussion bears this out. What is not sufficiently appreciated is that anthropology is not a self-sufficient science to which all other major disciplines in part contribute, but requires co-operation with the humanities and the other social sciences as more or less independent disciplines. Otherwise, there is a tendency in the name of "integration" to set up a new kind of cultural imperialism, which the other people do not like.

OAKLEY: Hallowell's point that culture is the emergent, and the focal point is well taken. We have had several illustrations of how physical anthropology can be fully understood only in terms of man as a cultural creature.

REDFIELD: . . . Dr. Kroeber spoke well of the phrase "descriptive integra-

tion." (My impression was that I had borrowed it from him.) He also spoke well of my conclusion, and the proposition that anthropology is not only a science. This is related to the question as to what social science is. I would answer both questions as follows: The social sciences—and anthropology—are ways of understanding man in society which in some lines of investigation and with regard to some subject matter correspond pretty closely to the procedures and the ways of organizing knowledge which the physicist uses but in other parts and places correspond rather faintly, or not at all. . . . Dr. Kroeber remarked that descriptive integration is apparently congenial to the subject matter of humanity, and to this proposition I would only add that the explanation seems to me to lie in the fact that we who do the describing are human beings ourselves. . . . We see the world outside us as a reality which is somehow ourselves in reflection, and it is always something of an effort to bend it, to break it, to analyze it. Bending and breaking is easier if we put our attention upon some particular subject —a stone tool or a highly abstracted system of kinship, or the like. But we are forever drawn back to the reality.

There seem to be just two points of difference between me and others. Professor Clark's surprise at the description I gave of the academic hierarchy is easily explained in terms of local differences. . . . Professor Nadel's surprise that I said something about the direct apprehension of reality in my paper suggests in part a misunderstanding and in part differences in the way two men work. With respect to the latter, some men look at what they are studying—a personality, or a culture, or a society—and right away put it down as a whole, later modifying and substantiating what they say, while others find small things which they can add to others to assemble a whole. . . .

Dr. Bidney's expression of satisfaction that anthropology is again interested in man rather than only or especially in culture reminds me of something that Sidney Oliver wrote a good many years ago about psychology and anthropology. . . . He said that anthropology had named itself the "science of man" but was concerned with all sorts of special things about man and not about man as a whole. We have now a healthy return toward the whole man, the integrated man. But, with regard to integration, here I think Dr. Mead is right in saying that we must keep our exploratory nature. Anthropology is going to continue to bother people because it will want to be put in so many places and no place that you put it will really be right. That is good, and anthropology, by threatening its own disintegration, is a great integrating factor in human knowledge. . . .

NADEL: The Oxford dictionary defines "process" as the fact of going on or being carried on, continuous and regular action or succession of actions, continued movement, definable progression, leading to accomplishment of some result.

I here merely pull together a few facts that came out in the discussion around this definition of process as it happens in the phenomena that we observe. In Carter's paper the distinction is made between endogenous and exogenous processes, or evolution. It seems to me that is one criterion which we implicitly applied when we talked about evolution. We were thinking of endogenous processes, so far as man as a biological entity is concerned. When we talked about ecology, we were talking exogenous or adaptation processes, so far as man as a biological unit is concerned. There we switched over to adjustment to cultural conditions or the effect of physical conditions upon culture, and the exact parallel to these exogenous processes in culture itself

would be acculturation. We have not, unfortunately, had a full discussion on what is or might be the phenomenon analogous to endogenous biological processes in culture. . . .

Professor Redfield's paper puts the whole discussion on a new level, yet still very closely revolving upon this central motif of our discussion. . . . In natural science we always try to emphasize the deterministic element in the process and its compulsory nature. Professor Redfield's paper clearly emphasizes that the humanistic disciplines loosen up the compulsion and the determination. Where he speaks of values and the value of the observer himself, I think we also allow some thought of creative freedom to intrude, and, as he says, this may lead us into philosophy. I, for one, hope so.

CASO (submitted in writing after session as "Notas al estudio del Dr. Robert Redfield"): Según creo, Historia y Ciencia son dos métodos diferentes para el conocimiento de la realidad. La diferencia fundamental entre ambos métodos, es que el histórico trata de entregarnos la individualidad del fenómeno, con toda su riqueza de cualidades concretas, mientras que el científico tratade entregarnos la *relación necesaria* que existe entre un fenómeno y otro, o sea la ley, que es aplicable a fenómenos semejantes.

Lo que determina la aplicación de uno u otro de estos dos métodos a un fenómeno, no es una cualidad intrínseca en el fenómeno mismo; es decir, no hay fenómenos históricos y fenómenos científicos. Lo que determina la aplicación es nuestro *interés humano* en el fenómeno.

En efecto, todo lo que existe objetivamente es individual, y en consecuencia podría ser materia de conocimiento histórico, pero no todo lo que existe nos interesa individualmente. Por ejemplo, un cristal o un insecto son individuos, no pueden repetirse en la naturaleza; pero no nos interesa en ellos su individualidad sino su semejanza con otros seres. Los fenómenos que en ellos observamos, solo tienen importancia para nosotros, en cuanto que manifiestan en un ser individual, lo que sucede en el género; por eso no los consideramos sujetos de conocimiento histórico, sino de conocimiento científico.

Aun en el caso de que intentáramos una descripción pormenorizada de su individualidad actual, o del proceso que los ha hecho nacer y transformarse, una especie de biografía, siempre sería la de un individuo tomado como modelo de un género, no la del individuo mismo.

Desde este punto de vista, una rosa, Julio César o la cultura maya, son idénticos en cuanto que son individuos; pero lo que nos interesa en la rosa intelectualmente no es su apariencia única, ni el especial proceso que la hace nacer, abrirse y morir, sino su semejanza con los otros individuos de su especie. Las complejas causas que motivaron que uno de sus pétalos cayera hoy, no nos importan en su aspecto particular. Todo lo contrario sucede si el individuo es Julio César o la cultura maya. Aqui estamos en presencia de individualidades que nos interesa conocer como tales, y las causas que motivaron la personalidad de los actos de Julio César o el modo de vidade los mayas, el proceso que forma su biografía, es precisamente lo que estudiamos de un modo histórico.

La historia es entonces universal.[2] Todo tiene historia o puede tenerla, y será nuestro interés el que determine si un ser o un conjunto de seres debe ser estudiado histórica o científicamente. Pero la ciencia nunca trabaja con individuos, trabaja siempre con conceptos o géneros, y éstos se forman suponiendo que las semejanzas que

[2] Antonio Caso, *El Concepto de la historia universal* ("Ediciones México moderno" [Mexico City, 1923]).

existen entre los individuos, pueden validamente ser consideradas identidades. Podríamos decir que el concepto consiste en la identificación de semejanzas.

Por eso las leyes solo se aplican como tendencias, nunca como la expresión de lo que ocurre en la realidad; porque la realidad es individual y las leyes se han formado con falsas identificaciones de semejanzas.

Podemos hacer otra consideración; no solo en la descripción científica sino en la simple expresión verbal de todos los días, nos es imposible referirnos a un objeto si no es empleando términos generales, es decir conceptos. Si describimos un hecho cultural, como por ejemplo un mito, nuestra descripción para ser inteligible, tiene que utilizar conceptos generales que van precisando el objeto hasta lograr que nos formemos una imagen que ya tenga características individuales. Podríamos decir entonces que la descripción de un objeto individual se logra por la "intersección" de conceptos generales. Por otra parte, una ley no es applicable a un género como tal. Toda ley se aplica a situaciones individuales y en casos concretos. De tal modo que parece existir una aparente contradicción entre la descripción histórica que tiene que emplear conceptos generales para llegar a la integración de la individualidad, y la ley científica que partiendo de observaciones individuales se eleva a la generalidad, pero tiene que aplicarse forzosamente a individuos concretos.

Las ciencias antropológicas tienen un propósito que podría pensarse paradójico, puesto que por una parte pretenden con el método histórico, describir una cultura o un aspecto de est cultura, ya sea en forma estática o dinámica. Pero por otra parte pretenden elevarse a reglas generales o leyes, aplicables cada vez a un mayor número de casos. Y esto último parece inevitable si el antropólogo pretende no solo saber sino aplicar sus conocimientos, lo que supone la obtención de fórmulas generales de las que pueden deducirse reglas que se apliquen no sólo a los casos observados sino precisamente a los no observados, como hacen todas las ciencias.

Una regla lógica dice que si aumentamos la denotación de un concepto, disminuye su connoctación. Es decir, que si un concepto es aplicable a mayor número de individuos, significará menor número de cualidades o características. Entonces el único modo de lograr que una generalización en antropología social se aplique a mayor número de culturas, es perder indudablemente muchos de los aspectos concretos de cada cultura.

Podemos decir de un modo teórico y fundados en el principio de uniformidad de la naturaleza, que en la antropología, como en cualquier otra ciencia, si las condiciones son iguales, debe producirse el mismo fenómeno. Y esta afirmación teórica no es discutible, pero el hecho social es tan complejo, tan único, que nunca estamos seguros de que las condiciones sean iguales. Por esa razón las apresuradas conclusiones de los filósofos de la historia, han resultado útiles en cuanto que han provocado el conocimiento de las sociedades humanas, pero inútiles si se las toma como la expresión de leyes generales aplicables a todas las culturas, bien sea en una forma estática o en el proceso de su desarrollo.

Supongamos que estudiamos la organización económica de las tierras de los nobles aztecas y de las grandes haciendas mexicanas, y que decimos que eran regímenes feudales; en el acto advertimos que nuestra generalización solo es válida en términos muy vagos, pues si bien es cierto que por ejemplo el *mayeque*, el peón y el siervo no podían abandonar la tierra y pasaban con ella al nuevo propietario, hay en cambio enormes diferencias que hacen

inconfundibles y únicas las tres situaciones.

¿Quiere decir entonces que la Antropología Social es imposible como ciencia, que nuestras generalizaciones en cuanto se vuelven más generales, se volverán también más vacías de contenido, hasta que al llegar a las últimas se vuelvan lugares comunes, para cuya formulación no se necesita el conocimiento antropológico?

Que tales generalizaciones pueden hacerse, es evidente; hablar de cultura es una primera generalización, y lo mismo diríamos si hablamos de lengua, religión, arte, organización social o de términos aun mas concretos como sintaxis, politeísmo, estilo, monarquía etc. Pero una ciencia no consiste solo en la definición de sus términos o sea de los conceptos generales que emplea, ni tampoco en la clasificación de los fenómenos que estudia. La definición de conceptos y clasificación de fenómenos son sólo los primeros pasos para el fin fundamental de la ciencia, que es establecer relaciones de causa a efecto entre los fenómenos, que le permitan sacar conclusiones generales, aplicables a la previsión del futuro.

El problema no consiste en saber si podemos formular definiciones generales en antropología; ni siquiera si podemos lograr clasificaciones en esta ciencia; consiste en saber si podremos descubrir leyes generales en los fenómenos culturales.

En conclusión, diríamos que el hombre, en su totalidad física, psicológica y cultural,—objeto del estudio antropológico—, es tan interesante para el hombre mismo, que siempre será estudiado históricamente y, en segundo lugar, que una ciencia tan nueva como la antropología, necesita pasar por un período previo de definición de conceptos y clasificación de tipos, período que llamaríamos descriptivo, antes de elevarse a la categoría de una ciencia abstracta, en la que posiblemente, como en las otras ciencias, el método matemático llegue a formular leyes aplicables a los fenómenos futuros.

CHAPTER X

Problems of Application: Results

Chairmen: George P. Murdock and Muzaffer Senyürek.

Papers discussed: Newman, "Applied Anthropometry";* Ackerknecht, "Paleopathology"; Tanner, "Growth and Constitution";* Caudill, "Applied Anthropology in Medicine";* Chapple, "Applied Anthropology in Industry."

Speakers: McCown, Senyurek, Newman, Shah, Ivaničck, Bird, Lowie, Tanner, Murdock, Boyd, Ackerknecht,† Washburn,† Nadel, Mead, Bates, Monge,† Rowe, Hallowell,† Sangvichien, Métraux, Fejos, Majumdar, Held, Paul, Caudill.

RANGE OF APPLICATION OF PHYSICAL ANTHROPOLOGY

McCown: I want to say, first, that, if Dr. Newman had made a list . . . of all the things that are being done in applied physical anthropology, it would have been a fairly long and very diverse one. But I think the importance of his paper lies in the attempt to assess the strategy of applied physical anthropology. . . .

I think he has estimated well the strengths of this particular discipline. I think he has also estimated quite well its weaknesses, even its principal weakness . . . a weakness with regard to theory.

You come up against a basic problem here that I think is not only analogous to, but actually identical with, that faced by engineers, in contrast to people working in the physical sciences, and that is what constitutes engineering theory as against theory in physics, chemistry, psychology, or what not. The engineer has the same difficulty that is plaguing the physical anthropologist, but with the latter it is more acute because there has been a tendency to speculate rather than to test hypotheses. I refer particularly to what might be called "subsidiary hypotheses," ultimately depending on what I regard as the sort of master-hypothesis that applies to physical anthropology.

Practically, as far as I can see, the application of anthropometry, used at the present time, involves little, if anything, in the way of theoretical constructs. It is taking a method, which has been applied in rather inadequate ways, I think, to test theory in the main field of pure physical anthropology; it is taking a methodology which has proved of some usefulness there—I say "some" advisedly—. . . and making such application of it as we can.

On the other side of the picture . . . there is a real stimulation in terms of dealing with concrete problems. The actual prosecution of the projects for which one is under contract with some agency, either governmental or otherwise, brings one up very sharply to face the realities that one does not have for use either theory or, in many cases, a sufficient body of adequate data that are part of the area of pure physical anthropology.

Dr. Newman brings this point out very clearly. Not alone are measurements of bodily dimensions—linear, surface ones—involved. The fact that we are up against is that we have only

recently reached the stage of trying to secure adequate data with regard to such questions as estimating stature, with respect to skeletal material. . . . We have been using a purely rough-and-ready estimate of plus or minus 1 inch, without knowing whether or not this is really adequate. It is a very large standard error, I would say, not based on anything more than intuition that comes out of a certain amount of practical experience.

For many types of problems we have absolutely no way of making adequate assessments of age. . . . Now one aspect of applied physical anthropology, in the sense that Dr. Newman alludes to only very briefly . . . has to do with the identification of the dead. We face the consequences of a responsibility assumed by the United States Department of Defense of identifying any man, dead or alive, who is a member of the Department of Defense. The consequences of this have clearly not been envisaged by the agencies charged with executing this particular responsibility. We are up against the situation that we do not have good age data, and this despite a very considerable amount of published material.

Therefore, the two sets of procedures —those of physical anthropology proper and those of applied physical anthropology—are, I think, in the process of interacting very well. Applied physical anthropology may well have one of the effects which Dr. Washburn envisages, in that it will serve as a real set of stimulators for work, asking and posing basic problems with regard to particular parts of both procedures and, more importantly, the theory in pure physical anthropology.

SENYUREK: I was struck particularly in Dr. Newman's paper by the fact that he gives only a few instances in which physical anthropology has been applied —that is to shoes, clothing, and comfort requirements. . . . I shall bring up certain things in the case of dentition. We physical anthropologists have, for a long time, been studying the dentition of primitive peoples. We have noticed certain things; for instance, that the teeth are badly worn, in many instances coming down to the gums. This we have attributed, of course, to the amount of grit contained in the food and to the large amount of exercise which the teeth received. But also we have noticed that primitive people have very little caries in their teeth. We have the converse situation in the civilized world today; now that food is soft and refined so that teeth are not worn, there is caries. Dr. Campbell, an Australian dentist who has inquired into this situation, says the teeth of primitive people are free from caries because primitive people exercise their teeth, and that the constant attrition prevents caries. This is one hypothesis, and I am not saying that the question has been solved. Nevertheless, our knowledge of primitive peoples can be used with respect to the dental and medical sciences as testing certain hypotheses.

I shall not go into definition in detail, but I am one who believes that various phases of physical anthropology, for example, can be applied in industry and other human occupations, and that physical anthropologists, wherever possible, should ascertain whether or not what they are doing in the line of pure research can be applied. . . . We must be aggressive salesmen and not wait for industry to come to us; when we have a problem in mind and can solve it, we must go direct and say that we can do better, or improve the conditions, or save money. I think if we can do that, industry will be interested. . . .

I learned from Dr. Newman's paper that, of all the universities in America, only at Harvard is there a single course in applied physical anthropology. This is really amazing, because America is now leading in the field of anthropol-

ogy. I think some courses should be given in the universities, not only in departments of anthropology but also in the engineering schools, because, as I pointed out before, in matters of clothing and engineering, the problems are all different. The applied anthropologist must know not only his anthropometry and bones but also something about engineering and about various mechanical devices. In departments of anthropology there should be courses on various phases of applied anthropology, as given now, and in engineering schools some courses on anthropometry.

NEWMAN: I have just one exception to take to Dr. Senyürek's statement, advocating giving applied anthropology to engineers. There is nothing more dangerous. It is exactly like taking an amateur pothunter, giving him a brief manual, and turning him loose with your blessing, to wreck, essentially, a lot of work that other men could be performing constructively. . . .

SHAH (statement revised after session): If anthropology is to keep its hold as a useful and practical science in the field of human relations, it has to adopt the more exact technique of human biology and help the evolution of modern man by a series of investigations in human biometrics in which measurements take an increasingly predominant part. A time will soon come for extending the attention of research workers beyond the study of the culture and social structure of the primitive tribes to the study of the factors that determine their health, nutrition, and physical efficiency, so that they are not a drag on the rest of the humanity but contribute to the production, progress, and peace of the world as assimilated members of the modern society. I would here make a query as to whether the anthropologists attending the symposium can give information concerning the extent to which anthropology has contributed to improvements in international biometric standards. Without insisting on full uniformity, I think it is desirable that an international meeting like the present one should help in organizing a committee to determine a standard list of, say, about twenty anthropometric or biometric measurements, laying down a uniform technique for each of them. These might then be adopted throughout the world in anthropological measurements of either rural or urban, primitive or advanced, communities.

IVANIČEK (submitted in writing after session): . . . The need of collaboration between the anthropologists and the specialists in human pathology and clinical medicine, surgery, gynecology, etc., should be specially emphasized. The student of medicine, during his ordinary medical study, has very few opportunities to hear about comparative racial and constitutional anatomy and physiology—things that are well known to the physical anthropologist. The collaboration of an anthropologist or of a physician with the necessary anthropological specialization is very necessary, not only for theoretical research, but also in the research for practical applications of clinical therapy. Some kinds of human diseases are direct objects for anthropological research. Research in hereditary and allergic diseases or in vocational diseases in the army and in industry are examples. The specialist in forensic medicine in the future will not be competently trained without a grasp of certain fields of physical anthropology. The problem of doing research work on the inclination of some races and some constitutional types toward certain diseases is a special field, also, for the anthropologists. . . .

BIRD (submitted in writing after session): The necessity for similar collaboration and integration between archeology and paleopathology was

mentioned by Ackerknecht in his review of the present status of paleopathology. This needs emphasis, for, as he points out, if we are truly to evaluate the past record of human society, we must understand all the factors affecting it. In view of the potentialities of sites in Peru and northern Chile, it is surprising that no field program in that area has ever taken full advantage of the opportunities for the recovery of pertinent material. These are far more favorable than has been realized, for even Ackerknecht does not mention the identification of intestinal parasites from the dried feces available. Where a good sample of the former population, many in mumified condition, can be secured and accurately correlated with the cultural record, it is distressing that the gathering of pathological data should be neglected. In the only instance I know of where a trained pathologist was included in an archeological field party in Peru, illness prevented. Examination of the material collected without his guidance makes it all too clear that this was a major loss to the project. We can only hope that Ackerknecht's work will focus more interest on this problem.

LOWIE: The late Professor Boas was intensely interested for a number of years in the problem of the chronological relations of certain developments of an anatomical or physiological character. He was interested, for example, in the question of whether, if the first incisor, let us say, appears at a very early stage, the first molar appears at a very early stage also, or whether there was a correlation between the appearance of the molars and the appearance of, let us say, the menopause.

This is an eminently practical question, as I discovered when, on behalf of the National Research Council, I went to one of the vice-presidents of the Metropolitan Life Insurance Company, who assured me that he regarded the problem as an extremely important one from the point of view of life insurance companies. I do not know whether anything vital has been done subsequently, and I should be interested to learn what the status of that particular problem may be.

TANNER: So far as I am aware, there are no studies on the relation of time of appearance of incisor or molar teeth and the menopause, which is the thing you suggest . . . because there have been no longitudinal studies—studies of the same people continuing throughout their life—in the adult range, except for one study on males, who do not suffer this phenomenon. . . . All I can say about it, without notes, is that there is some relation between the appearance of incisors and molars. . . . There is work going on in about five places on that, but no hard-and-fast results have been obtained.

VALIDITY OF CONSTITUTIONAL STUDIES

MURDOCK: The paper by Dr. Tanner, on the subject of growth and constitution, may prove controversial. This is, to an individual like myself outside the field of physical anthropology, an admirably organized and seemingly complete survey. I am, however, skeptical as to the validity of this general approach, which . . . seems to challenge the conclusions of genetics on the biological side and of bodies of psychological theory, such as learning theory and psychoanalysis, on the other. . . .

BOYD: I have always had considerable reservations about work on constitution, because I could not see quite how one would analyze it genetically, and the constitutionalists themselves seem to have made no attempt to analyze it genetically. In fact, if you talk to Dr. Sheldon, it becomes apparent that he is not even interested in analyzing it genetically.

Furthermore, the question of the reality of the components which the

constitutionalists separate seems to me a very real one. . . .

I ask you, looking at the human body or a photograph of a human nude, can you actually decide how much of which of three components this individual has, accurately enough for it to mean anything? In my opinion you cannot.

NEWMAN: Dr. Boyd is confusing constitution and Sheldon. Sheldon is simply the most vocal advocate of constitution. There are now, or there will be in the next few years, methods of quantifying exactly the same thing that Sheldon has been talking about. . . . The great difficulty that most people feel is that this is a purely subjective technique, known only to Sheldon and God and a few disciples at the present time. We do not like something that we cannot get our hands on. Sheldon has only one approach, and he is not the first nor is he the last. I can show you, when we come to the technique discussions, how, absolutely, to get a quantified arrangement of the first component, and if we have enough time and money . . . we can show you how to get the second component.

I will admit that only Sheldon and God know what the third component means, but that is not going to stop us because we can go on and devise further and more quantifiable measures than have been done at the present time. I agree that it is not going to be susceptible to genetic analysis for a long time. The question arises as to whether we are therefore to put it aside until we get genetic analysis or use it in the meantime.

ACKERKNECHT: If you want to sell something in this country, you must say it is new, and if you have to confess, as I have to confess, that constitution is something very old, then you almost lick the thing from the very beginning. But I must stick to the truth and tell you that it is very old. It is probably the oldest existing medical theory. It goes back as far as any rational medicine goes. It starts with the Hippocratic writers.

Now the Hippocratic writers were not exclusively constitutionalists; they were environmentalists of a sort, too. For instance, about 500 B.C. the author of *Airs, Waters, and Places* explained the character and the phenotype and the diseases of the southern Russians by tight swaddling; nevertheless, the primary outlook of the doctors up to the middle of the nineteenth century was a constitutional one. There was a famous four-constitutional series of Galen which lasted up to that time and which we still use in everyday language. I must confess it is a little annoying to me that we still have a four-class division of constitutions today—I being, to that point, assimilated as an American.

In the middle of the nineteenth century it was realized that the constitutional explanation of disease was a mixture; that there was a certain amount of reality but also a certain amount of ignorance and even of intellectual laziness. If you do not know what a disease is, you say it is hereditary. One of the pillars of hereditary diseases, the itch, proved, after all, not to be constitutional; then came the bacteria. Then happened what so often happens, regrettably, in science as a whole, namely, that one throws the child and the bath water out at the same time. It took a very long time, and it was a historical event which Dr. Caudill alludes to . . . which had a tremendous effect on medicine. It was the great influenza epidemic of 1918. Scientific medicine, found itself completely unable to prevent the death of more people from that epidemic than perished in the first World War. It was this disaster which brought back interest in constitution.

. . . I feel it is much more important

that people carry on these constitutional studies than tear them to pieces because they are wrong in this and that or because they are not very far from Galen. After all, this happens in other fields of medicine. Nobody wants to give up studies in epilepsy just because we do not know much more about it than Hippocrates did. . . .

. . . I think it might not be bad to bring a little historical outlook into this whole discussion. Anthropologists have played a valuable role in taking over what the medical man, at least in this country, did not want to take over but what sooner or later he will have to learn. I think, for a change, that, instead of throwing tomatoes at the people who do these studies, we should give a few bouquets once in a while.

WASHBURN: . . . It seems to me Tanner's inventory paper is far and away the best that we have on constitution in the country. It brings it up to date. He stresses Viola; he stresses the *Zeitschrift für Konstitutionslehre,* which is never, or practically never, referred to in American work on constitution.

I do think, though, I agree with Dr. Boyd. I believe I can illustrate . . . that the method is fundamentally wrong. Now, if we go to Dr. Ackerknecht's point, let us keep it on whether or not it is wrong, because there seems to be something here, clinically. . . . This is where the question of the people doing constitution studies becomes very important. I like to quote from Sheldon's book on the varieties of delinquent youth. He concludes that the delinquent youth are bred down to a shocking level of biological inferiority, like the Arkansas dogs, and that is almost a verbatim quote. He says that, on the basis of having looked at people in the New York subways, two-thirds of them should not be allowed to reproduce. In actual fact, in the United States now, this is the kind of thinking which is kept alive by giving too much tolerance to the subject of constitution. It is a matter of no concern to the United States whether a half-dozen people or so, or thirty or forty, do constitutional studies, if they stick to the academic level, but when, from their writings, comes a new racism—and that is what this is now —we have to be concerned on a very different level.

A student of mine, Florence Simon, quite independently from anything I suggested, made and published in the *American Anthropologist* a study on the articles appearing in physical anthropology in this country, concerned with behavior in any way. Her study showed that, as the number of articles associating behavior with race goes down, the number associating it with constitution goes up. There is almost a precise replacement, and some individuals even shift from the racial category to the constitutional.

If we are to regard this as more than questionable science, I think we have a right to insist that these overtones be removed. So far we have not had any frank statement from the constitutionalists, speaking generally, that they are not happy to have the overtones in. I see no note in Dr. Tanner's paper that he finds anything unscientific in Dr. Sheldon's varieties of delinquent youth, though I hope he may tell us that he finds the vocabulary of the book and its social implications revolting. If so, I would be very glad to agree that he has done a good job of inventory, and I hope that he succeeds in his mathematical analysis; but, if he enjoys the vocabulary and social conclusions, I shall personally classify him along with Dr. Sheldon.

NEWMAN: . . . I simply want to make the comment, which I think is apropos of what Dr. Ackerknecht said, that in the last two years—and this may be a queer coincidence—probably the

most important and stimulating work in human physiology has been exactly what we are talking about now.[1]

Human physiologists, ten years after the anthropologists and perhaps thirty years after the racists, are getting into human constitution. There is a very definite feeling in physiology that there have been too many discrete, minute investigations and that the time has now come for dealing with larger entities within the body. I do not think the physiologists are going to get into racist terms. I do not think that we have to get into racial philosophies when we deal with the components or major subdivisions of the body.

ACKERKNECHT: I share completely, and even multiply, the feelings of Dr. Washburn about the Sheldon terminology and what it stands for, just as I have the same feelings about the not so completely defunct racists whom we unfortunately sometimes encounter even in our time. But I would like to remind you that, when physical anthropologists studied race, there were these fanatics who were racists, but that was no reason for abandoning physical anthropology and races that were a fact. I think the same thing applies to this problem.

NADEL: I feel bewildered by this discussion. As a complete layman in this field, I have found that in some recent books on anthropology and similar social subjects—personality especially—the authors always say somewhere, either in the Preface or in the Appendix, "Of course, there are also constitutional factors," thus making a bow to some mysterious body of knowledge which exists on constitution but which they have been unable to draw upon. I had hoped that at this meeting something would come out of it. Now I am told that you all hope nothing will come out of it, because of the overtones.

This is surely a mystifying situation. Either there is something in it and people want it clarified, or there is not, whatever the overtones are. We had a similar experience shortly after the war, in England, when a great deal of work was done by Burt and his colleagues on the famous "G" factor of Spearman in intelligence. His published studies showed that intelligence was being bred out in England because the lower classes married and had more children than the upper classes. A great many of us were, obviously, upset by the overtones that might be imputed to any such study.

I had occasion recently to consult his study again and check on the statistics, and I must confess that I was, quite against my expectations, struck by the carefulness of the analysis. It was apparent that a great weight of evidence existed for a very complicated procedure into which I will not go, showing that there is such a thing as a hereditary general intelligence, or "G," factor. This factor may well be reduced, provided that certain social conditions remain the same, that is, if a kind of differential breeding rate continues.

Now I am not an expert on it; I merely say that I have read these things and have been disturbed by their implication. But, surely, one must give any such investigation the chance of

[1] I. S. Edelman, J. M. Olney, A. H. James, L. Brooks, and E. D. Moore, "Body Composition: Studies in the Human Being by the Dilution Principle," *Science*, CXV No. 2991 (April 25, 1952), 447–54; J. Brozek and A. Keys, "The Evalution of Leanness-Fatness in Man: Norms and Relationships," *British Journal of Nutrition*, V, No. 2 (1951), 194–206; R. A. McCance and E. M. Widdowson, "A Method of Breaking Down the Body Weights of Living Persons into Terms of Extracellular Fluid, Cell Mass, and Fat, and Some Applications of It to Physiology and Medicine," *Proceedings of the Royal Society, Ser. B, Biological Sciences*, Vol. CXXXVIII, No. 890 (1951).

proceeding further, whatever the unpleasant consequences. I might say that to some extent my misgivings were alleviated by the fact that, while Burt found that intelligence in England was being bred out, Thompson found that intelligence in Scotland was not bred out.

BOYD: . . . I doubt very much if intelligence is being bred out in the world in general. . . . I think what worries us about the Sheldon stuff is not so much the unpleasant overtones as the unreasonable overtones.

MEAD: I would like to speak to Dr. Washburn's point and Dr. Nadel's, that one of the best ways of forcing one part of a science out, so that it feeds into what one regards as unpleasant or undesirable or retrograde aspects of the climate of opinion, is rejection inside the subject. We have had a very good example of that, I think, in the way in which the work of the Gesell group has been treated by the child-development people in this country. They resisted it; then, in order to finance it, Gesell went to the general public. The study was cut off from the rest of the sciences in many cases. There was support from the general public; there was interpretation by the general public of points that were never even meant by Gesell, because they were unchecked inside the discipline. Yet we are suggesting doing something of the same sort here, if we are to take Dr. Washburn's point that it is very serious if constitutional work is to be interpreted as Sheldon has interpreted it—with which I entirely concur. I am inclined to believe that it may be unwise to favor too much constitutional research, because of this climate of opinion in the world. This does mean, however, that we must keep it close to ourselves, inside the discipline, give it a fair hearing, criticize the detailed work on its merits and in detail and not by analogy; otherwise, we will force the constitutional people out and give a real basis for their being used for trends in society that we do not want to follow.

WASHBURN: I think my remark was slightly misunderstood. I am all for people going ahead on this kind of research. I think it is contrary to genetics. I think that they will not get anywhere, and I think if I had time I could convince you of that. It seems to me it is unimportant whether or not they go ahead with the research. What I strenuously object to is referring in scientific publications, even in the inventory, to a book which is written in violently nonscientific, emotional language, without any attempt at qualification. Sheldon's last book on delinquent youth, based on a study in South Boston, for example, refers to President Roosevelt's career as one, from the constitutional point of view, of unparalleled delinquency. . . .

All that I am requesting is that, as the constitutional work proceeds, these overtones, which cannot be proved as right or wrong by anybody, be removed and the work be presented as science, and that references not be given to these vicious documents without statements being made that these are, in fact, not scientific documents. The delinquent youth, for example, or most of the people referred to were never studied by constitutional techniques.

NEWMAN: Much of the constitutional work and much of the growth work . . . fall into the realm of applied physical anthropology. Among the important problems of applied physical anthropology today are some such things as have been brought up by Dr. Shah—the problem of nutritional anthropometry—and, as undoubtedly will be raised by Dr. Monge, the problem of environmental stress.

Constitution and the various aspects

of growth investigation, which seem most applicable to the analysis of constitution, are at the present time two of the most rewarding ways we have of investigating these nutritional and environmental problems on which there is a fantastic amount of pressure and on which there will probably be more money available in the next five years than in any other one subject in physical anthropology.

TANNER: I expected to find one-fifth of my paper discussed for nine-tenths of the time, and so it was. . . . I was particularly careful, in the first two or three pages of my paper, to define exactly what I meant by "constitution," its relation to genetics, and so forth; so that I think no confusion should have arisen on those grounds.

Dr. Murdock said that he thought constitution challenged the conclusions of genetics, learning theory, and psychoanalysis. Obviously, such a statement is quite impossible to answer in a few minutes. I would, however, refer him to a forthcoming volume of discussion: *Prospects in Psychiatric Research,* which resulted from a conference such as this held a few months ago in Oxford. We had professionals concerned with learning theory and with psychoanalysis talking about these subjects, and perhaps it would be interesting to see what they have to say.

As for Dr. Washburn, he said the method was fundamentally wrong. I am sorry to quibble about these things, but I really do not know what method he means, since there were quite a number of methods mentioned. . . . He thinks it is contrary to genetics. That is really a complaint—as so often these things are—of not enough research work having been done. We know very little indeed, as I pointed out previously, about the genetics of human morphology. It is one of my own major interests, and it is a matter of much more difficulty than single-factor work. We have not had the necessary tools. They have come up from animal research to a considerable extent and have been stated largely in terms of biometrical genetics. . . .

It is, of course, entirely untrue that no work at all on the genetics of human morphology has been done. It was started by Karl Pearson. R. A. Fisher made very considerable advances in it in this country, and Alice Brues has published a small amount. But the methods have not, up to the present time, really been available in biometry. I do not think you can talk about this work at all without a familiarity with this sort of biometry—which is really quite complicated. I think the criticism is one of not enough research having been done. . . . Well, let's get on and do that research.

However, the other point was raised by Washburn: Let's not get on and do it. That I would not be expected to agree with, naturally. . . .

I would like, with a friendly feeling, to say that when Dr. Washburn comments, as he did, that if I would pass a series of judgments on a publication which agreed with his judgments on the publication, he would agree that my scientific job had been a good one but otherwise not—I do not know quite what to say to that. All I really would say, I think, is that we are talking outside the area of scientific discourse at this stage. . . .

MURDOCK: As one who has used statistics, I am abundantly aware that the important thing is the theory and that the statistical methods must be associated with good theory or you get nowhere. I am sure you all recall the convincing demonstration by Galton that eminent men inherit between two and three times as much through their paternal relatives as they do through their maternal relatives.

BATES: It seems rather odd, as Dr. Tanner said, that we started on one-

tenth of his paper. The whole section called "Growth" and the longitudinal studies being carried out in this country under medical auspices in places like Denver and the University of Minnesota are, it seems to me, of tremendous interest to anthropologists. Yet because the word "Sheldon" appeared here, we have forgotten what the paper is all about.

PHYSIOLOGICAL EFFECTS OF HIGH ALTITUDE

MONGE (statement revised after session):[2] For some time I have had the opportunity to study men living in low and high altitudes.[3] I shall present, briefly, some biological patterns which I have been able to establish in studying such distinctions. If we examine the blood differences between men at sea-level and high altitude we find them to be as shown in the accompanying tabulation.

Altitude	O_2 Pressure	Hemoglobin (Per Cent)	Total Blood Volume (Liters)	Total Hemoglobin (Gm.)	Total Blood Volume (T B V.) in 100 Gm. Body Wt. (Cc.)	Lung Blood Volume (L V.) (Cc.)	L.V./T.B.V. (Per Cent)
15,000	90	22	7	1,500	100	22	20
Sea-level .	150	15	5	750	71	11	15

From these data we conclude that at higher altitude we have (1) higher "bellows" function of the lung; (2) higher blood volume of the lung; and (3) higher lung blood volume as related to sea-level lung. Interpretation of these points means more work for the heart in general, and particularly for the right side of the heart, as estimated mathematically from the Newman method applied to the determination of lung blood volume. (This work has been carried out, thanks to the Wenner-Gren Foundation, by Carlos Monge, Jr.)

Going on from these conclusions, we can predict that the anthropometric measurement of the thorax must reveal the same relationship. In fact, research carried out with this thesis in mind has proved that function determines the shape of the thorax. On the basis of studies made by Villard, a thoracic volume of 10,100 has been determined for man at sea-level, as compared with one of 12,150 for altitude man, who is, paradoxically, small—reduced to the limit in surface area in order to defend himself from a cold environment.

The ecological milieu in the Andean region varies from sea-level to an altitude of 15,000 feet. At sea-level there is an excess of oxygen in the air; at altitude, a deficit. In simple terms, "altitude man" is one who can stand the altitude; his body might be compared with the working of an airplane in its ability to go up to 18,000 or 19,000 feet and down to sea-level. In aviation we know that, when a pilot gets to 10,000 feet, he has to put on an oxygen mask, for man works best in a horizontal environment where there is an excess of O_2 in the air. Yet in Peru and Bolivia we have observed pilots going up to 20,000 or more feet in altitude without collapse. This is of enormous importance for applied biology in selecting men for different activities.

Nobody can go from sea-level to 15,000 feet without suffering serious

[2] Transposed from fifteenth session.

[3] A fuller statement appears in Dr. Monge's inventory paper, "Biological Basis of Human Behavior," which it was decided to add to the papers for *Anthropology Today: An Encyclopedic Inventory,* during the symposium.

disturbances. On the other hand, if we take Andean man and bring him down to sea-level, we have proved that his internal milieu, expressed by the acid-base equilibrium (that is to say, his Ph), shifts to the acidity zone, so that he cannot live as a normal being. The trouble . . . produces predisposition to respiratory distresses.

We know that when a man goes to a high altitude he has first to be adapted, and adaptation in itself is a malady. When adaptation occurs, acclimatization supervenes. Not all sea-level people can become acclimatized; some men lose their acclimatization; some become infertile. This failure of adaptation or loss of acclimatization I have called "chronic mountain sickness."

With all these facts in mind we can proceed to analyze such phenomena as the behavior of populations, migrations, wars, and internal colonization which, during the time of the Incas, were affected by the knowledge which these people had regarding altitude. . . . Men were trained as athletes for competitive games . . . for long-distance running. . . . The external colonization was built up by means of an organization called *mitimaes,* members of which were sent to places of the same altitude and climate. To make war against lowland people, the Inca had two armies in the altitude, alternately shifted to avoid the stress caused by the superoxygenated environment of sea-level. As a punishment, criminals were often sent to the jungle and low plains.

At present, numbers of migratory laborers go up to the mines or come down to lower levels for agricultural purposes, but they return, eventually, to their original milieu. Even individuals who have left have, like the swallows, a secret urge to return. Biology thus seems the determining influence on the individual and on the sociological behavior of the Andes people.

ROWE: Dr. Monge's studies on the relationship between altitude and physiology are extremely interesting, particularly to me, since I am interested in the cultural history of the Andean area. Here is obviously a case where physiological data are of considerable importance in cultural history and have to be taken into account by anyone working on ethnological or historical problems, to some extent possibly even on archeological problems.

One point which seems to be particularly important from the point of view of a cultural anthropologist is the one relating to ability to reproduce. There I am a little bit troubled by the nature of the case that Dr. Monge has cited, and cited in his book, to suggest that people from sea-level have difficulty reproducing at an altitude. I looked up the reference that he gives to the miracle at Potosí and it appears as he stated it. On the other hand, the miracle context in which it appears is a little bit suspicious. . . . It would be extremely interesting if we could have more cases studied of people who went from sea-level to a very high altitude today, different racial groups, if you will, and find out what the situation is with regard to reproduction, and then go back and recheck the historical evidence.

ANTHROPOLOGY AND MEDICINE

HALLOWELL (statement revised after session): Caudill's discussion of the study of medicine in nonliterate societies suggests opportunities for future investigation. In the past, this area has suffered from "compartmentalization." Theories of disease causation have often been reported as ethnographic fact, without systematic efforts to inquire into all the ramifications of medicine in relation to the culture as a whole and to the psychological adjustment of individuals.

One of the major reasons for envis-

aging the cross-cultural investigation of medicine is suggested by the universal situational imperative stated by Caudill in his paper. "Disease in some form is one of the fundamental vital problems facing every society, and every known society has developed methods for coping with disease and thus created a medicine." If so, we have a universal frame of reference dependent not on cultural content but on common human value: life as against death, health as against illness. This value, in turn, defines a motivational orientation dynamically related to the reduction of normal anxiety as conceived by May, Morirer, *et al.*, i.e., a generic response to threat on the basic level of personality. Fears are responses to threats at a more peripheral level, while disease is a threat to the self and easily becomes ego-involved.

The cultural variables that have emerged in relation to this situational imperative are basically complicated by two major factors: (1) culturally defined concepts of what is considered the normal functioning of a human being as against dysfunctioning, and (2) the sources from which dysfunctioning arise.

There are many complex interrelations here between empirical fact and interpretation, since malfunctioning is related to what are considered to be the indices of it, e.g., symptomatology. Remedial measures may, in turn, mainly be a function of the symptom recognized. This holds for both physical and mental diseases.

A good many years ago, I briefly discussed the so-called "windigo psychosis" of the Cree and Ojibwa. At that time I considered the possibility that there might be some etiological unity in such cases that could be related to known clinical entities in Western culture. I now think that the common symptomatology is the point to be stressed. A person who is considered a windigo defines personal dysfunction in cultural terms. These Indians do not know and cannot know the actual etiology. Their remedies must be related to the symptoms recognized. The *real* psychiatric aspects of these cases cannot have the same meaning for them as for us. My hypothesis is that such cases *may* be more psychotic than neurotic, i.e., *severe* cases of psychological dysfunctioning. Consequently, since no effective therapy is possible in this culture, the killing of such persons, as a last resort, is a practical alternative that eliminates them from the community. And the guilt ordinarily associated with overt aggression is relieved by the cultural definition of a windigo as a dangerous cannibal. The killing is justified as a measure taken in self-defense.

In other cultures, as in the psychotic case reported by M. E. Spiro,[4] dysfunction may be recognized, but the symptomatology may not suggest any danger to others and the individual is tolerated in the community.

The causal categories of disease take us into the world view of a people and the metaphysical assumptions of the culture. Among the Ojibwa and in many other cases, *impersonal* causation may not be recognized at all or be reduced to a minor cause; consequently disease is a form of personal aggression.

One of the cognitive functions of the socialization process, therefore, must be to acquaint the individual with those aspects of his world that involve threats to the self, together with the resources available for meeting this danger. And here *materia medica* may not be clearly distinguishable, linguistically or otherwise, from *materia magica.* It is not without significance that the five basic world-wide concepts of disease traced out by Clements

[4] M. E. Spiro, "A Psychotic Personality in the South Seas," *Psychiatry,* XIII (1950), 189–204.

(sorcery, object intrusion, soul loss, breach of taboo, spirit intrusion) all involve more or less direct threats to the self. If, as a number of psychologists have recently pointed out, one of the positive conditions of a level of adequate psychological functioning is a response to the self as a primary object of value, medicine in its universal aspects may be seen to have a function that transcends the physical benefits that are so frequently emphasized in our culture. Medicine as an institution is a means of enhancing *every* aspect of individual security—psychological, physical, social. But such a function can have meaning for the human individual only in terms of the total system of values, goals, and motivations that are defined by a given social-cultural system.

Dr. Caudill goes on to raise the question whether we cannot refer to a psychological locus of culture that includes the unconscious as well as the conscious aspects of personality.

I do not believe there can be any doubt about this. Certain aspects of culture, at least, necessarily become involved with unconscious processes, if only for the reason that the psychodynamics of personality development and functioning are inextricably connected with attitudes, meanings, and values which, from the start, become an integral part of the psychological economy of the individual through unconscious identification with parents or other cultural surrogates.

One of the most direct sources of evidence and one that demonstrates the functioning of culturally derived values in the individual at an unconscious level is dreams. Dream imagery is, of course, culture-laden in its manifest content, but it is the manner in which dreams may function in relation both to the individual *and* to his culture that is significant.

We have evidence from some cultures, for instance, that self-related impulses and acts that occur in dreams may be evaluated by the same standards that are applied to such acts performed in waking life. Devereux has recently cited a most interesting example of this. A priest in Papua, with whom he has had correspondence, informed him that "converted natives . . . who, intellectually at least, have been sufficiently well indoctrinated to know that they are not 'morally' responsible for the content of their dreams, frequently confess that they committed adultery, and specify only in reply to direct questions that the adultery occurred merely in dream."[5] In other words, the individual becomes socialized in a manner that integrates unconsciously produced dream imagery, taken at its face value, with overt, conscious, and volitional acts. No hard-and-fast line is drawn between them so far as moral responsibility is concerned. One is responsible for all one's "acts," waking or sleeping.

On the other hand, we have the more familiar example of tabooed impulses that may be completely disguised in dreams. There may be intercourse with a woman who does not appear as a mother or sister in the manifest content of the recalled dream, but who may, in fact, symbolize individuals in the tabooed category if the dream were probed for its *latent* meaning. But the dreamer, not knowing who is represented, need feel no conscious guilt. How are we to understand this except on the hypothesis that the values integrated with culturally constituted taboos function in terms of unconscious mechanisms. If an animistic metaphor may be permitted, it is as if the "unconscious" "knew" the culture and

[5] George Devereux, *Reality and Dream: Psychotherapy of a Plains Indian* (New York: International Universities Press, 1951), p. 86, n. 4.

"substituted" a sexual object of the proper social category.

Furthermore, forbidden conscious impulses, such as desire for a sister, may be unconsciously rationalized in a manner that conforms with wider cultural values than those associated with specifically tabooed impulses. An Ojibwa Indian of the nineteenth century married his sister, and she bore him several children. At the same time he was motivated to palliate his guilt and the social disapprobation he suffered. He justified his conduct by reporting that in a dream he had been commanded by the supernaturals to marry his sister. Since disobedience of supernatural commands brings personal disaster, this was a reputable sanction of the highest rank for breaking the incest taboo. It was a perfect solution, and I have no doubt that he actually had the dream. But, on the other hand, the kind of dream he had involved the specific world view and high-level values of his culture. Unconscious processes, as represented by the dream, served a double purpose. They enabled this man to violate the mores and to tolerate a burden of guilt while, at the same time, they supplied a sanction from a traditionally recognized source for his deviant conduct.

Unconscious mechanisms and processes in man appear to have a kind of homeostatic function in the psychological economy of the individual. But they could not operate in this way if cultural norms and values were not integrated with the personality structure as a psychobiological whole. There is also a corollary. Unconscious processes may, I think, be said to have a social function in so far as they likewise help to mediate individual life-adjustment in a manner that is consonant with the maintenance of the central values of a sociocultural system.

ACKERKNECHT: I cannot resist my interest in detail: Actual cases of a related psychosis, as described by Diamond Jenness among the Carrier Indians, lend great probability not only to the existence of windigo but of a diffused pattern of a similar reaction in that region. . . . I would like to speak about the study of the social aspects of our own medicine by anthropologists. . . . The work of the social scientists in medicine is most welcome and will help us to learn more about the social definition of disease, not only in the sense that causation of disease is to a large extent social, but also that permission to be or not to be sick is granted by society. . . .

The hospital is a most interesting social formation in our society. Somebody has called it the "factory of health," and we are told that there is less social harmony in hospitals even than in factories. . . . Studies of the social aspects of doctors, nurses, and hospitals will be of great practical as well as theoretical value because they increase our social self-consciousness, which is necessary for a conventionally rational society to function.

In connection with the field of psychosomatic medicine, I was happy that Dr. Caudill avoided a dogmatic statement on the increase of chronic disease and left open the question of whether it is merely a statistical increase or an actual increase partly due to our changing social conditions. I would like to mention . . . that one of the seventeenth-century psychosomatic doctors, Sydenham, stated that 70 per cent of his patients were, as he called them, "hysterics," which we would today call "neurotic"—a percentage which even we would have a hard time to beat. On the other hand, Freud once remarked that perhaps the increase of neurotics in our society has something to do with the disappearance of monasteries. . . .

Now there is only a very vague border line between straight mental

and psychosomatic disease. . . . Psychiatry is, perhaps, our most fascinating facet of medicine, but it is also the most confused and the least scientific one. [I think it worth while to caution anthropologists who accept psychiatric theories because they work therapeutically that the history of medicine contains many such theories, from the four humors to homeopathy, which have not stood up against time.] . . .

MEAD: The work in modern society that Caudill describes has been done under the heading of mental health. This field is young—virtually a post-World War II development—so that anthropology [could enter it from the start] and is a field of interdisciplinary research which is oriented around preventive medicine rather than the old type of therapeutic medicine. It is a good area for co-operation—one that has good ties to the World Federation of Mental Health—an area of research for which there is a good deal of financial support available.

The other problem that Dr. Caudill raises, which is important at the primitive and ethnological level (and we will not be able to get information out of already accumulated material), is the one raised by all the psychiatrists as to whether there is some kind of deeper layer in man that is unaffected by culture. There is supposed to be something going on inside, sometimes called the "unconscious" or the "deeper impulses," that is equated by psychiatry everywhere with a human nature invariant, unaffected by culture. We probably can solve this important problem far more quickly with primitive than with civilized material.

SANGVICHIEN: I do not think that studies of hospitals and of hospital people such as those referred to by Dr. Caudill are applied anthropology or belong to medicine. These are special kinds of studies, but they will not contribute to raising the standard of medical practice.

MÉTRAUX: Research in primitive medicine is important for projects in fundamental education and the different organizations working in underdeveloped countries, because a medical program appeals to the native more than anything else. However, a doctor sent by World Health Organization or another agency immediately clashes with local medicine; thus it is extremely important that he be acquainted with the pattern of primitive medicine in the region where he works.

NADEL: In reference to Dr. Hallowell's statement about basic cultural adjustment to disease, I think you will agree that in our own society the values are mixed up a bit, and there is some desirability, some value, which attaches to none too unpleasant kinds of disease, thereby giving certain satisfactions. I do not know of any similar case in really primitive cultures, but in the cultures pervaded by Islam you get a very definite change in the value assessment of disease as against health, and certain types of debility in serious disease imply a positive rather than a negative value.

In every society another general value system exists, the scale between good and evil, and in most primitive societies one or the other variety of disease will be taken as an index, symptom, or consequence of evil behavior—guilt or sin. How that works out in relation to the actual diseases prevalent in the area, I do not know. There seems to be no rule except one—that a very common disease never appears as one that operates with a supernatural sanction. The common cold, for example, is nowhere a supernatural punishment, and malaria never is in malaria areas. However, in the Sudan two diseases are typical supernatural sanctions: leprosy, which is prevalent

but not so prevalent as malaria, and spinal meningitis. Yellow fever, a "new" disease (not recorded during the twenty-five years of the medical service's active work), was immediately interpreted as a supernatural sanction. When doctors come into conflict with the indigenous experts on medicine, it is very often not a question of competition; on the contrary, you can invariably exchange your medicines for secrets of indigenous medicine. But there is a much deeper resistance by a moral system threatened by the fact that, with new drugs, a supernatural punishment can be made to disappear overnight.

FEJOS: I found that along the upper Amazon malaria was taken as magic and not as a disease. In fact, there is no concept of disease, and all deaths (except those due to accidents like drowning) are magical.

The native shaman or doctor will exchange information and even submit to treatment, provided that the treatment somehow gives an immediate and dramatic effect. This is an experience known also from Indonesia and New Guinea. I cannot imagine any better method of getting rapport with natives than to stop the toothache of one person in a community. It has an almost magical efficacy and, in my experience, does not get the enmity of the native medicine man or shaman. He will feel that the method one used was different from his and not in direct competition.

NADEL: A correction. I did not mean that it was not interpreted as magic; I merely meant not as a supernatural sanction, as a punishment for sin, guilt, crime, and so forth.

MAJUMDAR: I have been interested in a Mongoloid people in Uttar Pradesh, living in Terai, who are supposed to be immune from malaria. People who go there get malignant malaria, with the result that the government has to send officers every fifteen days and give them three months recuperation afterward. Later I found that, instead of dying from malaria, the inhabitants were increasing in number. We found, by a study of the blood-group genes, that there is a high incidence of B blood type in that particular area and that in the neighboring areas, which are not affected by malaria, there was a completely different blood picture. I also found that those people who suffered from malaria generally were not B but either A or O. But those who did not suffer from malaria were mostly B. . . . I do not know whether this blood-group incidence has been due to selective effect of the environment, in that A people and O people die out because of malaria, leaving the B people to multiply.

MONGE: I think it is a well-known fact that in some tropical countries people become more or less adapted to malaria and develop physical resistance: low blood pressure, low frequency of the heart beat, some enlargement of the thorax.

To conclude, I want to say that, in adaptation to high altitude, people can develop mental symptoms. Some of them are treated by psychiatrists, but the better thing is to send them to the lowlands. . . .

SANGVICHIEN: A colleague of mine worked in a Navy camp in Thailand which had a high incidence of malaria. He divided the Navy men into two groups, giving quinine or atabrine to one and a noneffective imitation drug to the other. He found that 30 or 40 per cent of the people in the second group, after six or seven days, did not have any more fever. Further, there is some immunity in the malaria district, especially in some seasons, and also malaria permits a certain immunity among the people who live in the malaria district.

MEAD: I would like to ask Dr. Held

if he can throw any light on the Caucasians and Chinese who have lived for a long time in Indonesia sometimes running amok. It has often been said that long residence in Indonesia means that they take on the exact pattern of the particular type of psychotic behavior that we associate with that part of the world.

HELD: I cannot make any definitive statement about the occurrence of the so-called *tropenkoller,* if that is what you mean.

I can hardly believe that *tropenkoller* is exactly the same thing as amok. Amok is believed by some medical people to be an organic disease, a phenomenon of malaria. *Tropenkoller* is a general neurotic situation resulting from living in isolation, and it is certainly related to the white personality type. I am struck by the clues which disease and medicine give you to personality types. There can be two tribes, one beside the other, in one of which people believe that disease is everything caused by black magic, and in the other people consider as disease only those things which lead to death.

LOWIE: Is there not a term *latah* employed in that area? What does that mean?

HELD: I do not know. I would call that a neurotic situation also, in which a person immediately imitates everything he sees done by somebody else.

PAUL: We know that the world view of people, particularly their beliefs about disease causation, is very important in their acceptance of, say, a public health program. But it is very hard to predict what parts of the cultural beliefs will become involved when a particular program is prosecuted. For example:

Among the highland Indians of Guatemala, there is a widespread belief that blood has various magical characteristics having to do with ill health and good health. Furthermore, in their projective system there is what might be called a "cannibalistic theme," whereby under certain circumstances monsters, other people, or foreigners will consume the local people. Now what will happen when a nutritional program goes out to these people and periodically weighs, measures, and takes blood samples from the school children? This is being tried now in one village, and they ran into great trouble. The Indians felt that the extraction of blood was to ascertain whether the children were now ready to be shipped to the United States to be eaten.

However, in four other villages that have the same belief, there was no objection to the program. The question is, why in one case and not in the other four? In the first village, unknown to the people prosecuting the program, a schism had occurred prior to their arrival, and this tension then mobilized the cultural beliefs. My point is that the concept of culture is incredibly important but is insufficient as a basis for anticipating action. It has to be [related to the entire situation], and only by the specific kinds of observation of attempted action are we going to get refinement in cultural process and theory.

FEJOS: May I just answer the questions about amok and *latah?* According to de Langen of Batavia, *koro, latah,* and amok are all three purely ethnic diseases, and he doubts the existence of them among the white people. Further, *koro* is present only in Chinese inhabitants of the former Netherlands East Indies who were immigrants and never occurs in an Indonesian, whereas *latah* never occurs among the Chinese but only in the born Indonesians.

CAUDILL: Dr. Hallowell mentioned that the society knows only the symptoms and not the etiology of psychotic-neurotic disturbances in a culture and hence can treat them only in terms of the symptomatology. I think this is true

at the conscious-rational level, but probably a great deal goes on therapeutically at unconscious and unverbalized levels between people; the society is treating people in ways that it does not even know it is treating them. This also was, I think, a problem for study. . . .

Dr. Nadel mentioned the idea of health and disease in all societies. I sometimes think that health is at a different level of abstraction from disease. All societies have disease, but not necessarily the notion of health. This is quite an American notion.

In my paper there is the assumption that one needs to study detailed interactive processes over time. Dr. Chapple defined applied anthropology as being the study of detailed interaction over time. This is a peculiar and somewhat technical use of the term "applied." It has a more common-sense notion of going in and doing something in a situation. . . . I have viewed the particular hospital studies I have been doing not only in the common-sense notion of applied but also in the wider theoretical sense. Much culture-personality work has had to rely for its personality data on projective techniques brought back and interpreted largely by psychologists or others who have no interest in, or knowledge about, the culture. A mental hospital can be thought of as a small society which is dedicated to the proposition of working up psychodynamic material on one level of its members, of patients. If you get the social role and societal data, you have a chance of putting these three things together in the controlled situation, with the possibility of coming out with some propositions of much wider theoretical interest than just a hospital study.

CHAPTER XI

Problems of Application: Results

Chairmen: Margaret Mead and A. J. Dias.

Papers discussed: Held, "Applied Anthropology in Government: The Netherlands";* Forde, "Applied Anthropology in Government: British Africa";* Kennard and Macgregor, "Applied Anthropology in Government: United States";* Métraux, "Applied Anthropology in Government: United Nations";* Redfield, "Relations of Anthropology to the Social Sciences and to the Humanities"; Chapple, "Applied Anthropology in Industry";* Haas, "The Application of Linguistics to Language Teaching."*

Speakers: Nadel,† Forde, Mandelbaum, Ackerknecht, Shah, Caso, Kennard, Macgregor, Redfield,† Lévi-Strauss, Spoehr, Sangvichien, Eiseley, Namba, Majumdar, Canals Frau, Métraux, Lindblom, Chapple, Eggan, Kluckhohn, Mead, Steward, Lowie, Held, Beals, Hoijer, Voegelin, Lounsbury, Greenberg, Haas, Rowe, Brew, Thompson.

ANTHROPOLOGY IN GOVERNMENT

NADEL: I am going to talk about the applied anthropologist as an employed anthropologist, working as instructed or requested, studying a particular range of problems, so that his employer can then utilize his knowledge and, in a different sense, apply it to practical problems. The employed anthropologist need not work for a salary; he may get a subsidy from the employer or feel a moral obligation toward an employer. The employer is the government—more specifically, the colonial government in the British Commonwealth, mainly in Africa, with some reference to Australia and New Guinea. Finally, I shall talk about the results—Where do we now stand in this kind of research?

As regards the usefulness of anthropology to the employer, I have little to add to the background papers and will mainly express some misgivings which have arisen in the course of my own experience with work in applied anthropology.

One type of applied anthropology revolves upon broad surveys of a region or tribe or group of tribes. It seems an ideal way of doing research; the anthropologist is quite free to do what he likes or dislikes. But only part of our analysis is likely to be used, and quite often we put our hearts and energies into this kind of work, only to realize that the government is going to use only the sort of thing that other people, with "local knowledge," could also provide. I do not mean only that the anthropologist feels he could contribute much more but that this is, in a sense, blind research; neither the employer nor the anthropologist knows at the time what it is going to be used for or what it will eventually be needed for. Thus either the anthropologist will overlook important things, since his attention has not been directed upon them, or his employer, unaided by a

pointing-up of the problem, will not be able to utilize such information when it is produced. Specific formulation of problems and specific presentation of material oriented toward the problems are much more useful for the employer than a broad survey. Further, the anthropologist cannot produce at a given moment data automatically useful to the government, whatever its plans or problems. It is necessary for the two to collaborate over a long period, so that the anthropologist may see what use is being made of his data. He can then correct them, taking into account derivative changes in the situation.

Certain misgivings also arise in connection with specifically oriented studies, where the handling of the practical issues remains entirely in the hands of the government, of the men "with common sense.". . . I have always mistrusted the division between anthropological knowledge or insight and common sense. If the anthropologist knows enough about societies to be asked, he is also a judge of how and what to plan for these societies. Admittedly, this knowledge derives not solely from the anthropologist's scientific background but also from viewpoints and experiences acquired from his interest in general and social problems and his inheritance of a particular tradition of thought. However, although the requirements of the practical situation are the decisive criteria in any planned change a government may conceive, being an anthropologist does not disqualify one from handling the criteria of expediency and practical needs or deprive one of a sufficient sense of realism to appreciate them.

The point has been made that the anthropologist should not be an administrator or that he would make a bad one. I think a good anthropologist would be a very good administrator, at least as good as anybody else. The employer should be made aware that the anthropologist has the qualifications to make definite proposals and suggestions.

With regard to the effect of anthropology on planning, policy-making, and so forth, there is now considerable dependence on anthropological work on the part of colonial governments, but by no means an exclusive or indispensable one. . . . It is only the minority of administrators who really realize that the kind of information they get from anthropologists is different from that obtained from others and who think in terms of future consequences, which would show the real difference between the two types of information.

Now, regarding the usefulness of applied anthropological research to anthropology, we have acquired a large body of knowledge and developed a considerable number of techniques in the study of particular problems which occur repeatedly in the sort of study we are asked to undertake. We have a body of knowledge which enables us, when coming across a certain problematic situation, to diagnose it immediately and make fairly reliable prognostications. We have been dealing with quasi-experimental situations, and it has been possible to verify some general hypotheses which anthropologists have made and expressed in theoretical writings. At the moment, some of my students are carrying out the study of the Cargo cult, a very typical nativistic cult on the east coast of New Guinea, which will enable us to test some of Linton's hypotheses on nativistic cults. . . .

FORDE: I do not feel so badly about blind research as does Nadel. First, one starts almost completely ignorant of social structure or cultural values. Therefore, the comprehensive study, using standardized anthropological field techniques, gives one a scope of results which are likely to be valuable over a longer period than research di-

rected to a specific problem. Second, many great advances in the physical sciences have resulted from blind research, the practical application of results having been seen only afterward.

Concerning the status of the anthropologist with regard to administration, the point on which Dr. Nadel and I would tend to diverge, relates to our knowledge of administrative policy and of the social processes by which it is formulated. Concrete advice as to how the government ought to handle a problem may be advice given in very considerable ignorance of the workings of the administration and of the pressures with which it is involved. Unless the anthropologist has been allowed, as it were, to do field work on the administrative system, I do not think he is capable of giving more than an analysis of the consequences of a series of alternative measures.

MANDELBAUM: . . . . I wonder if Dr. Nadel does not overestimate local knowledge. I wonder how frequently you can get local nonanthropological knowledge from a planter or an educated member of the society you are studying, who, by reason of his very status position, does not have some very strong biases which militate against the usefulness of the knowledge he provides.

ACKERKNECHT: I would like to contribute a quotation from Pasteur, who says there is no such thing as applied science; there is only science and its applications. Second, I found it very fine that Dr. Nadel brought in that long-neglected child of the human mind—religion—in speaking of nativistic cults. When anthropology is applied to primitives, that is usually duly regarded, but not so when it is applied to modern society. I feel that, in applying anthropology to a field like health, it would be extremely useful to look at certain so-called health attitudes as convergents of religious tendencies. The same holds good for certain national character studies, where only a look at the religious or nonreligious attitudes of such people as the Germans would provide some practical clues.

SHAH (statement revised after session): In India there is a tendency to associate anthropologists with government departments dealing with the problems of primitive tribes. These tribes and other social castes were intrusted to the care of the president under the republican constitution of the new India and a special department was constituted for this purpose. An advisory council was appointed to which anthropologists from all over the country were invited. A knowledge of anthropology was considered of special value in officers selected to supervise the development of the backward tracts. . . . An attempt is being made to make these tribes useful and efficient members of the country—a great experiment in cultural dynamics. It affords unusual opportunities for study, and it is possible that at the end of ten more years there may be no primitive tribes living in isolation in India.

CASO (Métraux translating): In Mexico, in fulfilment of the functions of the Instituto Indigenista, anthropologists engage in a threefold activity: (1) they do regional studies, investigating conditions of the native population; (2) they present to the government advice on measures to improve the conditions of the Indians; and (3) they themselves apply these measures and intervene in their application by others. Thus the anthropologist is at once an investigator, adviser, and administrator. As an administrator, he has the help of technical assistants in the different fields of health, agronomy, and so on. This is the line actually followed by the Mexican government.

KENNARD: The common assumption that administrative objectives are long-term [is frequently belied by the fact

that staff members whose appointments are relatively short-term, feel that, to "make a record," they must accomplish something new.] This results in terrific discontinuities in the administrative process. . . .

MACGREGOR: On the point of broad surveys, I think them necessary if they are considered as a base for future studies and for understanding problems of the administration. In programs concerned with technical assistance to another culture, they are very necessary for determining the kind of assistance and evaluating the change which results.

The difficulty of communicating anthropological data and concepts to the administrator [raises a series of problems which can be overcome if the anthropologist becomes a member of the planning group, which I believe he can do and still retain his objectivity. On the other hand, if he enters administration proper, he is no longer an objective anthropologist because he becomes too clearly connected with the tensions and complexities of administration and thus loses much of his value.]

REDFIELD: We see two kinds of doubts expressed concerning applied anthropology in government. There is Evans-Pritchard's view that the anthropologist who investigates practical problems is not engaged in science but in "the nonscientific field of administration." Apparently, however, the weight of opinion is that scientific value is achieved by studying practical problems. We have also heard a wide range of advice as to the particular form of connection. . . . But these are details, and it seems more important to consider here the other source of doubt.

Dr. Held judges it unwise for the anthropologist to identify himself with government administration, but his reason is not that which Evans-Pritchard gave, which was that not enough science would get done, but that the anthropologist's evaluation of native life and of what ought to be done about it may differ from that which prevails among government officers; in short, that there is likely to be an embarrassing conflict of values. . . . It is not a mere formal rule that the anthropologist may feel called upon to criticize; it is the value called into question by the way in which the anthropologist's special knowledge is put into effect. This returns us to the ethical position of the anthropologist. Forde quotes with approval the words of G. Wilson that "the social anthropologist cannot judge of good and evil; the conception of technical information is for him the key to the correct relationship between social scientists on the one hand and the man of affairs on the other," I believe that this is the standard, usual, and correct position. However, American anthropologists, in so far as they are organized in the Society for Applied Anthropology, have adopted a code of ethics which says that the applied anthropologist must never maintain "that he is merely a technician unconcerned with the ends toward which his scientific skills are directed."[1] Perhaps it is only Americans who have difficulty. It may be that in England, where the differences between government and opposition in foreign policy are no greater than the differences between Mr. Churchill's and Mr. Attlee's colonial policies, the position of purely technical service is held without difficulty. . . .

These papers indicate that a difference in value judgment between anthropologist and government administrator is likely to be real and serious, no matter how enlightened the government, because of something in the very nature of anthropology. Dr. Held refers to this when he speaks of the tendency

[1] "Report of the Committee on Ethics," *Human Organization*, VIII, No. 2 (spring, 1949), 20.

of Dutch anthropologists to value positively the static nature of Indonesian native life and to deplore loss in native culture. American anthropologists have themselves declared, in an officially published pronouncement, the right of all peoples to live under the cultures that are already theirs. This is to set up at least one universal or possibly absolute value, that of cultural integrity. Anthropologists on the whole do value positively the cultures of other people and probably integrated culture more highly than disintegrated culture. This tends to align American anthropologists with the Indians as against the administrator, who, in America, generally represents a system which values native culture low and Americanization high. In contrast, Dr. Held notes that in Indonesia, where native societies have taken charge of their own political affairs and adopted values which put change and modernization high and primitive or aboriginal conditions low, the conflict in values, real or apparent, is between native and anthropologist. Dutch, like American, anthropologists value the native status quo, but the native leaders do not want the status quo.

It seems, then, that the relation between the anthropologist and the rest of his community is one of competing values, and he does not merely provide technical useful information. With regard to England, I have a suspicion that, on the whole, British colonial policy is like anthropology there, in that it tends also to favor the cultural status quo.

NADEL: . . . Value judgments are inseparable from an investigation. The idea that anthropologists can study a society from a completely value-free point of view is a complete illusion. Anthropologists are not clear about this point, but the very words that Dr. Redfield used—the value that we attach to integration as against disintegration—is a case in point. We think of society in terms of some kind of valued state toward which it must strive or which it at the moment exemplifies; and thus the distinction between the anthropologist as an objective, so-called "student" and the somebody else who puts things into practice is a fallacious one.

DIAS: In connection with the question of values, many people in the world do not always accept all the values from technical civilization. . . .

LÉVI-STRAUSS: A rather current point of view in France is that modern civilization constitutes what might be called a secondary progress, that is, a way to ward off the evils resulting from increasing population pressure. There are in the world today three hypothetical situations: one where population pressure does not make itself felt, one where there is a high population pressure without the remedies resulting from modern civilization, and one where we have the population pressure and modern civilization. . . . The worst thing which can befall a modern society is to have population pressure and no remedies to protect itself from it. Our feeling in France would be that modern civilization is not in itself a good but is the remedy for a disease.

SPOEHR: Apropos of Dr. Redfield's paper, a problem which American administration in Micronesia has faced is that the administration thinks anthropology and progress are fine, but the lack of a definite value system precludes a consistent picture, and complications are even greater.

SANGVICHIEN: Social anthropology [evidently] becomes needful when white people make a colony of a nation or have a minority problem, as with the American Indian, or with industrialization. . . . But Siam has long been independent and has no minority problems [because the government recognizes the cultural rights of all groups]. The Thai government, perhaps because of its Buddhist religion, does not try

to suppress the life-ways or habits of the other groups. [In the Moslem areas administrators are required to be familiar with the local language and with the Koran. Disputes are settled by native Koranic law. The Thai people are very adaptable and are therefore making an easy transition to modern technological civilization. Many Western-educated Siamese come back and want to change Thai culture to an American pattern.] I wish you would point out to them that Western civilization may be useful to only a few and not to all of the Thai people. . . .

EISELEY: . . . The applied anthropologist, in many instances, is interested in introducing medical features which will reduce mortality but, at the same time, argues for the continuation of the more static aspects of culture which often involve the perpetuation of a large-family system. I would point out that, in terms of population problems in the world, we are assuming a rather paradoxical standpoint.

NAMBA: I want to talk about value orientation and technological development. The pattern of technological development of Western nations might be characterized as subjugation-oriented, while that of Japanese technological development might be said to be adaptation-oriented. To the Japanese, nature is man's co-operator, friend, and even co-creator. The supreme value is not to subjugate nature to man's purpose, but to establish harmony between them. [Although population pressure and shortage of raw materials are important factors, one must also consider, in connection with Japanese technological development, this value of the Japanese people.]

MANDELBAUM: Dr. Redfield points out very correctly that the anthropologist, perhaps because he is concerned with culture, which is an enduring, persistent tradition carried by adults who cannot divest themselves of patterns and values very quickly, tends to emphasize the persistence, the continuing aspect of the people's life, rather than the sharp sudden breaks and changes. What are the implications—is this something to be acknowledged, regretted, taken into account, deplored, or what?

REDFIELD: To be taken into account.

MAJUMDAR: I agree with Professor Nadel that the role of the anthropologist involves the collection of data, broad surveys, and also giving advice to the administration and, if necessary, handling the materials. The anthropologists in India are in a difficult position with regard to the last. Anthropological knowledge is welcomed by the administration, but the anthropologist is not. In this peculiar situation it is necessary for the anthropologist to diffuse knowledge rather than to do anything of a practical nature. However, in recent years his advice, if not his direct intervention, has been actively utilized. [For example, two or three communities in the United Provinces, deprived of their salt-making economic base by mass production, turned to petty theft. Anthropologists hired by the government discovered that their demoralization really came of their having been lowered in caste from Rajput to untouchables by becoming salt-makers. When physical measurements showed them to be Rajputs and they were restored to census recognition as Rajputs, the problem eased.

[In India, biologists describe population trends and processes in terms of the logistic curve of population growth. Some groups are decreasing in size, some leveling off, and some increasing. The latter are the "have nots," and thus the problem is one not only for the anthropologist but also for the economist.

[It is difficult to study values in India now because India is in a state of flux and because, after long suppression, the diversity within Indian value sys-

tems must be allowed to crystallize.] . . .

CANALS FRAU (Métraux translating): Applied anthropology in the Argentine is still in its infancy because of the political-social situation of the Argentine. Argentine is a homogeneous country—there is no Negro population, the Indians are rapidly disappearing, and very few are left in the Chaco. Thus the Instituto Interamericano Indigenista has no corresponding institute in the Argentine. There is only a Bureau for Indian Affairs that cares for the Indians' welfare. Recently, the rapid industrialization of Argentina has awakened some interest in what might be called applied anthropology, because, in spite of the general homogeneity of the country, the regions along the Atlantic are white and have more European characteristics, while in the interior you still have a population which is mixed with the native Indian population. That factories have been erected along the seashore has caused many elements to move to the Atlantic seaboard, so that the population from the interior has undergone some changes, the value aspect of which the National Ethnic Institute has for several years been engaged in investigating through studies of physical anthropology, especially of the height of this population, and also studies of social psychology, in which Rorschach tests were used.

MÉTRAUX: Whether or not the anthropologist should be an administrator depends on the field methods used by this anthropologist. For full participation in a native culture, you cannot be an administrator . . . because all the people whom you visit want this or that and, as an administrator, one has to refuse. Further, being at the same time an anthropologist and administrator is unfair to the administrators who come after one, because the natives complain, "The first man who came didn't do as much for us as the one who came later, but he was much more friendly and approved of what we do.". . .

LINDBLOM: I want to say that I miss a paper on applied anthropology in commerce. For instance, if you export goods to a foreign country, the color of the wrapping is very important, since some colors are considered lucky and some unlucky. You may improve your sales in Asia if you put on your label a symbol of luck.

ANTHROPOLOGY IN INDUSTRY

SPOEHR: Chapple's paper makes the point that, through the sort of applied anthropology that he favors, theoretical contributions can be made. I would add that the closest relationship is to the study of social organization. He has hit upon crucial types of situations, namely, the study through time if specific situations that can be observed by a single investigator, emphasizing the actual behavior patterns of people in relation to one another.

FORDE: A point worth mentioning is the new interest, in England, in studying a neighborhood—including the industrial productivity. However, the English are a commuting population in urban areas, which introduces a factor of complexity probably not yet fully tackled.

SPOEHR: A stumbling block in applied anthropology is the problem of the communication of special knowledge to the administration. I should like to ask Dr. Chapple how the problem of communication in the two contexts, the governmental and the industrial, compares, and what the prognosis is for the successful communication of anthropological knowledge to an administrative organization?

CHAPPLE: I do not find any difficulty in communication because I have endeavored to do field work among the administrators and translate anthro-

pological principles into administrative language. My approach is to study a social organization—factory, governmental unit, or whatever it is. This differs from the position of the British anthropologists, who are looking at something which is outside, even though, so to speak, at the bottom of their system. It is much easier for people doing applied anthropology in government and industry to acquire an understanding of the kinds of things [their subjects] are talking about and establish the reference of their symbols—their logics—and then relate the two.

NADEL: I think it is extremely important that we understand that, when we do any applied anthropology in colonial areas, we must include the government machine in our scope of investigation. . . . It is not enough to study only its symbol system and language, but we must also study the ideas and the motivations which lie behind the administration. That kind of study involves very high specialization, often making the anthropologist become a political scientist or a colonial historian. . . .

EGGAN: It seems to me that American industry has a very strongly defined value system and that it goes off in one direction. Every now and then applied anthropologists or psychiatrists put in the human equation, the social structure, and are surprised to see that it makes a significant difference. But then it does not seem to "take," except in a few key places. Why does American industry not take advantage of all this knowledge—what are the value factors that keep them going on in pretty much the same old way?

CHAPPLE: The difficulty is that the anthropologist has been too much concerned with the "process of conversion." He has been trying to bring about social change by finding a person who understands culture, human problems, etc., or someone who is vulnerable and can be worked on by a series of techniques which involve the social-psychological, group-dynamics sort of thing. . . . As long as conversion is the prerequisite, anthropology is not going to get very far. . . .

People are interested in what the great thinkers of anthropology, psychiatry, and psychology say. Most of the work that has been done has resulted from the fact that some important person who had particular interests in this general kind of human relations approach was willing to allow an anthropologist to do something. But if we are going to limit ourselves to that, I think we might as well go out of business.

KLUCKHOHN: I would like to put to Dr. Chapple two points. The first . . . is the interesting question as to why unions have, in general, been resistive to the Roethlisberger, Chapple, Burleigh Gardner kind of work. . . . Second, in the glossary of the book by Chapple and Coon, *Principles of Anthropology,* there is no reference to the word "culture." I was interested to discover that in this latest paper culture somehow has come back a little bit.

CHAPPLE: The approach we take, which is, since we must preserve these intellectual differences, not to be classified with the Roethlisberger-Mayo approach, is, for the lack of a better word, the cultural approach. On the other hand, the elimination of the term "culture" was done deliberately, not because we were not very much concerned with the problem, but because we were fed up with the word and thought we would try to describe what culture was about without using that catchall symbol.

The union movement, with two or three exceptions, is a relatively new kind of thing, and combat with management is still the basic concern. They are not allergic to anthropology . . . but the research staffs do not have the

opportunity to do anything other than supply grist for the negotiating table, and, in the few exceptions, the emphasis still is pretty much in terms of something which is destructive to management's position.

MEAD: But would you not also say that the very fact that the unions have this investment in combat means that their commitment to studies of the whole system is automatically less than that of management, who, on the whole, would like the system to work and who regard all the combat as a nuisance?

CHAPPLE: Also you must study the whole system, and if you start with the union, it will probably want you to go only so far. Management cannot help going the whole hog because the union is their primary preoccupation, whereas the union is looking at it from the point of view of the bargaining table or the problem, say, of better training for shop stewards. . . .

FORDE: I regret Dr. Chapple's reply to Professor Kluckhohn. I took the analysis contained in *Principles of Anthropology* to be an attempt to reach a level of abstraction at which the cultural content of social relations could be abstracted to the point where one dealt with interaction patterns and with the categories of phenomena which determined the rate, durability, etc., of interaction patterns. That is why you are not talking about culture; you are talking about social structure, which is a different level of abstraction. We try in England to teach our students this difference and to get clear ourselves about it. We confuse arguments if we do not appreciate the difference of the level of analysis.

CHAPPLE: I agree; however, too much is done in the field of so-called "interaction studies" which eliminates the factors which control, limit, modify, or change the potentialities of interaction. Consequently, you get into the sort of position which Mayo and people of that sort have, and I am therefore willing to compound a felony by shifting levels of abstraction.

MEAD: If you had the same factory setup in terms of flow and physical structure and item of production, and if that was entirely manned by Chinese or Japanese or Italians or French or Americans, respectively, it would be exceedingly important to know the way in which those individuals represented their respective cultures as a factor limiting your measurement?

CHAPPLE: That is bringing in another dimension.

MEAD: Do you accept that dimension, as I understood from your paper you did?

CHAPPLE: Yes.

STEWARD: We are avoiding what is, perhaps, the most fundamental and difficult and dangerous problem—the total culture of which the factory situation is a part. . . . In all the areas of technological change in different parts of the world where people have been on a subsistence basis, money is being invested, there will be created something comparable to factory systems, and drastic changes in the entire culture are taking place. If you are going to apply anthropology, I do not see how the whole problem of the basic values of the culture, the directions in which it is going, the changed lines of power in the society, the changes in the class structure, will not have to be called into question. . . .

CHAPPLE: At the present time some of us are concerned in other areas with something like that sort of problem. The anthropologist does have to look at the total system. Furthermore, he has to make this kind of statement: "If you do this, then this will happen." You are going to have to do that at all levels dealing with systems if you are operating in problems of human organization, of social organization, and

social structure, and the facilitation of change. . . . Whether or not somebody takes our advice is something else.

MEAD: Would you not say that it is the duty of the anthropologist to point out what the human consequences will be of the alternative sets of action?

CHAPPLE: That is what you have to do, and if you do not do it, as a consultant, you are going to be out of consulting work fairly soon.

STEWARD: I wonder if you do not have to go beyond just saying what the consequences are. If we had to tell Puerto Ricans what to do to solve their overpopulation, their low standard of living, and other practical problems, I think we would simply have to reply in the existing total framework, "You can't do a thing about it. It is going to take some drastic and far-reaching changes, the nature of which we are not prepared to say."

FORDE: We use this over-all global word "values," but it does not relate analytically to one single category. . . . The use of the word does not allow us to discriminate between statements of relations of activities in terms of cultural content and of cultural forces operating on them and the more abstract social relations concerned.

LOWIE: Regarding Nadel's point on value, I think we all ought to recognize that there is this subjective element. It exists not only in cultural or social anthropology, but also in sciences such as astronomy, where the subjective equation has long been recognized. . . .

My subjective reactions to three groups with whom I have worked have been very different. I thrill to the Plains Indians, I welcome the Basin Shoshonians as representing humanity groping toward humanity, and for the Pueblo Indians I have the same feelings that Rousseau had toward overcivilization. I am well aware that my reactions toward the Hopi are not shared by a great many others. At the same time, in my discussion of the Hopi clan system and the Hopi kinship system, my personal reactions have not been, I think, unduly intruded on the reader. . . .

HELD: . . . From the discussions and the various papers, it is clear that much can and should be done in applied anthropology but very little is done. This situation is related to the situation in the field itself. There is an inverse ratio between the complicatedness of the culture and the amount of applied anthropology used in the particular culture. Much discussion has been devoted to groups with a rather simple social organization, and in these tribal groups there is much use for applied anthropology. In this case of simple groups I see no reason why the administrator should not be an applied anthropologist. . . .

But I do not know whether anthropology wants to restrict itself to the tribal groups. The remark was made during one of the sessions that anthropology should be concerned, in the first place, with *Gemeinschaft,* while some other science—say, sociology—should be more concerned with *Gesellschaft.* In Indonesia this leaves out of the picture the more complicated cultures, like those of Java, Sumatra, etc. An applied anthropologist there is in a completely different position. For example, the Menangkabau are matrilineal. This system is not accepted by Moslem people, so for over a century there has been a difference of opinion between the government, on the one side, and the Moslem people, on the other side. The Moslems want to change it from a matrilineal to a patrilineal system. The position of an anthropologist in such a situation is impossible—this is not a question of appreciation of Moslem culture or of other cultures but a question of values as applied by the native culture itself, and you have to take the position, and that is what I was refer-

ring to in so far as value systems are concerned.

NADEL: There is another motive, besides advancing science, in our turning to applied anthropology. That motive lies in the fact that we are not only anthropologists but also citizens. Evans-Pritchard has suggested a procedure whereby you are an anthropologist on Mondays, Wednesdays, and Fridays and a citizen on Tuesdays, Thursdays, and Sundays. I do not know how that can, in fact, be carried out, but the point is important in regard to what Dr. Lowie said. As we are not only citizens but also heirs of a tradition of thought, we must expunge our personal individual values, preferences, idiosyncrasies.

But as to [our values as citizens], we cannot and should not expunge them.

I speak with feeling because during the war, when certain measures were going to be taken on the basis of what I had investigated, I would not agree, not as an anthropologist but as a human being. I faced court martial on one occasion for refusing to agree to a measure of collective punishment of certain tribes, and I can see no reason why any anthropologist should not stand up for his opinion, even though he may be an administrator or part of an administrative body. This is a question of conscience, but I think that in the study which we proudly call the "study of man" conscience has a very definite place.

HELD: I did not intend to say that it is possible to leave out your own value system completely, but I think there is a difference between the applied anthropologist's being creatively active in his own value system, as, for example, in industry, as contrasted with trying to be creative in another value system.

FORDE: I agree with Nadel's theoretical position, but, practically, it does not usually come to critical situations of the kind he was just mentioning or even to major difficulties. One of the most important values which an anthropologist carries around with him is the value of objective investigation. That word "objective" means that he makes an investigation according to the canons of his discipline, that he faithfully recalls both the essential observations and the argument by which he reaches his general principle. That is a value element of great importance in all scientific workers. Where you have extreme values of another kind impinging on scientific work, scientific work can become impossible. . . . An anthropologist might make an investigation of the social structure of a community which could equally well be used by two diametrically contrasting administrations. . . . When we are dealing with what use should be made of our knowledge, we are at a different level in the value system from the scientific code which we employ in investigation.

MEAD: In the code of ethics of the Society for Applied Anthropology, we have attempted to go one step further and say that no citizen who is trained as an anthropologist can ever act as a citizen without taking his anthropology into account—that he cannot suddenly divest himself of his skill, of his knowledge of social process, and act as if he had never been an anthropologist. . . . We must be responsible to our own discipline for the way in which we use the skills of that discipline.

EGGAN: I would like to talk in terms of these higher levels of abstraction. The international rules of law provide for collective punishment under certain conditions. What are you going to do about that?

NADEL: Precisely what I have tried to do, and I pulled it off, too.

BEALS (submitted in writing after session): The discussions of applied anthropology in government shocked me by their complete acceptance of the view that the basic problem is how

someone can do something to other people tacitly understood as inferior or subordinate. Not only was this true in discussions of government anthropology of the United States, Great Britain, and France; it was present in the opinions voiced by Mr. Shah regarding the tribal peoples of India, and in reports of the fate of divergent cultures in Indonesia. Mr. Chapple avoided some of the onus of a similar attitude in his own position regarding anthropology in industry by insisting on "if . . . then . . ." statements, i.e., "*If* this is done, then the following may be expected."

It is time some, if not the primary, emphasis of applied anthropology should be on determining what people want and aiding them to get it rather than on how they can best be persuaded to do what people in another culture think is best for them. The latter too often is a rationalization really concealing what is thought best for the dominant culture. I recognize that the task often is not simply to find out what people want but to help them arrive a a solution of present difficulties satisfactory to them. It may be said that, if dominant cultures would leave other cultures alone, there would be no need for this second task. . . .

In our present world-wide technological revolution, applied anthropology ethically should be most concerned with enabling people to recognize the inescapable forces pressing upon them and presenting to them, so far as is possible, the various alternatives open to them, together with the hidden implications of each course of action. I would argue for cultural self-determination to the fullest extent compatible with world needs. Whatever the rights and wrongs, I am sure no people is going to be able to sit unmolested very long on a rich iron or petroleum deposit, but at least let people have some voice in their own fate.

I can hear some anthropologists saying that this is arrant nonsense because: Who is going to pay for this kind of applied anthropology? To this I have no immediate answer save to suggest that those of sensitive consciences may sleep better of nights doing this kind of application than do those serving the cause of "policy" and administrative expediency.

LANGUAGE TEACHING

HOIJER: It seems to me that the apparent difference in success between that which the linguist has had in convincing language teachers of the method of language teaching and that which has occurred with respect to other fields of applied anthropology relates to whether we are talking about culture or interpersonal relations. The linguist has attempted to introduce a very small modification in the culture of those who teach languages. His lack of success is in part related to this: that it is perhaps more difficult to change culture patterns than it is to change, readjust, or repair disturbances in interpersonal relations, whether on a large scale or on a small one.

VOEGELIN (statement revised after session): I have been struck with the recurrent theme this morning of questioning whether applied work in anthropology would or would not contribute in a reverse fashion to theoretical work.

It is hard to see how some of the applied work in linguistics would help us in theoretical work. Thus far, it has not. There is, however, one point in which the applied work apparently has some interesting theoretical possibilities, and that is what Mary Haas describes with great clarity, the work of Charles C. Fries and his Latin American Institute. There is also the work which is being done at the Foreign Service Institute by Smith and Trager and others, and by Harris at Pennsylvania. Clues to significant features of a language that is being learned may

emerge which may have been overlooked in the previous technical analysis of this language; for example, stress patterns, junctures, and intonations in English, when taught to native speakers of Spanish, German, Italian, and other languages, highlight these significant features.

LOUNSBURY: Part of the difficulty in changing the culture of language teachers, of which Professor Hoijer spoke, results from the lack of use by linguists of some elementary concepts in applied anthropology.

MEAD: This distinction between changing a culture pattern and changing a system of interpersonal relations is a matter of level; it is one of those problems of simultaneity. If you study them both at once and try to alter them both at once, you may get very good results.

SPOEHR: This factor of resistance in changing language teaching is itself an important problem which might lead to significant theoretical results.

GREENBERG: It is not only a matter of changing the attitudes of language teachers. Assuming that they would be sympathetic with us, the fact remains that our society and its school system have set up certain aims and purposes for which languages are to be used. To learn a language by the methods developed by professional linguists in the Army program means that far more time would have to be given to a particular language in our teaching. That would upset the whole curriculum and produce a vast number of other problems.

HAAS: I was just about to say what Mr. Greenberg has said. Another very important factor in resistance is the extreme expense involved in this type of teaching if it is properly done. . . . You need a native speaker in addition to the linguist, and you need many more hours and smaller groups. . . .

ROWE: . . . Much of the resistance to linguistic language teaching is connected with the fact that the linguists are not very much interested in the sort of literature studies that many of the traditional language teachers are. One reason for the success of the linguistic method in Army programs is that the people in charge of the program were not interested in literature.

BREW: Here is the first optimistic thing I have seen in this linguistics discussion. The one break in the college curriculum is coming in that area, and we are seeing in some places a division, even in some cases a departmental, split, between teaching a language and the literature in the language. . . . It is a break you can take advantage of.

THOMPSON: The objections that are made by the conventional language teachers have not been entirely represented here. . . . One is the matter of hours: "If you give us conventional language teachers that many hours a week, we can get further than the other people can." That might be true, and I think no test has ever been made of it. Second, the people study these languages for about one year. They may be better speakers of the language, but their actual knowledge of the language may be a good deal less. . . . I do not think we need be apologetic about the fact that our people want to learn some literature as well as language. Then there is a feeling on the part of the conventional language teachers that the so-called "linguist" is a little better off if he does not really know anything about the language at all—if he just started with a clean slate and began to analyze the language. The idea that, as a part of the pedagogical process, one has to work out an analysis of a language before one can start teaching the language has turned up very frequently and is a vulnerable point in the whole system.

CHAPTER XII

Technological Aids in Anthropology

Chairmen: Junius Bird and J. M. Tanner.
Paper discussed: Rowe, "Technical Aids in Anthropology: A Historical Survey." *
Speakers: Fejos,† Rowe, Lounsbury,† McCown,† Tanner, Washburn, Oakley, Ivanicek, Brew, Lévi-Strauss, Mead, Termer,† Tax, Caudill, Bidney, Lowie, Bird.

PSYCHOLOGICAL BARRIERS TO THE USE OF TECHNICAL AIDS

FEJOS: Dr. Rowe's inventory of technological aids in anthropology has a dual importance for the symposium. It represents the first systematic summary of tools and methods contributed to anthropology by the physical sciences. This is its first importance. But its real significance is that it confronts us with an implied—so to speak, silent—diagnosis of the discipline, and that, I believe, asks for examination.

Dr. Rowe's paper is wide in scope and thorough. The short time at his disposal for such a very large subject seems to have hampered him little. His paper gives the impression that anthropologists use tools and methods from physics to biochemistry with great inventiveness and understanding; that these tools are used intensively and frequently. If, however, we bear in mind that Dr. Rowe covers the history of technological aids through the whole life-span of anthropology, this seeming record of magnificence becomes a certificate of poverty, for we find that the methods and tools are used only occasionally, and frequently without much understanding as to their nature and potentialities.

Technological aids are often referred to by anthropologists as "gadgets." This I believe to be somewhat diagnostic. Webster defines "gadget" as "a contrivance, object, or device; a 'thing'; a 'jigger'; esp. a part of machinery;—often used of something novel." I find that the term has a sort of derogatory connotation when it is used to refer to a spectroscope or a carbon 14 laboratory. I have the feeling that little understanding can be in the mind of a person toward the scientific instrument that he calls a "gadget"; and, as one who has earned the nickname "gadgeteer," I would like to present a brief in behalf of the gadgets.

The time has come when anthropologists must recognize that human sensory organs are notoriously poor and unreliable instruments. . . . The vibrations of valence electrons are, in fact, the only sector we can see. Our ears cease to hear at around fourteen or fifteen thousand cycles. Our senses of smell and touch are equally poor or even worse instruments. Physicists, chemists, geologists, and so forth have for decades discarded the sole reliance on their eyes and ears and have invented and, routine-like, are applying

[1] In addition to this session, an evening was devoted to exhibition and demonstration of various technological aids, under chairmanship of Dr. Paul Fejos. The proceedings were not recorded.

mechanical, electrical, or electronic instruments with which to see, hear, smell, and touch, recognizing that our everyday senses have insufficient capacities and accuracy for scientific research.

A great number of these tools are available for the anthropologist. Others would need to be altered for our special purposes. They would enable us to solve innumerable problems that we face at present in anthropology. But in order that such a thing should come to pass, the anthropologist's attitude toward gadgets needs some slight change. It is not necessary that he become a physicist or a chemist, or even a technician. He would merely need to recall some of the basic sophistication he received about chemistry and physics in his high-school years.

Let us suppose that a primitive folk, such as the Jivaro on the Marañon in Peru, were visited by an epidemic or other catastrophe that made them extinct. Let us also suppose that Karsten's and Stirling's monographs on them had never been written and that Julian Steward's admirable handbook had never existed. Two hundred years hence, but with our present methods and usual equipment—the spade, trowel, scalpel, camel's-hair brush, notebook, and pencil—an archeologist would decide to dig on the sites of the Jivaro.

What would he be likely to find? A few large chicha jars, some ollas, an amount of broken pottery, some with ornamentation. If he were open-eyed and good, he might discover a few postholes and, possibly, traces of the haphazard agricultural chacras used by the Jivaro. That would be all.

As the customary next step, our archeologist would wash and classify his pottery. He would find that it was made by coil technique, occasionally with geometric designs in the interior of the bowls. He would set up an elaborate typology of color, shape, etc., but at the same time he would also arrive at the conclusion that the material resulting from the dig was indeed poor and hardly sufficient to reconstruct the bygone culture of the Jivaro. As the next step, therefore, he would proceed with the examination of the archeology of the neighboring areas and the ethnology of its peoples. He would find that the Andoa, Maina, Puruha, and other tribes neighboring the area are, as the Jivaro must have been, simple forest people with a culture centering around subsistence activities—haphazard agriculture, hunting, and fishing—and, with the exception of rare and minor intertribal fighting, a peaceful folk, living an idyllic existence in the Amazon forest.

In reality, as we know, he would be far from the truth, as Jivaro culture seems to have as its paramount feature the extreme zeal for war and blood revenge. The integrating factor is, as a matter of fact, war. But our hypothetical archeologist could hardly be blamed. The *tsantsas,* warclubs, and spears could not be found by him. His monograph would very properly state that his findings are based on meager evidence, since, with the exception of pottery, all other artifacts "disappeared" or were "lost" in the two hundred years of burial in a tropical forest.

It is with the "disappeared" and "lost" that I would like to quarrel. Our hypothetical archeologist was taught in high school that the basic law of chemistry is the conservation of matter. Nothing disappears or gets lost after two hundred or two thousand years of burial. Artifacts, however, do change in their appearance and constitution sometimes beyond recognition during the passage of time, from innumerable and sometimes complicated causes. It is at this point that our hypothetical

archeologist was hopelessly handicapped by his singularly specialist-oriented mind.

The etiology of this ailment, I believe, at least in part, lies in our past. Early archeologists in Europe were more interested in art history than in culture. Their aim was to obtain objects of artistic value and to exhibit them in the vitrines of their museums. They were not interested in recovering evidences from "changed-beyond-recognition" artifacts and the reconstruction of an ancient culture. For their aims, such methods were indeed sufficient. Archeology inherited these methods but, with the exception of the more elaborate notebook, changed them little.

I believe that the present-day archeologist should have something in common with the police scientist. Both are interested in prospecting for evidence, recovering and preserving or recording that evidence, and finally analyzing or interpreting it, to arrive at facts and conclusions.

There is, however, a great difference between the two groups. The police scientist reaches out ever further for other disciplines to aid him, and adopts new methods daily for his purpose. The archeologist, by and large—and I am happily and gratefully aware of the exceptions—wishes to work as a "lone wolf." The police scientist for two decades has tapped the extensive literature on identification of and change in materials. The wood and leather technologists have filled volumes with information as to what happens to wood, leather, etc., under varying time, weather, temperature, and humidity conditions. The metallurgists have composed libraries on the subject of weathering of metals and their alloys. By and large, archeologists seldom consult such literature and are frequently unaware of its existence.

As a result, an untold amount of highly significant archeological evidence becomes irrevocably lost at many digs. To a large extent, the archeologist has little idea as to what objects he is seeking, or, rather, he finds objects only which he can readily recognize with his naked eye. When the worker's spade hits something solid, the archeologist takes over, to clear the object with his trowel, later scalpel, and, finally, camel's-hair brush. But if the object has changed its appearance and constitution to the extent that it is not solid enough for the spade to feel, it is thrown against a screen. If it disintegrates or is small enough to pass through the screen, we can regard it as lost. For instance, if an object is made of iron and is sufficiently thin and if time and humidity conditions are proper, it will change from Fe to become Fe_2O_3, in other words, be transformed to powder. If it is then thrown against a screen, it will pass through and be lost. In the laboratory, however, with a very simple and very old method, the Fe_2O_3 could be reduced to the original metal, and we could have the whole article.

Lost also will be imprints in the soil. Burials are cleared carefully as to cleanliness. Textile imprints, for instance, visible on the hardened soil surface near the bone will pass through the screen without any detection and then really disappear. Microanalysis by grid method on suspected areas, made feasible and even practicable and cheap by the present-day spectroscopic method, has been employed only once or twice in the whole span of archeology. The tools and methods for better results for recovery of evidence do exist, and the *Journal of Scientific Instruments* has practically no number in which potentialities are not numerous for anthropology.

It is probably a great task for the

anthropologists to become somewhat more technically minded, and many of them will consider it an impossible task. I have found this to be true with a number of anthropologists and with most of my own students. Not only, however, would I strongly disagree that such objection is valid, but I am tempted to ask if it is not a "must" for us, no matter how difficult.

I have here the mail-order catalogue of Sears, Roebuck and Company, which I consider a pretty good inventory of American material culture. I would say that the average American citizen really possesses the things which are in this. I have taken this catalogue and removed every page which depicted an artifact, that I would consider would "disappear" or be "lost" after two hundred years' burial in a tropical wet-forest ground. More than half the catalogue is out, you will notice; that is to say, if we did not have actual knowledge of this culture, if we did not have literature upon it, we would face only this remnant, and this is generous.

I have tried and also made my students try to reconstruct the United States culture of today from the materials remaining in the catalogue. The conclusions at which one is bound to arrive during such reconstruction are somewhat startling, to say the least. This is an exercise I would like to recommend for use with students. They will then see how different from reality and fact are the conclusions at which one would arrive on insufficient evidence.

Another aspect of the gadgets I would like to mention is the attitude in using them, both of the archeologist and of the ethnologist. As was mentioned by Dr. Rowe in his paper, the use of technological aids is generally for an obvious purpose. Thus magnetic recording, for example, is used almost exclusively for linguistic texts or the recording of music. In the hundreds of loans we have made from the Foundation's loan equipment of wire, tape, and disk recorders, I cannot recall a single instance for the use of recording interrelations, stress situations, cultural dynamics, or anything of that sort. Only the obvious subject was recorded. This holds equally true for motion-picture cameras or still cameras.

I am well aware of the fact that a few researchers do use such instruments for analytical purposes or as tools to record the less obvious. But they are a small minority among field workers. The potentialities of the instruments lie in other directions; that is to say, for instance, the photographic camera should not be used exclusively for "keepsake" photography but, rather, as a scientific tool, to aid accurate and true recording, or, what is even more important, to enable us to see things the naked eye cannot. Unfortunately, the attitude of the anthropologist is to use such equipment only when it can be handled as "pushbutton" magic. There is a great deal of reluctance from field workers who come to the Foundation to borrow equipment to get acquainted with the instrument, even to a minor degree. Although we would be glad to give information and training about the use and the potentialities of the tool, the answer that we frequently get is, "Oh, just give me the gadget; I'll find out about it when I go down to Peru, or go to New Guinea." The result is that the tool often gives poor service, if any, and the method is dismissed as unsatisfactory.

Many times, so-called "economic" considerations falsely exclude the use of potential tools. The weight of a camera or its size is taken into consideration alone, not the quality and quantity of the results that are to be expected. Instance the example of a student, who, when I talked about aerial photography and mentioned that I thought aerial photography had even

greater potentialities in ethnology than in archeology, looked at me rather puzzled and then said, "What do you mean? I mean, how could we use it?" I pointed out that he was interested in the Amazon forest area, and if he went there to do ethnology and wanted the population count, aerial survey would be the fastest and easiest way to do it; in about thirty minutes' time, he could count the population, which normally would take him a very long time. He listened to it nicely, and then he turned to me and said, "Yes, what you say is true; it must be a great gadget, but it is not for us. It is easy for you to talk here at the Foundation. Where will I ever get the money to hire a plane and photographic camera?" He forgot that the cost of photographing for two hours in the air, let us say in Peru, would amount to about $150, whereas the population count for about six months, along the Patumayo River, if you can do it in six months, may run into about $10,000.

The fact that an instrument weighs ten or fifteen pounds often deters the field worker from taking it, because he says, "I am going by plane; that is excess luggage." The excess luggage maybe would amount to $30, and he would save several thousand dollars, plus the fact that he would come back with an accurate result. But this is "economy."

Too little or no acquaintance with tools or methods and an almost dogmatic defeatist attitude that "the gadget may be good, but, look, I'm no physicist" is largely to blame. The therapy for these ailments, I believe, is to be found in the "how" of education for future research workers in anthropology. Additional courses would be needed, tool and method courses, not standing isolated in the curriculum but organically tied into it. Such courses should also take care to abolish the current frequency of the "magic" attitude, the desire to have "pushbutton" gadgets, and firmly plant the knowledge that an instrument is only as good as the degree of the researcher's acquaintance with it.

Anthropology inherited a certain amount of romance with both ethnology and archeology. This possibly gave us a certain looseness as to scientific accuracy. But, with the present growth of physical sciences, such a degree of looseness is not long tenable, however comfortable or happy it may make us.

As one small example, I would like to ask what scientific strategy do we really have for selecting or prospecting archeological sites? I am afraid that more than half these sites are accidentally and not scientifically located; that is to say, I walk in Peru and a peon brings me an artifact and I recognize that this is proto-Chimu, so I ask him, "Where did you find it?" He points out at the end of the quebrada. That is where I will dig. I am very much afraid that an oil company, for instance, would never prospect for oil in the same fashion.

Some of you will feel that I have vastly overstated my claims. I am aware of the small group of researchers who are alive to the uses of tools and techniques borrowed from the physical sciences. But as a foundation worker, who, through the last decade, has had to keep in close contact with field-work plans, methods, and also results, I see a certain need for a small change, or at least an attempt at change, in our attitude and, above all, in our curriculums for our students.

ADDITIONAL NEW TECHNICAL AIDS

ROWE: My inventory paper includes a considerable bibliography. I want to point out that I have an additional hundred or more bibliographical references, including a good many cases of application of techniques in anthro-

pology which I did not cover for lack of space or other considerations.

However, the survey was also limited by the amount of time that I had to do it, and I have run across a number of additional items, including corrections and additions, which I would like to call attention to.[2]

First, there is a comment which needs some correction. This refers to a case in which the chemist, Dr. Fink, did a processing job on a couple of Egyptian specimens and concluded that the Egyptians possessed a chemical method of plating antimony on copper. At the time I put in this item, I had been reading Forbes's book on ancient metallurgy rather superficially, and further reading in the book brought me to the following passage: "The sensational discovery of Fink and Copp that the ancient Egyptians understood the art of plating copper objects with antimony was disproved by Lucas, who quite correctly points out that this antimony plating is due to the electrolytical reduction of the copper object. The antimony is simply an impurity of the copper, and the so-called plating due to the method of restoration and not a process applied to the original object. The only instance of antimony in Egypt is the find of a few beads in a grave at Lahun of the XXII Dynasty. Petrie quite correctly infers that these are imports, as antimony ores are not known to occur in Egypt."

This case is instructive and important. It is very easy for anthropologists to put excessive trust in a laboratory report involving methods and processes which they do not fully understand; and we should remember, in considering the use of technical processes, that the technical literature needs the same kind of critical evaluation that we expect to bring to bear on our colleagues' field reports.

With regard to additions, I want to call attention to certain publications. Dr. McCown has shown me a work published in 1909 on photogrammetric anthropology, including a discussion of photogrammetry for other purposes as well. This is a French work called *Anthropologie métricique,* published in Paris. Alphonse Bertillon was the senior author, and it was designed to give practical instructions for photogrammetric anthropology to people going to out-of-the-way parts of the world.

Dr. Heizer called my attention to a reference to what is probably the first use of ultraviolet light in anthropology which appears in the *Annual Report* of the National Museum of Canada for 1931, written by W. H. Collins. The passage reads: "An ultraviolet lamp has been constructed and has proved to be of value. By its aid, faded labels and specimen numbers are rendered more legible. The use of iron in anthropological specimens, thus proving them post-European, is easily detected. Aniline dyes are rendered evident in many cases. Faking and retouching of specimens is shown. Various fibers can be distinguished from one another, and pathological conditions in bones and teeth are sometimes revealed. Such lamps have already been employed in art museums, but this is believed to be the first time ultraviolet light has been employed in anthropology." This was published in 1932.

I also want to call attention to a bibliography of microfilms and microcards issued by the General Reference and Bibliography Division of the Library of Congress in 1950. It is by a lady named McCrum, and it is entitled, "Microfilms and Microcards: Their Use in Research."

Finally, I want to mention a reference which I picked up from a Berke-

[2] Dr. Rowe's inventory paper was revised before publication to include in its bibliography items here discussed.

ley newspaper and which Dr. McCown was kind enough to help me with. He directed me to a technical publication on the subject. Professor Paul Leland Kirk has published a small book, which appeared in 1951, on density and refractive index, their application in criminal identification. The technique described is one which has not, as far as I know, ever been used in anthropological work. It is supposed to be useful in the identification of specific sources of specimens of soil, rock, glass, paint, hair, wood, paper, and other solid substances, and one suspects that there might be certain archeological problems to which it would be relevant.

The density business involves putting a small sample of dirt, let us say, in a series of columns of liquids, and finding the pattern in which the grains settle in the various columns of liquids, and apparently quite precise identifications can be reached by this means.

TECHNICAL AIDS IN LINGUISTICS

LOUNSBURY: . . . In the case of a language spoken in a nonliterate society, nothing is left from the past, and if languages now spoken die out before anything is done about them, it is a loss.

Most anthropologists and many linguists at the present time are disinclined to feel that this is really much of a loss. After all, language will always go on. I might say that, also in linguistics, there has been a trend to discount the importance of knowledge of a great variety of languages in the world, because of a growing interest in what one might call studies in depth and in detail, in language, as opposed to studies in variety. The fact is, however, that the natural science of language is built up not by experimental method or anything like that, but derives from the natural history of language . . . We can have a natural science of language only if we have some sophistication as to the dimensions of variability and the ranges of variability and all the different aspects of language, and that we can get from studies in variety alone. . . .

Now, I am very much interested in gadgets, as they have been called. I have used them. I have seen others, however, be unsuccessful in their use. As a matter of fact, on the basis of this, I divide humanity into two types—those of the watchmaker type and those who are like a bull in a china closet. There are people who simply cannot use a wire recorder; others, however, have no trouble with it.

A couple of summers ago, I had a chance to go to Matto Grosso, and my object was to get as much linguistic data as possible, making use of machines. As it turned out, I managed to get data on six languages from four different linguistic stocks in only two months by the use of machinery.

This did not all work out ideally because, although many types of machines are available to us now, there are still types of machinery which we need and which are simply not available. . . . I will merely say that by making use of what we did have I was able to get much more than I could have without machinery.

First of all, the general method: I proceeded as usual, taking notes on phonetics, arriving at a tentative phonemic solution, arriving at a fair key to the grammatical analysis, and so on, all of which can be done in a week's time. Then I supplemented with recorded texts, making use of two recording machines. If you just record a text, letting the natives tell a story and so on —which is all many anthropologists and nonanthropologists bring back from the field—it is totally useless, really, because you can never correlate translations with these recordings. I

make use of two machines, let the informant tell a story and, as there is always some phonetic marker in any language, for "end of sentence," besides "pause," which may or may not be there, it does not take long to discover the phonetic markers for "end of sentence." I made the test, by the way, before I ever took the machines into the field. I took some Iroquoian texts and described what the phonetic markers for "end of sentence" were to one of my students, and tested him on it. I asked him to raise his hand whenever there was an "end of sentence" in a language which he could not understand. I used this with informants, and was able then to play back my recording of texts and interlard translations, sentence by sentence, while both the playback of the original and the interlarded translation were re-recorded on the second machine and thus got them correlated. Texts were collected this way more rapidly than could have been done by hand. This is functional when you have only very little time in the field.

I have had previous experience with the problem of transcribing from disks. I went through one fairly lengthy ceremonial address in Cayuga, which I had previously recorded and transcribed on a homemade repeater that I had rigged up myself. But we need another method for transcribing, most of all an automatic repeater which does not require dubbing the recording onto a second machine. I have designs for such a machine, but none has ever been built.

Another thing we need, now fortunately on the market, is a magnetic recorder which does not waste electricity on propulsion of the recording medium but makes use of a spring motor which is hand-cranked and makes use of electricity only for the magnetic recording itself, and this, with a few flashlight batteries, can record a hundred hours or so of text, if cranked every six minutes, as the recordings are being made. This is the obvious solution, of course, for places without electricity.

This equipment was not available when I went to Matto Grosso. What I did was to take the soundscriber inverter with me and borrow a truck or jeep from any missionary that I could find in the vicinity, put the inverter on the car battery, and record in that manner, while in other places, I carried a heavy storage battery with me. There are some other things that we need in order to make such methods completely feasible. When we have them, we can really envisage a program which would salvage what we need to salvage before it is gone.

DRAWING AND DRAFTING

McCOWN: Is there some reason why drawing and drafting were left out?

ROWE: I felt that it was not within the limits of the sort of thing that was suggested by the planning committee, though I did put in a reference to one book, Ridgway's *Scientific Illustration*, which is listed in the bibliography and referred to in the section on photography.

McCOWN: I raise the question because, increasingly, certain kinds of drawing and drafting are going to be of obvious importance, that is, the construction of graphs. I am more and more impressed by the fact that too many people approach this entirely free of any conception as to what a graph is supposed to do.

PHOTOGRAMMETRY

TANNER: Since we are here talking merely about technical additions, corrections, and so forth to what is essentially a technological bibliography, I want to say a very few words about what is mentioned as "photoanthropometry and standardized photography." Dr. Rowe means, I take it,

measurement of the human by photographic means.

It might be worth pointing out that the term, "photogrammetry," was introduced . . . in 1859 by a balloonist, who measured and mapped some ground by taking aerial photographs. He called it "photogrammetry," and that term has been widely used ever since by surveyors and engineers and physicists to designate the science of measurement by photographic means. There is, for instance, at University College in London a professor of photogrammetry and surveying. There are also texts on the subject. For instance, a book by Otto Luckman,[3] published in 1950, which treats of all aspects of the photogrammetry and particularly photogrammetric anthropometry. . . .

I might mention in passing that there is work going on at the present time in stereoscopic photogrammetry which enables you not only to measure the straight dimensions of the person you are photographing but also to get his body volume, and his surface area, and to make complete contour maps of him in precisely the same way as you make contour maps from an airplane. As a result we now have a man drawn out in contours and, from the contours, reproduced on flat surfaces. By machines, you can construct cross-sections of the man in three dimensions, so that you can reconstruct the whole man in whatever detail you wish, by the use of stereoscopic photogrammetry.

One other point: the matter of distortion. I mention this not only because this happens in photographing the whole person, but in photographing skulls and similar objects. There is some confusion about this distortion. A lens should not distort more than a few tenths per cent. The idea that distortion in the proper physicist's sense of distortion is anything except malmanufacture of the lens is quite erroneous. There is a thing which the physicist or the photogrammetrist calls "parallax error.". . . The difficulty arises, you see, because, if you take the side view of a man, his side is nearer the camera than a plane through his middle, and the error thus introduced by this parallax is proportional to the distance away that you are. If you go away to about 10 meters, then you cut the errors down to something of the order of 1 per cent. If you go away less than that, you have larger errors. That is all there is to it, but it seems to have caused some confusion.

There is also a feeling current that if you use a particular sort of lens, a lens which has a certain focal length, this has something to do with the distortion. Again, of course, this is quite an erroneous notion. The distortion depends on the quality of the lens and not in the slightest degree upon its focal length. The focal length merely tells you where to put the lens with relation to the object, so you fill the film up; that's all.

Second and last, I see in an unpublished manuscript, coming from Sheldon and Barbara Honeyman, something about stroboscopic lighting. I am not clear about this. Stroboscopic lighting, interrupted lighting, has been used for a very long time by zoölogists and other people, but I think the lighting they actually mean is cold-cathode electronic-discharge lamps, which is quite different in principle and action and very useful.

REORIENTATION OF PHYSICAL ANTHROPOLOGY TOWARD TECHNICAL AIDS

WASHBURN: . . . We were all conditioned in our education to use measurements as the primary tool. This interest shifts very easily from measurements taken with calipers on the individual to measurements taken off the

[3] Otto Luckman, *Die Photogrammetrie in ihrer Anwendung auf nicht topographischen Gebieten* (Zurich, 1950).

photograph, in which you are obviously doing essentially the same thing. On the other hand, most of the problems which we discussed can be solved only with other techniques, so I feel that what we have to do is to change our attitude relative to techniques in physical anthropology and make physical anthropology not a science of measurement but one that gets after certain problems with whatever techniques are handy, and in that case, we might be using one technique this year and a different one next year, and different people different techniques.

THE ELECTRON MICROSCOPE

OAKLEY: . . . I think that an application of the electron microscope which has recently been published in *Nature* would be worth mentioning. Professor J. T. Randall, at Kings College, London, is doing work on protein fibers and especially collagen fibers. He has been so impressed by the uniformity of structure of collagen through different animal groups that it occurred to him that it would be interesting to know whether this uniformity extended into the past.

The British Museum of Natural History let him have some fossil materials in which collagen might have been preserved. People think of collagen, as a rule, or any protein, as something that does not survive for very long. We provided him with a series of specimens of bone and mammoth ivory from various parts of the Old World, and he decalcified these and put the residue under the electron microscope. From the mammoth ivory, procured from frozen soil in Siberia, he got the collagen fibers perfectly preserved at a magnification of 40,000 diameters, and the characteristic banding at 640 angstrom units was almost as clear as in fresh material.

In fossilized ivory from unfrozen ground, ivory of about the same age, perhaps 20,000 years, the collagen fiber was still preserved, with the characteristic banding, but more fuzzy. I also provided Professor Randall with a small pinch of the lip of a mammoth from frozen soil in Alaska, and the collagen fiber, of course, was perfectly preserved there.

Professor Randall said that he had not expected that the actual detailed structure down to 640 angstrom units would have survived even under refrigeration conditions for 20,000 years for these internal denaturing changes to take place. This just opens up a possible field for the application of electron microscopy to anthropological problems. We cannot think of a problem immediately, but it is just worth knowing about. . . .

REPLICATION OF ENAMEL SURFACES OF TEETH

OAKLEY: I would like to touch briefly on the work of Dr. David Scott, of the Bethesda Dental Research Station, who is using the method of replication of enamel surfaces of teeth on films of collodion and then shadowing the replicas *in vacuo* with vaporized metal. Fine microscopic structures are thus made more evident. There are several papers on this technique by David Scott, Wyckoff, and Pedersen. Professor Pedersen (Copenhagen) told us in London recently that he had in mind applying this technique to the study of enamel surfaces of fossil human teeth, because he thinks that there might conceivably be racial or specific differences in the pattern of ridges and pittings which these surfaces develop during life. It has already been found that the teeth of Eskimos and American whites differ in such respects, probably owing to different dietary habits.

POLARIZED LIGHT

IVANIČEK: I would like to speak about the application of polarizing light

in anthropological research on human bones excavated from old graves. We have in this method possible new auxiliary means of dating. In the ground, all bones of skeletons consist of inorganic or organic substance. Both disintegrate and rot until total disappearance. Meanwhile, both parts of bones—the organic and the inorganic—do not disappear at the same rate. The organic substance disappears first, and later the inorganic substance disappears. The most resistant element of organic tissue in bones is the collagen matter of the osteofibrils. The disappearance of collagen matter is dependent on the humus acids in the earth. If we know the quantity of humus acids in ground or soil and the degree of conservation of organic substance in bones, we have the possibility of a new way of dating.

In this work it is necessary to know, first, the acidity of the soil; second, the mode of burial; third, the geomorphologic characteristics of the soil; and, fourth, the degree of conservation of collagen matter, which we can ascertain through research with polarized light. I have tried this on the bones of old graves, where the date of the burial was well known. . . . The preliminary results have been encouraging, and it is hoped that further research may refine the method and enable comparative age studies of bone to be made.

KITE PHOTOGRAPHY

BREW: . . . In Dr. Rowe's paper there was a brief mention of the use of kites for photography. I can say, having used them, that it is an admirable solution for archeological work where you have a big site. We made a box kite and sent it up with a Voightlander camera on it. One could use an alarm-clock arrangement, but we just hung a string down, pulled this at intervals and got very fine pictures. . . . I can say, on the basis of this small experiment, that you can get very good vertical shots for practically nothing by the use of a meteorological box kite.

USE OF MOTION PICTURES

BREW: The subject is one that I normally would not mention at this stage of the project, because I dislike saying much about things which are still in the formative, experimental stage, where the results are as yet unclear. On the other hand, we have a certain number of results. Many of you have heard about this before; some of you have seen a few of the pictures, and heard a little of the sound.

We had an expedition in the Kalahari desert last year, which was making an attempt to utilize moving pictures to get a complete photographic record of the technology and other features of a primitive group. . . .

The point is that we found we had no difficulty with the people at all, and we have the complete sequence of the making of a bow and arrow, from the cutting of the wood up to the final digging-up of the poison grubs and poisoning the arrows; so that the camera is busy all the time. It is not a sampling, but a complete record. There is a ceremony with trances and two medicine men, which runs for two hours and a half, with the sound recordings.

This is not merely in order to show a Bushman technology ritual. It is part of a plan for the fuller utilization of the medium of film, in the same way that we use the graphic arts or language, and it is based on a different concept. The concept of visual aid has really degenerated to a certain extent. We use movies to illustrate a few points, or we use them in order to keep the students from getting too bored with continual lecturing.

The purpose of this expedition really is to begin to lay a factual groundwork for the use of film as a scientific record

or as a historical record, in what you might call reasonable completeness. I have been asked merely to mention this, and I want to repeat that the important thing to me is that, if we are successful, we shall have an example of an expansion of the use of film so that it becomes a real record as against merely being an illustration.

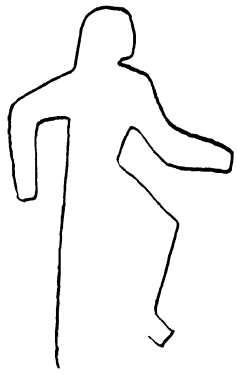
TELEVISION

LÉVI-STRAUSS: A recent development in France, stemming from television, frequency only, you have a sketch like Figure B.[4]

Just as the new machines of the Bell Telephone Laboratory may be useful to reduce language to its basic constituents, so television may be used to reduce art forms to their basic constituents.

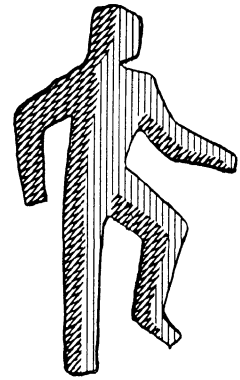
PHOTOGRAPHY

MEAD: If I am to discuss photography I will have to do it on the basis of the three years' work that Mr. Bateson, Miss Belo, I Made Kaler, and I did in Bali, as an experiment on the contribution that photography could make to field methods. Dr. Rowe tells me that he omitted all discussion about it because he disagrees with the approach to Balinese character, which I assume is tying method and results even more tightly together than the applied anthropologists, who believe in the relation of means to ends, would do.



A

Low-frequency bands only



B

High-frequency bands only

can be of great interest for the study of art among different societies.

Some people in the television business have started playing with television, cutting off, for instance, the high- or the low-frequency bands and seeing what happens. It immediately became obvious that some basic distinction in aesthetic thought was made apparent by this process. The classical opposition between drawing and painting did indeed correspond to high- and low-frequency bands. Cutting off high-frequency bands alone, that is drawing; or low-frequency bands, that is painting. For instance, if you have only the low frequencies, you have something like Figure A, while if it is high

However, our intention was to work on the contribution of photography to good field methods and not to docu-

[4] *Premier collogie international de cybernétique sous la presidence de M. Louis de Broglie, Centre national de la recherche scientifique* (Paris, 1951).

ment any particular approach. . . . The problem was whether photography could correct for the changing sophistication of the field worker, because, as we all know, each day you know more about the culture than you knew the day before. Therefore, as a recording instrument, you are continually changing. We believe that photography can correct for this, provided that you take enough photographic material in long enough sequences. The length of the sequence is important, because in moving pictures the principal point from which individual bias is introduced into photography is in terms of when you start exposing and when you stop, if you have a limited amount of film.

The new kind of moving-picture camera, which will make it possible to take two hundred feet with a machine attachment, without rewinding, should correct for that. We also have experiments, done by Grierson and Flaherty, when they both held the camera at the same position, theoretically midriff, and took the same scene, and came out with different pictures. And we have experimented with classes since we came back, with pictures that Mr. Bateson took and Miss Belo took of trance. If you give them Miss Belo's set, they come out with her theory of Bali personality . . . and if you give them Mr. Bateson's pictures, they come out with ours; they are not contradictory theories but tangential ones. Therefore, unless one can place the camera in a fixed position and leave it there and run it for a considerable length of time, you cannot correct for bias by this particular method. There are two other aspects of this correction for bias.

One is that if you take pictures with considerable background depth, in every picture you will have a lot of things that you were not looking for; therefore, if you are focusing on some ceremonial event or piece of behavior in the foreground, these background pictures provide a way of rechecking your hypotheses later. If you are taking a picture of a priest performing in the foreground, you are not taking somebody in a far corner, if you are any kind of a photographer. Therefore, if somebody in the far corner is doing something that you later find is significant, that to a degree is objectively collected data.

The other major point is that we have found that unless you have long sequences of still photographs that you can leave in sequence, place in time, and have absolutely recorded observations by somebody else at the same time, the photographs are less valuable. I have just gone through Collier's series, and he has many beautiful photographs. The only thing he does not know is what is happening while the photograph was taken. The ideal team seems to be one person photographing with stills, one with movies, and one recording, or, if the language is not well understood, one person recording in the native language and one person recording outside it.[5]

MEAD (submitted in writing after session): Dr. Rowe's statement that he omitted all mention of the photographic methods developed in Bali and New Guinea effectively demonstrates the importance of distinguishing between methods used for recording material in the field, methods used for analyzing materials out of the field,

[5] The following contain explicit statements about field methods which involve photography: Gregory Bateson and Margaret Mead, *Balinese Character: A Photographic Analysis* ("New York Academy of Sciences Special Publications" [1942]); Claire Holt, "Analytical Catalog of Collection of Balinese Carvings in the American Museum of Natural History, New York" (unpublished); Margaret Mead, "Researches in Bali, 1936–39," *Transactions of the New York Academy of Sciences*, Ser. II, II (1939), 1–4; Margaret Mead and Frances Cooke Macgregor, *Growth and Culture: A Photographic Study of Balinese Children* (New York: G. P. Putnam's Sons, 1951).

susceptibility of analysis of different sorts of materials, and methods of publication of the materials.

The methods which Dr. Rowe did not even recognize by critical mention may be summarized in the following way:

1. The use of *rapid Leica* (or other 35-mm. camera) photography in *long sequences* in a quantity which differs *in scale* from previous work. (Over 25,000 stills for Bali alone.)

2. The use of still-sequence and ciné-sequence (16-mm. silent black-and-white) photography as *an integral part* of a record which is also recorded by other techniques: pencil and paper, using of timing, use of native informants recording conversation, etc.; multiple points of observation by a team, etc. (22,600 feet of ciné films taken by Gregory Bateson in addition to films by Jane Belo.)

3. The use of the records so collected for purposes of communication with interested members of other disciplines and students preparing to go into the field. An invitation to such inspection was published in 1939.[6]

4. One recorded experiment in such communication between the staff of the Gesell Institute for Child Development and M. Mead and F. Macgregor.[7]

The method assumes that the value of any single still photograph or of any single piece of ciné film is related to the number of other observations which have been made in the same or different recording media, at the same time, immediately following it, of the same personnel in other contexts, the same contexts with other personnel, and over a time interval sufficient partially to rule out such features as seasonal changes or accidents, such as the illness of a member of the team or of a principal participant in some native ceremony (e.g., trance). The method therefore requires that photographs be placed in context; that the time, place, and personnel be accurately specified; that sequences be long enough to test one still in a sequence against the other stills in the same sequence; and that there be long comparative series from the same type of human relations, the same type of ceremony, etc.

The principal device which has been developed to meet these requirements is the placing of all records—notebook, observational (by anthropologist and trained native) stills, and ciné, diary, etc.—in the *same framework of time.* By this method of time-based cataloguing, any single photograph, ciné film, or observation can be immediately related to all the other observations. The still sequences are never cut; hence the negative rolls remain a permanent record of the actual sequence with no chance of error, and the rolls are numbered consecutively from the beginning of the field trip to the end. When a team works together, the times of loading are also recorded.

So far I have discussed methods which in no way implicate the theoretical uses to which the materials are to be put, except the rather basic theory that observations must be placed in time and space and social structure (done through the exact specification of personnel where the social structure has been explored and mapped, so that the name of an individual gives information as to household composition, place of household, caste, etc.), and that methods which yield a large number of simultaneous observations in different media are superior to methods which involve different media at different times.

In regard to use of the records: For purposes of analysis and shared com-

[6] M. Mead, *Methods of Research in Bali and New Guinea* "Transactions of the New York Academy of Sciences," Vol. II, Ser. II, No. 1 (November, 1930).

[7] See Mead and Macgregor, *op. cit.* The Appendix describes the method of co-operation in detail.

munication, the Leica negative rolls were all printed on diapositive rolls, which are replicas of the negative rolls. These can be handled as film strips in a projector and used to make choices among the negatives for particular purposes.

For expository and teaching purposes, the ciné film was duplicated in the case of reversible film (the bulk of the film used) and printed in the case of negative, and for a period of ten years used in conferences, graduate classes, and small conferences with selected individuals.

EVIDENCE

FEJOS (statement revised after session): I fully admit that Dr. Mead emphasizes certain things which are important, but I am afraid their importance is almost nullified if one does not first consider basic propositions in field work and photography.

First, with respect to field work, I would like to bring up the question of "evidence." I came to learn, from reading the anthropological literature of the last ten years, that we have a sort of confusion as to what is evidence. I am afraid we feel that if I bring back photographs from tribe X, then those pictures are evidence of almost anything that I decide to say. Let us suppose that one were to go to the mythical island of Hispaniola and there photograph a native, who would have hanging next to him a coconut rope with a loop in it, and one would photograph the man next to the rope, and in the background would be natives working or laughing. To describe this as "evidence" that the Hispaniola people have a culture which has a basically suicidal tendency, I would not consider valid. I would not do what is frequently done in anthropology—i.e., confuse the concept of evidence with the concept of proof. I looked up the word "evidence" in Webster, and that set me on legal literature. Today I know full well that evidence is not proof.

As to photography, I do not believe that we can arbitrarily lay down rules that one, two, or four photographs are needed, or that 100-, 400-, or 1,000-foot magazines should be used. All this should be elastic to conform to conditions which the field worker may encounter. But I do think that we should set up some sort of standards as to the sort of information the field worker should bring back, how he should make his photographs, and how he should record his sound material. Without such information, scientific evaluation of the evidence is well-nigh impossible. . . .

PROTECTION OF COLLECTIONS IN WARTIME

TERMER: The problem of the protection of museums and collections in modern war is a problem of real concern to anthropologists and one with technological aspects worth considering. Can we, in view of our experiences, devise better means to insure the survival of museum collections and records?

I wish to refer here only to experience in German ethnological museums and mainly to my own in Hamburg. The museums were plunged into the war without any preparation. Memories of the total preservation of the collections in the first World War were still so strong, I suppose, that the boards of management, as well as public administrative bodies, did not care about any preventive provisions. If the managers of the museums made objections, they risked being pilloried as opponents of war and being removed from office, if not being persecuted in a worse manner; so it happened in Hamburg that at the beginning of the war, in 1939, the authorities strictly ordered that all exhibitions should be kept open for visitors. The purpose of this conduct

was of a propaganda nature, to offer diversion and relaxation to the inhabitants of the city. This situation changed suddenly when the serious air attack on Lubeck on the night of Palm Sunday, 1942, destroyed a considerable part of the museum collections there.

The Ethnographical Museum, which was in the same building as the Museum of Natural History, burned down. It was due to the heroism of the curator of the ethnographical department, Miss Margarethe Schmidt, a niece of the late Professor Eduard Hahn, and of the few people of the museum staff who hastened in the same night to their place of employment that about two-thirds of the collections could at least be thrown out of the burning building, but almost all labels of the objects were lost. It was not possible to save the greater part of the African specimens; thus the unique and valuable collections of Tessmann from western Africa were totally destroyed, as were the important archives of negatives which Karutz had brought from his expeditions in western Asia.

Now, suddenly, the cultural department of the state of Hamburg became alarmed. It was ordered generally to remove all collections from the city without any reasonable consideration concerning the technical way this should be done, namely, to what places, in what kind of packing, and by what means of transportation. It must be remembered that, at this time, all raw materials and merchandise had already been seized by the civil and military authorities.

Many times I had reflected what could be done in such an emergency, and I was inclined to retain most of the collections in the museum building, even though this meant running a great risk. Only smaller parts, if any at all, were to be removed to more distant places, and for this I had been able to rent privately in 1941 an unused old factory building in the Harz Mountains near the small village of Lautenthal. Later on, I also privately arranged with the owner of a sixteenth-century castle, in eastern Saxony, to store some objects there. Much more difficult was the procurement on short notice of boxes and other packing materials which could be obtained only indirectly, at best by way of the black market.

The greatest puzzle in such sudden circumstances was to decide which part or which single objects of the collections should be removed. All objects of unique value, such as the Peruvian textiles, the valuable collection of Indonesian batiks and ikats, and the bark clothes of Oceania, were left in Hamburg. The smaller and highly inflammable materials of wood, bark, thread, especially from Oceania, as well as some from Africa and South America, were sent by trucks to Lautenthal, transported in the same boxes in which they were kept in the museum storage-rooms. Also included were selected specimens of pottery and, unfortunately, the greater half of our collections from the Northwest Coast and the Plains Indian, together with specimens from Melanesia and other regions. Although this filled more than eight thousand boxes, it was a small number in comparison to what would have been needed to hold the total collection of about two hundred thousand specimens.

Some time later, in the spring of 1943, we transported by trucks to Saxony about two hundred boxes, containing pottery, some ethnographical objects from Melanesia, and the greater part of the collections of Melanesian skulls brought to Hamburg by the South Seas expedition in 1908–10. By these selective techniques, we had at last removed about one-fifth of the collections, or about forty thousand specimens. We kept the rest in Hamburg.

The most valuable or rare books of the library and all complete series of periodicals, we stored in two big concrete air-raid shelters in two distant city quarters. The finest and most rare ethnographical specimens, as, for example, our two great wooden figures of Nukuor, the Benin Bronzes, the gold collection from Ecuador and Peru, obtained by the late Bruning, the archives of negatives, and all catalogues and field books of expeditions, we stored in one of the strongest safes of a banking house which was constructed 10 meters underground. All featherwork, all furs of polar and Siberian tribes, all the rest, I left in the museum.

This was the disposition of our collection at the time of the intense air attacks of July, 1943, but, fortunately, there was no serious damage to the building until March, 1945, when windows and roof suffered.

The results illustrate what one can expect in wartime. Our Duke of Mecklenberg and the South Sea and African collections of some ten thousand glass-plate negatives, packaged in wax papers and stored in the deepest bank vault in Hamburg, unequipped with air-conditioning machinery, survived in perfect condition. The material in the old factory near Lautenthal was completely destroyed by fire in the last days of the fighting in that area, in April, 1945, the most serious loss to the museum during the war. No precautions had been taken at the time of packing this material against moths and moisture, but it had been periodically and systematically checked by the two custodians in charge. They reported virtually no damage up to the time of the fire.

Wooden plows and bowls stored in an unused subway tunnel in Hamburg survived the war but were used as fuel by the unfortunate displaced population which moved into the area. Incidentally, some of the same people discovered a large zoölogical collection preserved in alcohol and stored in a similar situation, and made a profitable liquor business, to the detriment of the specimens.

With the advance of the Russian troops into Germany in 1944, I sent secretly, not without many difficulties, two of the most reliable technical workers of our staff to Saxony, who, after many adventures, removed most of our boxes from the castle to the town of Riesa on the Elbe, from where they were transported on barges to Holstein. Fifty boxes which had to be left behind in Riesa were discovered casually after the war by a clergyman who had once studied ethnology with Professor Weule in Leipzig, and, aware of the value of the objects, [was able to save them].

Within the museum, damage was primarily due to moisture from rain entering the damaged roof. To the comprehension of the British Military Government and especially to the officer in charge of the museums, who was himself a prehistorian and an expert on museum matters, we owe the basic reconstruction. Thus we were enabled to put up in the winter of 1945–46 a first special exhibit in the one not too heavily damaged hall, where we showed our collections from Arabia in the general outlines of cultural periods in the Near East.

If we compare these experiences in Hamburg with the fate of other ethnographic museums in Germany, it appears that the best precaution in war is the removal of a certain part of the collections to shelters far from the home city of the museum. If there is the danger that the town or city may become the seat of war, the removal of the whole collection to distant provinces can likewise result in their complete preservation or their complete loss, depending upon unforeseeable circumstances. Ethnographic collec-

tions stored in salt mines suffered no damage while in the mines. Similar material, however, when stored in iron mines, where water seepage occurred, suffered. In no case were the conditions in the storage chambers artificially controlled, as was done so successfully in England.

Such controls should avoid the difficulty we experienced with the lettered catalogue numbers on individual specimens. As in many other museums, our method was to inscribe the figures of the inventory directly upon the objects and to varnish the inscription. This did not prove successful, because the figures became illegible through heat or humidity. Attached labels were equally unsatisfactory. If any objects of materials which are menaced by insects, for instance, feathers, textiles, felt, wood, have to be removed, they must be inspected at short periods. Perhaps the use of the newer chemicals, such as DDT, would simplify the problem.

In summarizing these notes, one becomes aware that there cannot be set up any strict regulation in a general manner as to how the collections of museums should be protected in war in our present time. It requires individualized procedure, adjusted to the size of the institutions and of the different collections, the value and raw materials of the objects, the technical possibilities of transportation and preservation, the control of climatic factors, and, last but not least, a well-chosen geographic situation of places where the sheltering magazines have to be built or rented. In any case, it is recommended that only a part of the collections be removed, and those which must be evacuated should be distributed in two or more outside shelters. It is naturally indispensable to make lists of all removed objects in three or four copies and to safeguard these in just as many places.

The microfilm process discussed by Dr. Rowe was not used in Germany for such lists. Certainly, it should be used, as it was in the United States, for copying all inventory or cataloguing records.

A precaution which would facilitate the rapid removal of collections is the one now adopted in Denmark, where a marking system is used on all specimens to indicate their order of importance. Such steps as these can be planned for in time of peace, but in our present situation, when hostilities may begin overnight with air attacks, no one can foretell their effectiveness.

The tremendous work of restoring the museum to its original condition and the restoration of objects means years of labor, but to describe this would require a special section.

BREW: I wish to outline briefly a book at present being prepared for publication by the Museum Division of UNESCO in their Committee on Preservation of Monuments, and so on. This will be a manual for packing and preservation in times of war, which will go into as complete detail as possible on the various types of specimens and handling and also will discuss the construction and the handling of shelters themselves, that is, the types of shelters, and what you should have for certain kinds of material. It also has a section on the philosophy of the subject, as to what things and how many, with a number of different theories on what you should treat, in what manner. We hope to get it out during the next year, that is, next fall or next spring. The plans call for completion of the manuscript this year, but I do not know what stage it is in at the moment.

PROBLEMS CONCERNING THE USE OF TECHNOLOGICAL AIDS

MCCOWN: My purpose is to raise certain general propositions which flow out of what Dr. Fejos said earlier this afternoon. I submit to you that the basic problem that concerns the use of

technological aids in anthropology is that most anthropologists do not ask themselves the question, "Why?" Why they are doing archeology, why they are doing ethnology, and so on. Unless that question has been answered in a fairly clear and comprehensive way, what you do and how you do it obviously is not going to make much sense.

My own experience has been predominately in the fields of archeology and physical anthropology, and in physical anthropology it is this lack of any clear definition of why one is doing anything that has led to what can only be described as "neurosis," as far as the application of particular techniques is concerned.

I think that, while I certainly would not want to abandon measurements, nor would Dr. Washburn, you have clearly had neurotic situations develop in physical anthropology, centering around the problem of measuring for measuring's sake.

What we are interested in here is, with all these technological aids, to make more specific and more clear the basic descriptive facts on which we are making our observations. This is what I think Dr. Fejos would say is evidence. And then it is from that that you build up, through a series of inferences, to something which you submit as proof of certain general propositions, and the general propositions are those that you have initially started with.

Here I suspect there is going to be some disagreement, but I think that part of the problem, as far as anthropologists are concerned, is that a very large number of them are trained in the social sciences or the humanities, where there is a very strong de-emphasis, certainly in our educational system, on the idea of handling things in the way that the natural scientists do. By this, I mean that it is not just a question of their not being laboratory-minded. It goes deeper than that, in that the canons for judging evidence are regarded as being somehow qualitatively different from those applied in the physical sciences.

One possible remedy for this situation is recognition that, whether you are going to be a social anthropologist, a folklorist, or not, there is some value in training in the natural sciences at some level of education. I think that the idea that this can be accomplished in high school is out, when we face the real problem that we have in this country with regard to our present kind of high-school education. High-school training in the natural sciences is clearly insufficient, in terms of the kinds of opportunities that are open to an anthropologist, to get enough familiarity or acquaintance, so that he can assess the relative value of one or another of these various technological aids.

Traditional attitudes affect this, and they also affect this whole question of setting up the initial propositions—in other words, defining the problem.

The second thing is connected with the ideas that Dr. Fejos and Dr. Tanner mentioned about magic. We kid ourselves that, because we have automatic dishwashers and so on in our homes, we are somehow technologically minded as far as our own culture is concerned, but I do not think that is the case. Actually, we regard these things, just as we regard an electron microscope, as being only sort of qualitatively different as far as their magical connotations are concerned; and when you are faced with the practical problems of doing something about your automatic dishwasher when it stops working, you realize your technological limitations.

Perhaps more of the technological aids are seen as pertinent to archeology. I think that is a function of the traditional attitudes that have developed in anthropology. The ethnologists still regard as basic equipment their

notebook and their pencil, and there is resistance to thinking of problems in terms of getting observational evidence of a nature other than that which is taken down in terms of recorded conversations.

To some extent, I think that the difficulties that Dr. Fejos has faced with regard to the use of recording equipment are that such equipment has been regarded merely as substitute for the paper and the pencil, and its opportunities for doing things which paper and pencil deny you have never been clearly envisaged because the problems themselves have not been treated with any degree of imagination.

MINIMUM TECHNOLOGICAL KNOWLEDGE IN ETHNOGRAPHY

ROWE: It is very easy to look at the matter of technological aids in ethnography as an all-or-none proposition and to say, "Well, it is obviously impossible to use all the technological aids that are available; therefore, we can get along without any." The point is that each of the procedures has certain very specific purposes, and it will not be any good for things of another nature. The technological aids that an ethnographer might be interested in using depend on what particular kind of information he is trying to get and how complete a coverage he is trying to get. If an ethnographer is going on an extended field trip where he may be in the same area off and on for several years, he will be able to visualize the collecting of more material and more detailed material than if he is going out for a couple of months.

There has been a certain tendency in ethnographic work recently toward longer and more detailed studies—the study in depth that Professor Nadel mentioned. In such long studies it should be possible for an ethnographer to set aside a certain time to make particular kinds of investigations, which he would not do if he were in the field for only a short time. It should be possible for him to consider, in the case of elaborate equipment, taking one piece into the field for a short time to get the particular kind of information that that piece of equipment is good for.

It might be interesting to try to set up a list of what we would consider the minimum technological knowledge that should be required of ethnographers. I would like to suggest the following, perhaps to start an argument:

First of all, typing, the reason being that it makes possible the production of legible field notes which other people can use, and this assumes that you should make your field notes available for others to use. It also enables you to make carbon copies, which are an insurance against loss.

Second, I would suggest knowledge of how to make very simple maps, with compass and tape—this on the basis that the special relationships of the places and operations with which the people being studied are concerned are important and cannot be made clear to the reader without some sort of graphic presentation. This does not necessarily mean that every prospective ethnographer should take a professional course in surveying, since they go into a great deal more detail than any ethnographer would ever have occasion to use.

Third, I would suggest still photography, and I should think that sufficient competence ought to be demanded for the ethnographer to know what all the various adjustments on the camera were for and to use them in taking black-and-white and color pictures, and so that he knew something about choosing his subjects and composing pictures as well.

Fourth, I should list the use of the wire recorder and not necessarily an elaborate knowledge of how to repair it but at least a knowledge of what a

fuse is for and what you do when it blows.

Either separately or as a subheading under the use of compass and tape, we might mention that it is very valuable to be able to make measured drawings. This would simply mean the ability to work up a sketch map from the measurements that you take with compass and tape, and also to make scale drawings of objects or small areas. The result of applying this sort of simple technological knowledge—and, of course, I have not mentioned any of the more elaborate things, such as movie equipment, which has been discussed before—would be to produce a very considerable volume of evidence outside the ethnographer's head, in the form of notes, in the form of recordings, in the form of photographs, in the form of drawings and maps.

Finally, I would like to suggest that if we are going to do anything besides write literature in the field of ethnology, we have to face the responsibility of putting this evidence in such repositories that it can be used by people who want to check up on our work, so that it will be possible to discuss evidence, instead, as we have done for so long in the ethnological field, of simply discussing opinions and conclusions.

THE PROBLEM OF EMPHASIS

TAX: For the ethnological field worker—and I am sure it is more so in his case than in the case of the archeologist and physical anthropologist—there are some techniques even more important than the ones that involve material objects, for example, psychological tests, which are not included in papers on technological aids but which must also be learned.

Even more important for us to learn, of course, are concepts. The major part of our training must be in theory rather than in techniques. Given the theory, these technical devices are important, but I do not think any of us ought to lose sight of the fact that it is the question that is asked, as Dr. McCown said, that is the important thing, and the technique is only a way of getting an answer. Once the question is clearly in mind, it is relatively easy to figure out ways and means of getting answers or to know that the answers are impossible to get.

I am therefore disturbed, with respect to cultural and social anthropology, when we are asked to put more emphasis in our curriculum on technological aids. I am in favor of them, but worried that our students will lose sight of the relationship between the general knowledge that they must obtain and the importance of being able to ask questions, and some of the techniques for getting answers. The perspective sometimes is wrong. A student wants to take his tape recorder to the field and a motion-picture camera; he goes to get a "complete record." But, of course, he does not have anything when he comes back except the same thing that was there when he went. He has brought some of the field back home. He still has the problem of doing something with the material, which he had in the beginning.

MICROFILMS

TAX: I have one other comment that relates to the microfilm. Some years ago Dr. Caso raised with me the problem of how to present the material that the ethnologist gets in the field. It is economically impossible to present the hundreds or thousands of pages of original field data. The solution appeared in a compromise by which we print relatively short articles and microfilm all the original field notes. This reduces considerably the amount of money spent on publication and, at the same time, makes field notes available to everybody.

However, in beginning a microfilm

collection on Middle American ethnology, I encountered a tradition in ethnology that one does not show other people his notes. This—considering the complexity of the relationship between the interpretation we draw and what it is based on, in the field—is a remarkable tradition for people who want to call themselves scientists. I do not know whether we can ever change this tradition. I do not think I have ever seen anybody's field notes.

MEAD: Have you ever asked to?

TAX: Part of the tradition is that we are ashamed to ask. If I asked you, I suppose you would say, "Well, come down to the museum, and here are file drawers full," and I would not be able to make head or tail out of them and would soon be discouraged.

The technological device here is a relatively simple one, and I hope it will be extended. It is exceedingly cheap; in fact, it costs virtually nothing. It is, very roughly, to organize the field notes and have an index to them, and then microfilm the whole. Libraries, happily, will buy them—the positive copies—and all contribute to the cost of the negative copy, and it costs utterly zero for publication. We have been doing this for some years, and we now have many thousands of pages on Middle American ethnology which are being used by scholars in the field, and if any one of us wants to look at all the original field notes of another, it is possible to do so in the nearest library.

But I find I can get things on microfilm only from people over whom I have some influence as friend or teacher. Otherwise, there are always excuses to be found. "Oh, I'm just about to publish this," or "It's really quite messy," or something of the sort. That is why I have concluded that there is a rather strong tradition against making available to others in the profession—because microfilm does not exactly get on the newsstands—the original material on which we base conclusions.

EVIDENCE AGAIN

TANNER: Perhaps I should say that I was thinking, as Dr. Tax was talking, as a biologist. We were talking at lunch today, and he seemed to think that the biologist is a person who publishes in a *Journal of Anatomy* or the *Journal of Physiology* or something like that, and does so rather differently. But, in point of fact, no person who does publish in those journals, in their very formalized fashion, would dream of really writing up what he did. It would not be cricket at all. Material is written up under certain formalized, conventional headings. It starts off with an introduction, then there is methods and materials, and so on and so forth.

We have, perhaps, a more rigid form of doing that than the social anthropologist. That I would not be competent to judge. But Dr. Tax's suggestion that what you actually did should be available to your fellow-scientists would, I am quite sure, shake the confidence of the natural and biological scientists even more than that of the social scientist, so that you are ahead of us there.

FEJOS (statement revised after session): Dr. Tax mentioned that the student using so much technology would be bringing the field back home. This does not seem such a bad idea. I shot some films in South America among the Yagua, and, of course, also kept a notebook. I honestly believed that my notes were accurate and that there were no major errors. Later at home I had the good luck to screen the film with Dr. Bidney, who was at the time using my notes, and he pointed out, "What did you mean when you said that the proceedings at the tribal council are highly parliamentarian and that only the chief speaks and other people speak only when the chief recog-

nizes them? Look at the picture. There are two people talking at the same time." And not only in that instance, but frequently, two people were talking. Then I realized that the shaman of the clan, my principal informant, was the other person who talked. Since he was the one who arranged things for me all the time, I was accustomed to his speaking at all times, and therefore the fact that he talked at the tribal meeting out of turn, and had the right to do so, had completely escaped me.

It is not such a bad proposition that we should bring the field back home. There is value in doing so. To draw an analogy, I am not an anthropologist by education, but a physician, and I know that if, tomorrow morning, I developed an antitoxin, I would not dare to publish my findings before at least three or four colleagues in other reputable laboratories had checked my titrations and arrived at the same conclusion. On the other hand, for ethnologists it seems that it is not cricket to invade someone else's field. That is to say, if I know that X worked with the Hispaniola tribe, then I do not go there because it is somehow "poor manners."

The question has been raised as to what would be prerequisite qualifications for people who photograph or record evidence. I would like to ask the question: Would you send out someone to the field, or would you take anybody's account as evidence, if you were to find that he was not acquainted with the significance of the terms, let us say, "culture," or "patrilinear," or "taboo" or "mana," or any such terms that we use? Obviously, you would not. Why, then, is an ethnologist using photographic equipment allowed to go into the field not knowing the difference between parallax and distortion? I submit to you that this is just as elementary in physics as is the word "culture" in anthropology. I would say that the qualification . . . would not be a Masters or Ph.D. in physics, but the person should at least know what is involved when the magic button is pushed on a camera and should have an idea of what happens on film.

However, let us not talk now about the anthropologists using technical aids, but about the graduate students . . . presumably ready to write a thesis or go into the field. I found that it took me six solid months of teaching to cover material on photography to which I had planned to devote only four two-hour lectures. This may be partly because I am a poor teacher, but I think most of it is due to the fact that the students were abysmally ignorant of the most elementary concepts in physics. I did not expect them to know that light speed is 300,000 kilometers per second or anything like that, but I did hope for acquaintance with minor propositions in optics, such as what happens when you press a shutter release.

Another difficulty that needs to be overcome has to do with the fundamental attitude which students have toward tools—an attitude which has several facets and which cannot be overcome by simply adding a tool course to a curriculum.

One example: All the students whom I had last year were provided successively with each of the different kinds of cameras that we possess, from the simplest to the most complex and specialized. They were provided with all the film they wanted, which was developed, printed, and enlarged for them. Having heard it stressed continuously that the camera is a scientific tool with a certain purpose for the anthropologist, the boys and girls still brought back during the first four weeks pictures of a cat on the corner looking cute, "my" child looking at some Halloween ceremony, "my" girl-friend in a

bathing suit, and so on. It took almost a half-year of hard work and browbeating to drive home the fact that, for a scientist, photography is not important for its keepsake value. Somehow students pick up during their university years a feeling that tools soil the hands of a scholar, and I fear that, here, we who teach are guilty.

Another example: I went along very unproductively with my students until we arrived at recording. At that point I bought each of them a little radio kit that even a housewife can put together with the help of a soldering iron, and asked them to assemble them. They were surprised, but did it. One of them, a young woman, later said an astonishing thing that truly encouraged me. "Up until now," she said, "I never dared touch anything like this. It looked so awfully complicated." She had soldered a radio set together in possibly an hour and a half, and, when she got the first squeak out of it, the holy feeling of creation entered into her, and I had a student. What I am driving at is that I do not believe one need desperately feel that one is not mechanically minded and cannot handle these things. The most complicated radio set, the most supercomplicated speech stretcher, is not so complicated as kinship, not to speak of linguistics. May I suggest that it is possible that an attempt to spark in the student a desire to adopt, as hobbies, things which he will be able to use later as technological aids may be one way of breaking down this attitude.

I am not asking for abolishment of the notebook and exclusive use of the recorder, but I think the recorder is a very good adjunct to the notebook and a very eloquent one because it tells not only what the informant said but *how* he said it. I have somewhere in my recordings seven versions of the same folk tale, secured from a tribe in the Amazon forest. Although the text is basically the same, the ways of telling it, the emphases on certain propositions in the story, are extremely different—and how different from one another these stories accordingly become! Friends have told me that folk tales are important because we can look for themes of culture in them. Well, among these seven versions, one account reveals hunting as the integrating factor, one indicates ceremony, and so on. Does this not suggest that bringing the field back home so that the comparing of notes is possible is not really a bad idea?

PERSONAL RETICENCES: ADDITIONAL TECHNIQUES

CAUDILL: I want to mention one point which Dr. Fejos raised in part just now, and that is, particularly in ethnology, we relate to people when we go out into the field, and, whether we are interviewing or observing, we all work out our own ways of getting along with the people and come to some understanding about how you do it with a pencil and paper or what you do when you write up your notes, and also, we all influence the situation, whether we are interviewing or observing. We change what other people do by our very presence, in being there.

I would like to consider what happens when you take a recording machine into the field—what you do about the interpersonal relationships between the people you are recording and yourself, whether or not they know you are recording, and all sorts of combinations —also the whole business of knowing yourself that your voice is being recorded. The first time you hear your voice coming back on a recording machine, it is certainly a very strange experience; so certainly you do a tremendous amount of qualitative changing of material when you use a recording

instrument. And certainly, with a camera, too, the interpersonal situation between you and the informant is going to be quite different.

Dr. Fejos rightly stressed that, primarily, one has to know how to use this equipment and how it is put together, but then you also need to teach people how to interview, how to get used to using recording equipment, and how to feel comfortable with it. These are teachable things, but they take quite a bit of time. And certainly, if an anthropologist were to go out into the field, I think he would need a considerable amount of training in how to use a recording instrument in this interpersonal way before it became really truly useful to him.

Dr. Tax's point is the final one—the reluctance of the ethnologist to let other people see his own personal material. Everyone, when he works, has a sort of creative relationship to the objects with which he works, whether these happen to be archeological pots or people, and this means that there grows up a very personal relationship between a person and the sherds or between a person and the people. A lot of this gets into one's basic field data—it has to. Under what circumstances should it be available to one's scientific colleagues?

MEAD: I would like to make three points. One, no one has mentioned that the stenotype is an ideal field instrument if we can alter the way in which the stenotypist is trained so that the stenotypist learns to look and type instead of listen and type. It is a beautiful field technique; it is absolutely quiet, absolutely legible, it can be put away for months, and it is exceedingly portable.

Two, in addition to what Dr. Lounsbury said about linguistics, posture and gesture are just as perishable, and at present we have no vocabulary for recording them. Unless we make moving pictures of posture and gesture, synchronized with speech, we are going to have a large number of human varieties of the same sort disappear.

Third, the photographs brought back from the field, either movies or stills, provide an ideal method of communication across disciplines or to other members of your own discipline. Semantic considerations do not come in. You can put the pictures or the film in front of you, and everyone can look at it and argue about that particular material. Also, you can collaborate with photographs in a way that you cannot collaborate with words. You cannot have an argument about how to put a sentence together. You can have an argument about how to put a whole book of photographs together when you want to juxtapose them. Therefore, getting these chunks of the field back here gives us an opportunity to have a large group of people work over the material and test it independently.

CO-OPERATION FROM OTHER DISCIPLINES

BIDNEY: Dr. Fejos made a comment about not leaving the search for evidence to accident. I am reminded that it was because of a lecture by a geophysicist at one of the supper meetings of the Viking Fund that Mr. de Terra was led to seek the co-operation of geophysicists and later came up with the discovery of Tepexpan man. The point is that more emphasis should also be placed on possible technological co-operation of people in different disciplines for use in planning research, because it has produced results. I wonder whether that has been followed up?

FEJOS: I am afraid, Dr. Bidney, that the instance which you cited is not a fortunate one. The archeological profession, by and large, did not accept Dr. de Terra's findings as a result of the geophysical investigation, as far as

I know, but termed it a strange accident, that just happened at the place where the geophysical plotting pointed. But on the question of how to get co-operation from other disciplines, I think I have an idea. There are a great number of scholars in physics, chemistry, medicine, and many other disciplines who are badly in need of a hobby, and I have found that they are most susceptible to anthropology. One really needs to do little else but sit down with a scholar and talk about anthropology, and usually you have created an amateur anthropologist, ready to co-operate with his specialized knowledge of another discipline. It is interesting that the quality of the material one gets from such co-operation is always very high.

SUMMARY

TANNER: It seems to me that the most important point that has come up is the training of the anthropologist. I, not being a professional anthropologist, have no idea whether anthropologists regard as desirable that all people should have the same sort of training in anthropology, whether they should come in from different fields and have practically no training in anthropology at all (which I know is the opinion of a few people here at least), or, if they come in more or less standard fashion, whether they should come in through the social sciences, so-called, or whether they should come in through the biological sciences. But this is a matter of first importance, and I wonder, since clearly I am entirely incapable of offering any sort of suggestion on this point, if anyone would care to make a few final remarks on this?

LOWIE: If no one else wishes to speak, I shall. . . . The question is, to what extent a course along the lines sketched and outlined by Dr. Rowe should be introduced into our curriculums of anthropology? While I am among the unfortunate people who are not naturally mechanically gifted, nevertheless I am not a complete moron, so that I have even learned to drive an auto, which some of my contemporaries and juniors have not been able to do; in other words, I appreciate very keenly all that has been said on the utilization of technological devices and should be very glad to use them myself if I had a sufficient span of life to qualify myself for their use. I think it is quite appropriate to introduce, in courses on field methods, such instruction as Dr. Rowe has alluded to.

As for the broader question which Dr. Tanner has broached, I should say that, naturally, some attention must be paid to the special phases of anthropology which are to be explored by the particular investigator. No doubt, these technical devices would be of use for a great many things which any ethnologist might be expected to bring back with him. If, on the other hand, your major interest is, let us say, the recording of subjective religious states of mind, then, while the taking of photographs of people in visionary states is auxiliary, obviously it is not in the center of interest.

Similarly, if you are particularly interested in the use of kinship nomenclature, then you will use one of these abstruse devices of which Dr. Fejos professes a dread. You will use the genealogical method, for example, of the late Dr. Rivers. That, however, is simply a question of narrower specialization. The ethnologist, for a good many years to come, I think, will still be expected, in going to a strange region, to record everything that he possibly can of the native culture. We do not have so many ethnographers that we can fail to economize time and energy. Therefore, I think every ethnologist in the future should be

equipped with that elementary knowledge which Dr. Rowe and Dr. Fejos have demanded.

BIRD (submitted in writing after session): This discussion has brought out significant differences of opinion which are probably a fair reflection of general thinking among anthropologists. There may be agreement on principle, perhaps on objectives, but not on methods or the emphasis to be given training in technological aids. The comments should have a leavening influence for all of us. Admit it or not, I believe we will return to our work with a greater awareness that, if our present technological methods can be improved, it may not be so difficult or complicated to do this as we may have imagined.

Unfortunately, there was not time to include a number of specific topics which might have clarified or added to those treated by Rowe. This may have been compensated for by the revealing comments on attitudes toward technological aids and the fact that there are distinctly differing views on their application. As an archeologist I know only too well the necessity of striking a balance in any field program. Once the objectives are clearly formulated, procedure has to be reconciled with time, funds, availability and amount of material, to mention only a few of the many factors which compel a compromise between the ideal and the practical. The situation can well be compared with beating to windward in a sailboat. A skilful sailor can outdistance a less skilful one, even when the latter may have a better-designed or equipped boat. The speed with which one moves through the water is not necessarily a true measure of approach to the objective. What counts is judgment of the problems involved plus full knowledge and utilization of available equipment and the ability to take advantage of each opportunity.

CHAPTER XIII

Cultural/Social Anthropology

Chairmen: Sol Tax and Gerhard Lindblom.

Speakers: Tax, Hoijer, Spoehr, Lowie, Kluckhohn, Bidney, Ackerknecht, Voegelin, Linton, Beals, Lindblom, Koppers, Termer, Birket-Smith, Forde, Lévi-Strauss, Nadel, Dias, Cruxent, Caso, Majumdar, Kroeber, Held, Ishida, Brew, Paul, Murdock, Steward, Hallowell, Greenberg, Eggan, Haas, Monge, McCown, Bates, Newman, Washburn, Kennard, Willey, Bennett, Kidder, Heizer, Clark, Mandelbaum.

SUBDIVISIONS OF THE FIELD

TAX: The following rough classification of problem areas, as understood in the United States, is presented as a basis for discussion:

1. Ethnological culture history
2. Culture process or dynamics, and acculturation
3. Comparative sociology
   a) Durkheimian
   b) Studies of social structure
4. Personality-culture; ethos; etc.
5. Substantive fields
   a) Technology and economics
   b) Social and political organization and law
   c) Religion, mythology, play, life-cycle
6. Areal fields, by continents and smaller units

The first question is how valid my North American colleagues think it.

HOIJER: Since Tylor's time, many of us have made the point that language is a part of culture and that any study of cultural anthropology would include a study of linguistics. Language would be included in the outline under the first, second, and fifth headings. Culture history includes linguistic history, cultural process includes linguistic processes, and language is one of the substantive fields.

SPOEHR: I question the first heading, "ethnological culture history." We are moving toward a closer integration of historical ethnology and archeology, as Kroeber mentioned; therefore, historical ethnology in the old sense is a shrinking field which will be incorporated with archeology and produce a keener appreciation of the field of culture history as a whole.

LOWIE: Would it not be sufficient to have "culture history" as the heading? That would embrace not only prehistory but also the study of higher civilizations.

KLUCKHOHN: What about art and music? Are they technology or play or in the life-cycle?

BIDNEY: What about folklore as a field?

ACKERKNECHT: Medicine is not under substantive fields, either, but I do not think it is one. Dr. Caudill has pleaded that it is, but my colleagues do not like to deal with it because it overlaps *a, b,* and *c,* which makes it unpleasant to handle. But, whether it is a substantive field or not, medicine is a projective test

[1] This session was not scheduled in the original plans for the symposium but during the symposium itself.

of culture, and so its study is important.

LOWIE: A subheading, "science," would include primitive or any kind of medicine and if, in addition, we had "art" under *c*, that aspect of medicine would also be taken care of.

VOEGELIN: I am bothered by the question of which fields can stand by themselves and which cannot, for the moment defining "fields" from the point of view that people specialize in them. Linguistics never does stand by itself; it is always allied with one or another subject, generally with culture.

TAX: Would the following revision then reflect our North American interests in cultural and social anthropology?

1. Culture history, comprising both ethnology and archeology; linguistic history

2. Culture process or dynamics, acculturation; linguistic processes

3. Comparative sociology
   a) Durkheimian
   b) Studies of social structure

4. Personality-culture; ethos; etc.

5. Substantive fields
   a) Technology and economics
   b) Social and political organization and law
   c) Religion, folklore, art, music, science, play, life-cycle
   d) Language

6. Areal fields, by continents and smaller units

LINTON: I would say we covered the field of American anthropology quite well. However, I am struck by the very slight attention paid to what is thought of in the United States as cultural process, that is, the whole field of diffusion, integration, invention, etc.

BEALS (submitted in writing after session): Acculturation is primarily a situational field which offers favorable conditions for the study of process and dynamics. It requires inclusion of a time dimension, on the one hand, and the use of structural-functional concepts, on the other. In so far as illumination of process is concerned, a variety of specialized studies of limited aspects of culture may contribute usefully to the development of theory. However, full understanding of a particular culture in an acculturative situation usually demands a more holistic approach.

Acculturation situations also offer excellent opportunities for the study of culture and personality problems which deserve independent attention as representing different kinds of phenomena. Studies of the role of the individual in initiating or resisting culture change may illuminate the mechanics of particular acculturative situations.

Acculturation must also explain the processes occurring in all the substantive fields mentioned. It must utilize what culture history it can. It may be related to linguistic and personality problems. Finally, it must depend for its validation, at least in part, upon comparative studies.

TAX: Let us now turn to some of our non–North American scholars to see whether this classification means anything to them or what parts of this they utilize.

LINDBLOM: I would like to say a few words about the Swedish meaning of "anthropology" and "ethnology." I think the practice is the same in the other Scandinavian countries, as well as Germany. "Anthropology" is still used to mean physical anthropology. . . .

Sociology is becoming a very important science in Sweden . . . [and is influenced mainly by American sociologists. There is some Continental influence from Max Weber and the German school, but the Durkheimian school is assumed to have no representative today. There is some interest

among the ethnologists concerning sociological problems, but we know little about them. Problems of personality and culture, ethos, and national character are very little heard of among Scandanavian ethnologists.]

KOPPERS (Lowie translating): The German use of terms corresponds to the Swedish. In Basel, in 1933, when the organization of the International Congress of Anthropological and Ethnological Sciences took place, there was an attempt to internationalize the terminology. Anthropology was equated with physical anthropology and related to the natural sciences; ethnology then represented the approach of the *Geisteswissenschaften* or sociological and, perhaps, humanistic disciplines.

Ethnosociology is merely a part of cultural anthropology, while sociology, although related, is nevertheless considered distinct.

Regarding personality studies, in the first volume of *Anthropos*, Father Schmidt called attention to the importance of individual psychological studies, and therefore a certain degree of sympathy with what is being done in this country also exists in Germany.

Ethnology is considered a historical discipline, and the methodology of professional historians must be adopted by the ethnologist.

TERMER (Lowie translating): In Germany current developments in sociology are definitely away from ethnology; for instance, a recent appointee at the University of Hamburg is a disciple of Max Weber and regards ethnological interest as quite alien to his field of studies.

BIRKET-SMITH: In Denmark we emphasize ethnology as the history of culture. I think there is no single science of anthropology in the Anglo-Saxon sense of the word. Rather, your anthropology is a meeting point, as Dr. Linton said, of different sciences which are all concerned with man.

BIRKET-SMITH (submitted in writing after the session): For historical reasons, ethnology in Denmark has been closely connected with archeology and geography. I consider this combination a very happy one. Archeology and ethnology represents two different methods of the same historical approach, and geography supplies an understanding of the physical setting of the peoples. This earth-bound attitude toward ethnology is probably the reason why Danish ethnology feels more inclined to work with concrete problems than to hover in the thin air of philosophy.

FORDE: The content of the outline of field subdivisions does not vary much from subjects which are considered to be in the cultural/social-anthropological field in England. But we would diverge from this scheme in the conceptual arrangement of these items.

We take as our basic category the recording of observed or reported custom, which you might call descriptive monographic ethnography. There might be subheadings under that general category, such as field observation, historical documents, combinations of both, etc. We would then make a distinction between the cultural and the social field, as points of view for analysis. Metaphorically, it can be thought of in terms of a prism; the ethnographic crude data are seen either through the sociological plane of the prism or through the cultural plane of the prism. Cultural analysis includes dynamic cultural processes; problems concerning cultural conditions of invention, diffusion, and so forth; cultural change through time; and analyses with regard to value systems. The sociological analysis need not be elaborated. If any English anthropologist were drawing up a table of the field, I think he would have these major groups: the

basic descriptive ethnography, whose content will be affected by his orientation; then the distinction between synchronic and diachronic studies.

Might I make a further point [about the lack of common terminology] among those working in the cultural-history fields and in the sociological or the socio-anthropological fields. People who are interested in the comparative study of art or in the culture history of peoples in different parts of the world have tended to retain a set of categories with regard to both cultural and social analysis which some of the anthropologists concerned with [social-anthropological] field studies find inadequate for their own, as well as the cultural anthropologist's, purpose.

LÉVI-STRAUSS: Except in my own teaching, where I follow American categories, the use of the terms in France is slightly different from what has been outlined here. Anthropology is limited to physical anthropology. Ethnography is considered a descriptive and geographical study. Ethnology is a systematization and uses mostly the historical approach. Sociology is still linked with philosophy and social philosophy, while new work in social psychology and descriptive sociology, close to the American approach, is being done within it. Thus the sociologists are beginning to have a kind of split personality.

I have two disagreements with the outline which has been presented: (1) Durkheimian sociology and studies on social structure were very far from comparative sociology; (2) I fail to see how play, life-cycle, religion, and mythology can be linked together. Life-cycle is the first approach to the field of art, while religion and mythology are something completely different.

NADEL: Recently, in England, it has become the custom to characterize as "institutional analysis" one of the two possible ways to treat ethnographic material . . . the other being from the point of view of structure. As Professor Lévi-Strauss has pointed out, there is no particular field of structure—it is only a viewpoint of analysis.

Concerning the relation between sociology and anthropology, which is extremely close in England but only with regard to some fields, a distinction ought to be made between sociology from the anthropological point of view, where particular techniques are being used to study more complex civilizations, and the methodological and theoretical implications of sociology, which reflect no boundaries.

DIAS: What Professor Lévi-Strauss said about France more or less holds true in Portugal, in that anthropology is only physical anthropology. Ethnology is the historical evolution of culture. In Portugal and Spain, folklore is now considered the only approach to study of our present-day people, and it is carried out in the same way that ethnologists study primitive people by doing field work in communities and preparing monographs. Of course, we cannot omit history from the study of our own culture or study it in the abstract.

CRUXENT (Tax translating): In Venezuela the study of anthropology has not yet really begun. We recognize as its branches physical anthropology, archeology, ethnology, ethnography, and folklore. These are fragmented in our university. Folklore has a separate institute. Sociology is separate and is a kind of applied anthropology.

CASO (Tax translating): In Mexico we have an institute and a school of anthropology, where the major division is between physical and cultural anthropology. Cultural anthropology is divided into archeology, ethnology, linguistics, and folklore. Sociology is separate in Mexico, and closer to the

faculty of political science than to anthropology. In a way, anthropology studies the Indians of Mexico, sociology the modern people—the Mexicans.

TAX: Apparently, then, anthropology in Latin America as in the United States, and unlike the Continent of Europe, seems to include archeology, ethnology, physical anthropology, linguistics, and folklore. Sociology tends to be a part of and connected more with philosophy and some of the other social sciences. In Mexico, particularly, there is great interest in applied anthropology for the treatment of social problems. . . .

MAJUMDAR: In India, anthropology is an integrated science, divided into physical and cultural. Under physical there are ethnology (the study of races), anthropometry, etc., and under cultural anthropology are technology, prehistoric archeology, and social anthropology, which includes social institutions, culture history, folklore, language, and applied anthropology. . . . Although science students enter physical anthropology while arts students enter cultural anthropology, each must have general background in the other field. In the final stage those who are interested in sociology take two papers in sociology.

LOWIE: I should like to hear from Professor Nadel or Professor Forde as to what extent the use of the term "ethnology," meaning the study of races, is current in England at the present time. In an old book like Keene's *Ethnology*, one will find that particular use to which Professor Majumdar referred.

FORDE: It implies historical study and culture, and does not mean racial history.

KROEBER: Is there anyone in England doing ethnology now, in the British sense of the word?

FORDE: Oh, yes.

KROEBER: Does anybody in England today admit to being an ethnologist, actively practicing ethnology?

FORDE: Miss Blackwood at Oxford is at the general descriptive teaching level, and, with regard to detailed studies, quite a number are developing. For example, in the African field Eva Meyerowitz is working through both the documents and field material for the history of cultural change and social development of the Akan-speaking peoples of West Africa, in the Gulf Coast, and in French territory.

NADEL: Professor Forde has been overoptimistic. There are a few ethnologists, but they are not attached to universities. [They are in museums and special institutes.]

FORDE: In Oxford there is a subdepartment called the Department of Ethnology.

TAX: Most of us regard Daryll Forde as something of an ethnologist.

KROEBER: I was just curious to know whether he would admit to being one.

HELD: . . . In the legal terminology of Holland, ethnology or ethnography is restricted to the primitive cultures, and sociology or sociography is always used for the higher cultures. However, we cannot use that terminology for Indonesia.

ISHIDA (statement revised after session): In Japan the classification has been fluctuating. When anthropology was first introduced into Japan as an academic subject, toward the end of the nineteenth century, the term "anthropology" was used in the Anglo-American sense and comprised the wide field of both physical and cultural anthropology. In the twentieth century, however, perhaps under European influence, it came to be limited in most cases to the more narrow sense of physical anthropology. Since World War II, the influence of American anthropology has grown so much that again the broader sense of the word "anthropology" is prevalent. As the

classification of its field will no doubt follow the American standard generally, I have nothing special to add except that, with us, the folk ethnology of Japan has a more or less independent position from ordinary ethnology (*Vòlkerkunde*). Although this field is still called today by the somewhat unfortunate term "folklore," it covers far wider scope than folklore in English. It approximates rather the German *Volkskunde* and has furnished us with an enormous amount of data on hitherto unrecorded Japanese folk life.

ACKERKNECHT: Fine points are not being noted here. In Germany, ethnology is a humanity and physical anthropology is a natural science, and they hardly speak to each other. In France, the difference is mainly in terminology; under the heading of "ethnology" I studied physical anthropology, prehistory, and cultural anthropology, so the French system is much closer to ours than the German is. Still, there is a real difference of degree between European anthropology and, let us say, United States and English anthropology, the important difference being one of values, that is, the former is far more tradition-minded than the latter. . . . I would add that this is not altogether bad, in this as in medicine and other fields.

BREW: It is only fair to point out that a large number of archeologists in the United States are not classed as anthropologists at all, but may be called archeologists or Egyptologists, etc., and included in the universities under fine arts or classics.

SOCIAL ANTHROPOLOGY AND CULTURAL ANTHROPOLOGY

TAX: The next question is the distinction, as we understand it in the United States and England, between cultural and social anthropology.

LOWIE: Culture being the whole of the social heritage, culture and society are correlative concepts. In the best of possible worlds, cultural and social anthropology would be one. In the existing vale of tears, however, they seem distinct. According to Radcliffe-Brown, social anthropology (in British usage) designates a nonhistorical, comparative discipline in quest of generalizations; by way of contrast, "ethnology" is a historical study indifferent to generalizations. I should formulate the matter differently.

Though once called a social anthropologist by a disciple of Radcliffe-Brown's, I call myself an ethnographer or cultural historian. In contrast to some *soi-disant* social anthropologists, I exclude no socially acquired traits from my potential field of interest. I want to know their distribution in time and space—what, where, and when they are. I do not reject—I veritably crave—generalizations, so far as they are demonstrable and so far as they rise above the level of platitudes. But my interest is not motivated solely by the search for "laws," e.g., I am interested in the varying uses man has found for the reindeer, I am interested in Laufer's discovery that a tribe known to the Chinese had domestic reindeer in A.D. 499. I am interested in Schmidt's earlier theory that the reindeer is the oldest of the larger domestic species and, equally, in his abandonment of that theory. If all the factual material that has accumulated on the reindeer leads to some generalization, I shall be happy; I refuse, however, to be unhappy if such generalization fails to come forth. I am interested in culture, in the whole of it, intransigently and impenitently.

PAUL: I address the problem of the difference in words between social anthropology and cultural anthropology, and wish to illustrate with a simple analogy. In English, the word "day" is used in two senses. It distinguishes a 24-hour unit of time from

other possible units of time—minutes, hours, years, or the like. It also refers to part of itself, as in the distinction between day and night. The term "cultural anthropology" in its relationship to social anthropology plays the same dual role as "day." Where it is significant to distinguish cultural anthropology from physical anthropology, the term, "cultural anthropology," prevails. If it becomes useful to distinguish emphases within cultural anthropology, then cultural anthropology becomes a part of itself as distinguished from social anthropology. Both of them may be generalized, both of them may deal with the cultural plane, but the emphasis in cultural anthropology is historical, concerning time and space.

The argument within the United States [is simply] whether the distinction should be introduced. When a man calls himself a cultural anthropologist, [he may be using the term either in the generic or the special sense]. When he calls himself a social anthropologist, he implies that his center of interest is somewhat narrower. I do not imply that social anthropology is a more benighted subject.

MURDOCK: I think I would get that implication. Social anthropology seems to me to be simply the branch of cultural anthropology that deals with interpersonal relationships. The great disadvantage of drawing the distinction has been to leave the rest of cultural anthropology relatively untilled, as our English colleagues have recently admitted. The great advantage of maintaining the view, which most American anthropologists do, that cultural anthropology, including social anthropology, is all one lies in the fact that we are in a position to look at social structure, for example, as a system moving and ordering through time, and comparisons may be made between moving, changing, adaptive systems.

STEWARD: (submitted in writing after session): I offer the following meaning of "social anthropology," as I used the term in planning and directing the Institute of Social Anthropology of the Smithsonian Institution.

"Social anthropology" differs from "ethnology" more in subject matter than in theory or method, except as the latter follows the former. Ethnology in the United States had meant the study of tribal and pre-Columbian cultures. In order to make government officials, congressmen, and others understand that anthropology could also study contemporary and acculturated populations, I felt a new name was needed and used "social anthropology." I would think of this field as covering acculturated Indians of the Americas, community studies of various peoples, and ethnic minorities. This distinction between ethnology and social anthropology thus differs somewhat from that of England, where basic theory, not subject matter, seems to be involved.

LÉVI-STRAUSS: Man can be defined in two ways: as a tool-making animal or as a social animal. If you consider him as a tool-making animal, you start with tools and go to institutions as tools which make the social relations possible. That is cultural anthropology. If you consider him as a social animal, you start with social relations and reach tools and culture, in the wide sense of the term, as the way in which social relations are maintained. The difference is exclusively one of point of view, and there is no deep difference between the approach of social anthropology and that of cultural anthropology.

HALLOWELL: It is significant that the term "sociocultural system" has not been brought up in this discussion. With it you include social relations or interpersonal relations, as well as something else. There is a feeling here that there is this double aspect.

GREENBERG: A repeatedly raised point is that social anthropology is on

a higher level of abstraction than cultural anthropology because you abstract patterns of social relationships from the actual culture. I would like to point out that it can be done exactly the other way, abstracting the customs and culture patterns from the social interrelations. Thus you could just as well say that culture is on a higher level of abstraction. I am afraid that in the history of American anthropology we have used "culture" without indicating the social structure involved.

NADEL: I do not think it makes any difference whether you call yourself a social anthropologist or a cultural anthropologist. In England, Malinowski, who had the chair in social anthropology, talked about culture all the time, and somebody who may have a chair in cultural anthropology in America may be talking about social relations all the time.

FORDE: May I show how historical detail affects this matter? Malinowski's chair was called social anthropology because Professor Zeligman was professor of ethnography or ethnology and they wanted to make a distinction between the two.

BREW: When a person is appointed to the staff of the Peabody Museum as curator or a research associate, I ask, "Do you want to be called Curator of Siberian Ethnology or Curator of Siberian Cultural Anthropology or Curator of Siberian Social Anthropology?" and he picks the one he wants, and so we have them all.

FORDE: Cultural and social anthropology should form part of a single discipline for many methodological reasons. This does not mean that we do not feel there is a distinction very much along the lines summarized by Lévi-Strauss. We would disagree with Greenberg, in that you can observe pattern behavior—you can record it in photographs and so on—but you cannot observe domination except as expressed in the particular concrete behavior from which you abstracted it, and the same holds true for all abstract categories of interpersonal relations.

TAX: I think the consensus here, with some exceptions, is that we ought to use the words "cultural" and "social" anthropology interchangeably and forget about the question of terminology and deal with the problems involved.

The next question is with respect to each problem area: what cultural anthropologists need from linguists, archeologists, and physical anthropologists. We have already decided that you have to throw linguistics and archeology and physical anthropology right into the pot with culture history.

CULTURAL ANTHROPOLOGY AND LINGUISTICS

FORDE: Are we agreed that culture history connotes a time-space descriptive, narrative study, with rather large segments of both time and space? Whether your last statement is valid depends on this.

TAX: In this context we are thinking of broad cultural history over long periods of time and perhaps wide spaces. In this sense of culture history we have all been in the habit of using all the fields of anthropology as evidences in the whole process. Is that generally accepted?

Now, if we may group questions of culture process—diffusion, invention, acculturation, and the like—are there comments on what we need from other fields or how we are to relate to them?

EGGAN: What is the relation between culture and language in such an area as the Navaho? There is a tremendous amount of assimilation and reinterpretation culturally among the southern Athabascans, but, as far as I know, in the language you have only five or ten or fifteen recognizable loan words. How do you work on the problem of

interpretation of language and culture, and what is the mechanism preserving the language in the face of this cultural assimilation and reinterpretation? Are the cultural and linguistic processes fairly distant or fairly close?

HOIJER: This question is difficult because, on the whole, linguistics has done very little in terms of dynamics, change, or acculturation. Linguists have confined themselves in the historical field to summarizing the results of change and the classifications that result therefrom. . . .

BEALS: A relevant point here is the sort of work that Barker did in Tuscon in showing that the amount and type of Spanish used by Mexicans relates to their social position and degree of assimilation in the community. Here was a very definite linguistic reflection of acculturation.

VOEGELIN: This work of Barker's is not really linguistic. He gives people's preferences for one or another variety of Spanish in Tucson, says people of different social classes speak these, and then he goes to sociology. . . .

TAX: I suppose, with respect both to the broader history and to acculturation, rates of linguistic change would be very important to cultural anthropologists as well as to linguists.

LOWIE: I should like to ask the linguists present whether they can explain why in all of Latin America there has been a great tendency on the part of the Indians to borrow cultural words, i.e., for domesticated animals and plants, while north of the Pueblo area the tendency seems to be in the reverse direction. In the Plains, for instance among the Sioux Indians, the horse was called either "mysterious dog"—in other words "mysterious new domesticated animal"—or "large domesticated animal." Can the linguists explain this diversity of reaction on the part of the different tribes?

HOIJER: No. It is not a new problem and has been approached in areas other than the fields of American Indian languages. I assume Mr. Greenberg has encountered it in his African studies.

HAAS: If you borrow directly the Spanish word for horse, which has been done quite often in North America, that is one thing. But you can also have indirect borrowing. One tribe takes the Spanish word, and another takes the Spanish word from that tribe; or one tribe invents a new descriptive term, and the next tribe borrows that invented term, so it becomes much more complicated. If we had a very careful and detailed study of diffusion, we could begin to answer some of these problems.

CULTURAL AND PHYSICAL ANTHROPOLOGY

LINTON: Cultural anthropologists can get a great deal from what you might call human biology or physiology, for instance, on the question of nutrition and its effect not only on population size but on population vigor, and also the important problem of disease susceptibility. There is still the question of the actual efficiency of different human types in different climatic environments.

FORDE: We badly need to know what the physiological variables are in populations. In the British area two research laboratories, one in West Africa near Lagos and one in Singapore, are making careful studies of both heat loss and fatigue processes among native tropical populations and are also exchanging data with the University of London on Europeans in Africa and Africans in Europe.

MONGE: In Peru we are getting much information by studying stress in different ways, such as at high altitudes, where we find there is an enormous difference from men in the lowlands.

CULTURAL ANTHROPOLOGY AND OTHER DISCIPLINES

FORDE: Although there have been very good studies of ethnobotany and ethnozoölogy, there has been very inadequate integration of habitat conditions as partial determinants of cultural pattern and of social structure. . . . It is very difficult to secure cooperation, and the cultural anthropologist has to be his own geographer and geologist to a very considerable degree.

TERMER (Lowie translating): I would like to emphasize the importance of geography, in correlation with both prehistory and ethnography. Geographically trained ethnologists or prehistorians could advance the subject very considerably.

LINDBLOM: Speaking of geography, at each university in Scandinavia there are professors of physical geography and, recently, of cultural geography.

TAX: In the United States we often have cultural geographers who are fairly closely related to anthropology in some cases.

NADEL: Anthropologists need to go outside their own field, so far as their interest in social structure and so forth goes, and study more thoroughly and systematically both logic and philosophy.

LINTON: Comparative sociology is an area in which demography can be of tremendous importance. Cities are never able to maintain themselves by the ordinary processes of biological reproduction, which means that they will always contain a large number of unintegrated adults. This situation, I suspect, is responsible for certain ideas of the sociological school of Durkheim and Lévy-Bruhl, which laid a great deal of emphasis on the necessity for a society to develop techniques by which it can establish patterns of integration, and a common will, a common understanding, and so forth. This is certainly not necessary in a community which is reproducing itself, where everybody grows up together.

HALLOWELL: In the future it is very likely that whatever psychological interests anthropologists have will be stimulated by psychologists and psychology rather than by psychiatrists and psychoanalysts, as in the past. . . .

WHAT PHYSICAL ANTHROPOLOGISTS WANT

MCCOWN: . . . The cultural anthropologist must realize that, if he formulates his problem too precisely and too definitely, he is often going to make it impossible for another scientist to cooperate with him fully. Where you have interdisciplinary or intra-disciplinary problems, the formulation should start very early as a co-operative project between the cultural anthropologist and the person working in another discipline.

BATES: . . . Many questions we are getting involved with here come up particularly clearly in the question of racial differences. Racial physiology of man, and environment in general, can be tackled only by long-term studies with well-established and strategically located laboratories. If you are going to tie in culture, psychology, and physiology, it cannot be done casually or on an expedition basis. . . .

TAX: So far, what you want of us has been in programmatic terms, rather than in terms of particular research ideas. Are we to assume that the physical anthropologist does not think the cultural anthropologist can discover anything that would be substantially useful?

NEWMAN: Applied physical anthropology deals with definable samples of biological populations, which, in turn, are portions of cultural populations. The cultural definition of the group and the social ramifications of recommendations originating from or implemented

by the applied physical anthropologist are problems that must be handled by the cultural anthropologist. . . . To date, no serious cultural faux pas has been committed by applied physical anthropologists, but the present rate of acceleration in this field, especially in world-wide application, will eventually plunge the physical anthropologists into problems that possess as many cultural as biological pitfalls. . . . One of the most intriguing applications of physical anthropology is to the industrialization of the world . . . and industrialization is not a process which can physically or culturally be taken from the United States and set down in a rural country without serious ramifications. Thus far, the problems have never coincided, but they must in the near future.

In response to the points raised by Dr. Beals and Dr. Linton concerning human physiology and what it can provide for cultural anthropology, the situation actually works exactly in the opposite direction.

I would like to caution those who speak of human physiology that human physiologists are, by training, a very different breed of men from physical anthropologists. They like to hold every variable constant, divorce man from his culture, and study him only as a biological entity in a very narrowly defined experimental problem. . . . It remains for the physical anthropologist to translate their work into population or general biological terms. The next step is to take man and throw in as many cultural variables as are possible. This will have to be done by cultural anthropologists or with their assistance.

The problems of integrating the culture and the biology of man in the free world today are a matter of world-wide political and economic strategy, with millions of dollars per year being spent on them. Thus far, anthropologists have done little on them, but they can and must translate experimental knowledge into terms more usable at the administrative level. In this country, we have been and will continue to be vitally interested in jungle, desert, the Arctic, high altitude, and nutrition. These interests open up a vast new field which can be correlated with cultural anthropology, if the cultural anthropologists will make the advance.

WASHBURN: There is much specific information that the physical anthropologist would like to get from other anthropologists, such as the time, space, and number relations of people in terms of history. How many were there, when were they there, and how often did people move from one village to another? In this case an aerial map, for example, of an area in East Africa, showing where kraals were located, some estimate of the number of people in them, and an estimate of how much people moved from one to another, is exceedingly valuable information.

The breeding structure of the population is also very important to the physical anthropologist. When work was started on the Navaho in New Mexico by Drs. Kluckhohn and Spuhler, etc., it was only by elaborate computations that one could tell how much the breeding structure affected the local genetic situation. Since then, a series of charts, published by Lerner, has been worked out by animal breeders, so that you can, in just a minute or two, tell whether the degree of inbreeding believed to be going on from your knowledge of the social structure is genetically important. Thus the social anthropologist can know whether or not a problem is worth investigating.

In general, there are two classes of problems, where there are relations in both directions. One is historical—the origin of human behavior, etc.—and the other is human biological.

Physical anthropology faces a difficult problem in the latter category, be-

cause it cannot become a miscellaneous biological service center and still be encompassed by a few individuals. So we have the physical anthropologists with their own field of study and also mediating between archeologists, social anthropologists, and members of biological professions. I think it is very important, in terms of human limitations, to keep the function of mediating separate.

KENNARD: I was wondering whether we could get from the human biologist some general criteria as to what are healthy populations in a demographic sense, which would provide us with comparisons to judge conditions under which human populations thrive.

TAX: Is the question whether we can define a healthy population?

KENNARD: Yes; what are the criteria, the human criteria?

NEWMAN: I think there are two approaches. The cultural anthropologist can ask the biologist what a healthy population is, and he can rightly answer, "You show me a culturally healthy population and I will tell you about the biology of it." You can make your criterion either way.

WHAT ARCHEOLOGISTS WANT

WILLEY: Archeology has two large problem fields. One of these is the spatial-temporal arrangements of cultural materials, and the second is what we might call "descriptive integration" or a series of synchronic integrated pictures or the reconstructing of cultural and ecological contexts.

For the first, archeology has the methods and the techniques. It is in the descriptive integration through time that we turn to the ethnologist and social anthropologist, because we will operate here by inference and analogy.

There are two types of analogy. One you might call a "general comparative analogy," in which we are interested in cultures for comparisons, in cultures of the same general level of technological development, perhaps existing under similar environmental situations. Then there is specific historical analogy, where we are on stronger ground for interpretations, provided that we can get information from ethnologists. Here you have, in a single area, a presumed or reasonably demonstrated cultural continuity from prehistoric to historic times, on into a living ethnology in the same area. In the areas of specific information where reconstructions of context are attempted, we need facts bearing upon the use and background of material objects or features.

The first of these areas I have listed as ecology. For instance, how do a particular people exploit an environment? How do they modify it—knowingly or unknowingly?

A second area would be subsistence techniques and their relations to artifacts or features in prehistoric context, social participation in subsistence activities, or cultural elaboration and intergration of subsistence activities.

Third is settlement: houses, construction methods, uses or functions of house types or other buildings, the number of people in a community, general demographic problems, the cultural significance of spatial arrangements of buildings of different types in a community, or the arrangement of one community with relation to another community.

Fourth, technology: methods and techniques of manufacture; uses of specific material culture forms; social divisions of labor—for example, do captive women or women married out of one group into another make pottery? —which would help us in the matter of diffusion of pottery styles; social and class divisions in usages and possession of material; priest class or shamanistic paraphernalia.

Fifth, art: ceremonialism, burials, beliefs in the afterlife. We are interested in the context of art on socio-economic

or sociopolitical levels, as well as in interpretations placed on the particular iconography. How is style or ceremonialism integrated into the culture?

Sixth, we might consider, under problems of development, diffusion, and acculturation, what ethnology could offer to archeology. The ethnologist is hampered by a short-time view, but his observations on acculturation may provide some very helpful analogies to project the present back into the past. However, in many areas, acculturation by means of Western civilization has presented conditions which probably are not analogous to what went on in prehistoric times. Under this, we could look for acceptances or rejections of innovations, as noted by the ethnologist, perhaps in some cases reflecting deep-seated or ancient attitudes of a cultural tradition. How are such innovations made, what aspects of life tend to change more readily than others, etc.? How do ideas spread between communities, tribes, regions? What are the means of diffusion, trade, imitation, exogamous marriage, etc.? For example, in the central northern Andes, we have noted for a long time that technologies in metallurgy and forms in pottery vessels are diffused from northern Colombia to southern Peru. Over the same area, the aesthetic styles in art, used on pottery, metal, or textiles, are extremely limited in their geographical spread. Can the ethnologist help us understand why technology or certain broad types will diffuse much more rapidly and easily than specific art styles? We might even ask what happens to the individual during periods of change or relative stability, if this can be viewed by the ethnologist. This may be personality and culture, but the reconstruction of ancient personality types is interesting to the archeologist.

Seventh, the problem of cultural types and models: for instance, Rouse, some years ago, had a type concept in pottery. He tried to achieve or recapture what was the original model or type in the mind of the prehistoric artist. Perhaps the ethnologist could find out how the craftsman in a particular group felt about a style or type, if he in any way verbalized or conceived of types and realized that the modes of a certain type were changing. Culture types or models also bear on the question of culture units. Did various communities of an area consider themselves politically or culturally linked? In general, how do their concepts of such divisions, on the present time level and political and cultural divisions, check with what we can find from prehistory?

This takes care of a few of the things we would like to know.

TAX: I take it you meant in all cases that the ethnologist would be supplying information about the descendants of the people who left the archeological remains?

WILLEY: As I said, there were two types of analogy, and my examples were largely from what I call specific historical analogy, where presumably there would be descendants, but there is the other type, too.

BENNETT: My friend Charles Walker, at the Institute of Human Relations in New Haven, who is studying General Motors' assembly lines, speaks of a technological environment. Instead of starting, as would an applied anthropologist, from the worker to the machine, he starts from the machine and goes back to the worker. This seems to me quite a significant approach, in so far as an assembly line and other material cultural techniques have characteristics which demand certain types of behavior in order to function. . . . Ethnologists are too limited in studying a single community or tribal culture and must be interested in regional pat-

terns over and above this level, to aid archeologists who are forced to interpret on a much larger scale.

We can say how a certain pyramid on the north coast of Peru was constructed, but we cannot say how the labor was organized. Was it corvée labor, slave labor, hired hands, religiously inspired? And would there be any way of distinguishing? These are ethnological problems. The pyramids are still being built, workers are still being organized in various ways throughout the world. Are there any characteristics that we could use, then, that would distinguish one type from another?

TAX: Are you talking about general knowledge from anywhere that might be applied to particular instances?

BENNETT: Of course, pyramids are not being built, but there is still labor being organized in Peru; such as for road-building, church construction, and other types of things that fall into the ethnological field.

KIDDER: I agree with what both Dr. Willey and Dr. Bennett have said. In many cases, however, archeologists have failed to take advantage of ethnological literature. The archeologist's wish to know the attitude of the individual artist or potter toward his work is beautifully exemplified by Ruth Bunzel's *Pueblo Potter,* a work which I have never seen quoted or discussed in any archeological presentation. . . .

HEIZER (submitted in writing after session): In one sense American archeologists have taken the initiative in dealing with the total culture history of the New World. Ethnologists have largely gone on into other types of inquiry. . . . As the aboriginal ethnology has gotten thin or a thing of the past, the archeologist has come in and done his work. In North America (with the exception of the Southwest, subarctic, and arctic areas) the archeologist is the main field worker. In Oceania, Australia, trans-Saharan Africa, northern Asia, and South America the ethnologist and archeologist may still work together on the common problem, but there are notably few examples of co-operative or team research. I would guess that the existing regional ethnographic coverage in any area of the world is going to be a disappointment to the archeologist, because the information he seeks is either stated too generally, was not thought to be of sufficient interest to justify recordings, or was simply ignored.

Let me illustrate this point with reference to the field I know best—California. Here the ethnographic description is completed in so far as possible. The archeological data are fairly full, as regards both geographical coverage and regional time sequences. We should now start combining all existing data into a reconstruction of California culture history. The broad picture is one of a parallel series of prehistoric culture sequences which reach back perhaps eight millennia into the past and are joined with the ethnographic present, with its diverse tribes, languages, physical types, and regional culture subpatterns. Occupation has apparently been continuous, so we may safely infer that the ethnographic situation is the end-product of a long and complicated history.

Physical anthropology may assist in the identification of the bearers of the cultures. On the recent level there are four or five recognizable physical types. Identification of these types in prehistoric times has not yet been attempted.

Language classification is a good reference point on the recent level. The variety of stocks and their distribution imply a complex history. The central and compact distribution of the five Penutian tribes indicates them to be more recent than the Hokan-speaking peoples, who are scattered discontinu-

ously all over the state. We need a decision on genetic relationships of the California Indian languages. . . .

Ethnographic cultural data can guide the interpretation of the latest archeological materials and should guide interpretation into the past. The ethnologically documented settlement pattern, daily and annual routine of life, demography, intervillage communications, trade and commerce, economic exploitation of the environment, and other aspects of culture may be inferable, at least in broad outline, using the one known recent example as a guide. It would greatly aid archeologists if ethnographers would always record the *exact* location of villages and give details on economy and such matters.

CLARK: The problem of India, with its longer history, differs from that of the New World, where it is much easier to get a direct continuity from archeology to cultural ethnology. . . . In Europe and parts of the Near East, one has to have recourse to survivals, very often of marginal areas, possibly in the far north or in Alpine valleys. But nearly all the peasant communities which survive in those regions are influenced by the cultural values and techniques of the more advanced historical civilizations in the towns. When studying the older Stone Age, there are no parallels in the area, and one is forced to go outside, thereby forfeiting historical continuity. Then, too, one has to make great allowances for varying ecological backgrounds and so on.

FORDE: I would indorse what Dr. Kidder said and give another example. There is much ethnographic literature now with regard to the manpower required for clearing bush in heavily vegetated areas by tool techniques of different kinds, and there is a good deal of information with regard to the demographic conditions which result in tiny forest clearings and in a system which we call, in the West Africa area, "block farming." If you have a rough idea of your population and the basis of subsistence, you may get some information as to the scale of labor organization. And if, by aerial mapping of destroyed vegetation, you can work out the areas of cultivation with a scale, you may infer the upper and lower limits of the population.

EGGAN: If the archeologist can give us more detailed information on local organization and make estimates of population size, perhaps he can, with the aid of the physical anthropologist, tell us something about the subsistence patterns. Then we can apply some of these models to Julian Steward's excellent series for the Southwest, where he refers to band organizations and their limits under particular ecologies. Also there is Murdock's larger-scale analysis of the limited types of structures in the world. We must also tie that up with ecological patterns.

In the Pueblos, I believe we can ultimately demonstrate some hypotheses with regard to the matrilinear character of Anasazi institutions, or particularly their matrilocal character. I would guess that the Hohokam were patrilinear, and the preliminary data for Mogollon suggest patrilinear or a mixed bilateral organization. In this way we can put together models with regard to social structure and project them back to the archeological data, not with a hundred per cent accuracy but with a good deal of confidence.

WHAT LINGUISTS WANT

GREENBERG: . . . The matter of public relations of linguists is very important. Ethnologists believe linguistics to be some kind of sacred mystery. Specialization is good and necessary, but in this respect it has gone too far. . . .

One indication of this: What does an ethnologist most often come to me about? He wants an alphabet for writ-

ing kinship and ethnological terms in a phonetic or phonemic orthography, . . . Without realizing it, he is practicing a scientific fraud by doing this. There are three levels of sophistication in languages. The first level is no transcription at all, which is the naive reaction of a person who has no linguistic knowledge and no phonetic training. Let us say that a district officer in Australia compiles a vocabulary. For the word "bone," he writes *f-i-d-g-e*. Being naïve, that is the way it sounded to him. An ethnologist, who knows something about linguistics but nothing about phonetics, goes to the same place. Let us assume this was not a labiodental *f* but a bilabial fricative *ɸ*, and this was not a *J* in English but a mid-palatal voice-stop ɟ. So the ethnologist writes it as "fij" when he hears it as "fidge" in English. Aside from giving us no information, he has created the false expectation that perhaps "fij" was really there, when actually it was something else.

Since most ethnologists are not expected to know much linguistics, they should not be afraid to give to the linguist the information that he wants. There are very few linguists and many, many languages. Hence for certain problems any unrecorded information about the language of any people has value. A simple, naively recorded (but so noted), vocabulary may help in genetic classification of the languages, or those of language divergence involving counts. Better still, every ethnologist should have a basic knowledge of phonetics and phonetic transcription. . . .

On more long-term problems, the more the ethnologist comes to the linguist to find out certain things, the better it is for the linguist, because by this development of co-operation, he begins to see what he, in turn, can expect from the ethnologist.

I am worried about certain very important fields which the linguist says are not linguistics but which the ethnologist is likely to neglect also. For example, the whole problem of semantics requires a good knowledge of the cultural background as well as a basic knowledge of the structure of the language. Greatly neglected also is the relationship between language and social structure. . . . That is of interest to the linguist, for, unless he wants his data to be uninterpreted in the historical and spatial sense, he will begin to ask why, in native California, there are a great many languages spoken by small populations and why a certain language has spread over very large areas, and questions of that kind.

I have a hunch that eventually there may grow up a separate field. Ethnolinguistics some day will be a science in which a person will have the not impossible techniques of the linguist together with an interest in those problems which lie on the border line between language and culture.

HEIZER: Regarding linguistic classification and its relations to archeology, as Grahame Clark just mentioned, later archeology, particularly in the New World, can be tied directly to the historic tribes and therefore to languages. Language would therefore furnish a very excellent and direct frame of reference from which to start the projection back in time of the culture, provided that we can be sure that the linguistic classification is correct.

GENERALIZATION AND ABSTRACTION

MANDELBAUM: . . . There is a distinction which several have made, Professor Lévi-Strauss especially, in dividing us according to our interest in man as a tool-using or as an institution-using animal.

A distinction which cuts across that is between those who do descriptive integration and those who are interested in establishing valid generaliza-

tions about human behavior. We need not split our personalities, for the archeologist, linguist, or ethnologist may wish to use one approach for a specific set of problems and then shift his role or his position in the next set of problems which he tackles—which I believe to be all to the good.

An example might be when Professors Willey and Bennett say that what they want from the ethnologist, for the most part, are certain generalizations. Archeologists make reasonable assumptions about human behavior. In making descriptive generalizations, they use as rules of thumb whatever generalizations and assumptions we have. So they ask the people who are interested in generalization to establish generalizations or to indicate probabilities. This we certainly ought to do for the archeologist.

Conversely, the person who is interested at a particular moment, or generally, in establishing formulations, generalizations about human behavior, must perforce call on the data of human history and understand process through time. So I see no necessary antithesis between the two kinds of people, rather a differing and valid stress on different kinds of problems, dealing with the same phenomena.

KLUCKHOHN: . . . This is a point for the constitutional anthropologists. From my experience in giving doctoral examinations, I think there may be a deep-seated temperamental difference between [the generalizer, on the one hand, and] the kind of candidate of whom the archeologists are most typical, who has a vivid sense for the concrete and resists abstraction. It is not so much a contrast between generalizers and the nongeneralizers as it is between the degree to which these two types of temperaments are prepared to abstract. . . .

CHAPTER XIV

Archeology

Chairmen: Hallam L. Movius, Jr., and Alfonso Caso.
Speakers: Movius, Bates, Oakley, Linton, Rowe, Steward, Lévi-Strauss, Mead, Brew, Senyurek, Clark, Nadel, Beals, Heizer, Caso, Mandelbaum, Kluckhohn, Redfield, Straus, Lowie, Krieger, Willey, Murdock, Kidder.

THE ECOLOGICAL APPROACH

MOVIUS: I think it is the sense of this symposium that anthropology is a focal point for other sciences and that the successful integration of these sciences should be the major objective of all anthropologists. But anthropology within itself must integrate its several disciplines or subfields. It is with this end in view that our program for this afternoon has been drawn up. . . . We are going to try to deal with broad aspects of archeology [and with their interrelationships. We will begin with a] consideration of ecology.

BATES: I was interested when, in one of the earlier sessions, someone remarked that we are still living in the Pleistocene, and I have been thinking about the implications of that ever since. . . . It seems to me to give us a slightly different orientation toward many things. Our today's geography becomes one aspect of Pleistocene geography, and, if we are concerned with Pleistocene man, we do not make a nomenclatorial jump from Holocene to Pleistocene but start working backward from the present and seeing changes of pattern.

I think it is Sauer's definition of geography that it concerns events in space as history concerns events in time. What we need is a three-dimensional aspect in thinking, I suppose, so that we can look at the world as it is today and then work back over its processes of change; but at any given time you also have the dimensions as you have them today, that is, you have people who have to be living in some climatic environment, dealing with some source of material which constitutes their resources, and acting in social organizations.

Then, of course, you have the tremendous problem of detective work, of how you can reconstruct the various pieces of this ecological picture at different times and different places. This is going to, and does, involve all sorts of people, but it has already given us a marvelous picture. I am lost in admiration for the way that paleontologists can build up a Carboniferous landscape out of a fragment of a femur or something; and human paleontologists seem to achieve much the same thing, although sometimes, perhaps, we can reconstruct our landscapes more carefully. . . .

MOVIUS: . . . Dr. Bates's [remarks point up] the necessity for reconstructing regional maps going back through time, as a basis for interpreting the possible patterns into which now extinct cultures once fitted.

AN ECOLOGICAL INTERPRETATION OF THE EMERGENCE OF CULTURE

OAKLEY (statement revised after session): . . . Tools and weapons may be

regarded as detachable additions to the body, supplementing mainly the functions of hands and feet. So long as our early Tertiary ancestors lived in trees, they had little need to use external objects as functional extensions of limbs. But as soon as they began to spend much of their time walking or sitting on open ground, their hands became free to handle objects, perhaps first out of idle curiosity, later to some purpose.

Baboons, which are ground-dwelling monkeys, sometimes use pebbles to kill scorpions, and, if followed, they will sometimes scamper up a hillside and dislodge stones or roll boulders down the slope to deter pursuers. Observations on anthropoid apes in captivity have shown that emancipation from tree life offers wider scope for their latent intelligence. Chimpanzees, not known to be tool-users in their forest environment, make use of sticks for various purposes as soon as captivity forces them to spend much of their time on the ground.

On the evidence of the discoveries in the Early Miocene deposits in East Africa, it appears probable that the earliest Hominidae became differentiated from the Pongidae during the later part of the Miocene period. It is possible that, in so far as they became adapted to life spent partly on the ground, they would have been capable of using improvised tools and weapons after the manner of baboons or of chimpanzees in captivity, whenever circumstances demanded.

The need and the opportunity for such activities would have been much more frequent where the biotope was open country (for here life was much more precarious) than in the protection of the forests. All this would have reacted favorably on the evolution of the brain. Hominidae may have remained at this stage of occasional tool-using for millions of years. The systematic making of tools would not have followed until the brain had attained a sufficient complexity of organization, and even then it is not likely to have become a regular practice until new habits of life were demanded by drastic changes of environment. We believe these earliest Hominidae were generalized creatures, capable of living in woodland and in partly open country.

Presumably before the dawn of the Pleistocene, nearly a million years ago, the typically human level of cerebral development had been reached. Undoubted stone tools of standard types are known from late Villafranchian deposits in Africa. These finds show that the manufacture of tools was no longer merely occasional but served some permanent needs of the earliest men. We need no longer apologize for using the word "men."

What were these needs? It was probably through the use of tools and weapons that the Hominidae managed to survive in open country. Apes of today are forest creatures, subsisting mainly on fruits, leaves, shoots, and insects; occasionally, if they manage to kill a bird, they are pleased, but they are, broadly speaking, fruit- and plant- and insect-eating creatures.

All known races of man include a substantial percentage of flesh in their diet, unless, for some secondary cultural reason, they have become vegetarians. There is ample evidence that most, if not all, early Paleolithic men of whom we have knowledge were hunters. Meat-eating is, one might say, as old as man. It is probable that any Pliocene hominids living in country like the African savannah would have become addicted to flesh-eating in times of drought; and I make this suggestion remembering the analogy of the baboons in South Africa, which, in times of drought particularly, prey on lambs and other animals of similar size, using their powerful canine teeth as offensive weapons and acting in bands.

In men the front teeth, as we have already remarked in a previous session, had become reduced before men were regular tool-makers, and this was probably connected with the extensive use of their hands, which became possible through bipedalism, long before they made tools.

Although early Hominidae lacked teeth suited to their carnivorous habits, they would have been at least as ingenious as baboons in killing small mammals. Life in the open set a premium not only on cunning but also on co-operation, and the proto-men were probably sufficiently socialized to hunt in packs, cornering their prey, and using improvised hand weapons, such as throwing-stones and the like. Although the killing of small game may have been relatively easily accomplished by these proto-men, they would have encountered considerable difficulty in removing skin and dividing the flesh, and, in the absence of strong front teeth, the difficulty would have been overcome most easily by picking up sharp pieces of stone.

Here, surely, lies the beginning of the tradition of tool-making, because, where no naturally sharp pieces of stone lay ready to hand, some of the more intelligent individuals would have seen that the solution was to break pebbles, to produce fresh edges. Perhaps the throwing of stones as missiles occasionally resulted in accidental breaks, and the intelligent proto-men may have observed these occurrences and remembered the sharp edges which resulted.

Now, once this tradition of tool-making had begun, the manifold uses of chipped stones became obvious. They were useful for sharpening sticks, for digging out burrowing mammals or grubs, for making spears sharp enough to be effective weapons in hunting the larger game animals, for scraping meat from bones, and so on. All the main uses of stone tools were probably connected in the first place with the adoption of carnivorous habits.

From the endowment of nature, we should be vegetarians. We lack the teeth evolved by true carnivores, and we have the long gut associated with a herbivorous diet. Furthermore, we are the only members of the Hominoidea accustomed to eating meat on any considerable scale.

We can well imagine that a changing environment, for instance, after a period of desiccation, would have produced an abnormal appetite in the early hominids. Gorillas in captivity quickly develop a liking for meat, and this appears to be due to a change in the flora and fauna of their intestines. Normally, their intestines are richly supplied with ciliate Protozoa, which serve to digest cellulose; but under the abnormal environmental conditions of captivity these Infusoria are ingested, and, with their disappearance from the intestine, the animal develops an abnormal appetite and readily takes to eating meat or may even prefer meat to its normal fare. (That is based on a work of the German primatologist, Reichenow.)

By widening their diet and becoming tool-makers, the Hominidae vastly increased their adaptability. The change from herbivorous to semi-carnivorous habits resulted in their acquiring a new potential. To obtain a given amount of energy, a carnivore needs a smaller quantity of food than a herbivore, but its way of life is very different from the point of view of economy of energy. Instead of eating almost continuously, like their fruit- and plant-eating ancestors, the earliest men must have spent most of their daytime in hunting.

This activity increased interdependence and led to the formation of larger social groups. New skills and aptitudes were thus developed, through which man was able not only to survive

climatic changes but even to create the environment itself. And that is really, I think, where I should stop, or I shall find myself attempting to deal with the whole emergent: human culture.

LINTON: Have we any evidence, actually, that the proto-hominoids were more exclusively carnivorous than modern primitives? At the food-gathering level, we are practically all mainly herbivorous or fruitivorous. There are very few places in the world where people live entirely by hunting, outside the Arctic Circle. You may remember that the occupation layers of Peking Man at Choukoutien have hackberry seeds.

OAKLEY: Yes, but I do not think meat bones are lacking. They are there in very considerable quantities.

LINTON: Can you estimate how much meat these people were actually getting in their diet, though? Most food-gatherers rely very heavily on vegetable foods.

OAKLEY: The Australian aborigine, so Donald Thompson tells me, is largely herbivorous perforce, but by choice *he is a meat-eater.*

LINTON: So are all humans, aside from the aboriginals.

OAKLEY: Certainly the idea that Paleolithic man was mainly a vegetarian is quite contrary to all the actual site evidence. Accumulations of meat bones are very conspicuous in Paleolithic cave sites, from Choukoutien onward. On all the Acheulean camping sites at Olorgesalie in East Africa there were quantities of broken meat bones. Game was evidently extremely abundant under pluvial conditions—great herds of antelopes and zebra and so on —so that the Acheuleans, even with relatively simple methods, became very successful hunters. Some of our marginal food-gathering peoples of the present day are not so successful, but perhaps that is why they are marginal.

ROWE: There are several other cultures besides our own in which hunting has a very remarkable amount of social prestige as compared to other subsistence activities. Not infrequently people who actually get a good deal of their subsistence from fishing and farming nevertheless regard themselves primarily as great hunters. . . .

STEWARD: Along this line, I was going to point to a problem we have encountered among the tropical forest peoples in South America, where it appears that the population might have been very much greater, in terms of the amount of land available for agriculture, except that the men do not like to farm. They spend an enormous amount of time hunting, apparently at the expense of farming. Now, why did they do this? Wagley seems inclined to one point of view, that cultural value was set on hunting. I incline to the other point of view, that there were dietary deficiencies in the predominantly starchy diet afforded by manioc and other foods and that the amount of fish the Indians got was not sufficient; the practice of earth-eating is possible evidence of a deficiency. I do not think we can resolve this without dietary studies. There is now a party in the field which we hope will get some of the plants and have them analyzed. . . .

LÉVI-STRAUSS: One point in support of Dr. Steward's theory is that in South America we have not only one dichotomy between the eating of meat and vegetable food but another, which has been generally overlooked, between boiled and broiled meat. Whenever we are facing a tribe which has both, there is no doubt that boiled meat is considered to be superior to broiled. Broiled meat is a delicacy, but boiled meat is food. I think there is strong reason to believe that this has a dietetic basis and that boiled food preserves much more substance than broiled.

MEAD: There is a certain amount of evidence, based on the study of food

habits today, that you can expect a relationship between the insufficiency of the diet and the amount of rigidity with which it is held. . . . For instance, among our sharecroppers in the South, we find that the parents insist, with a great deal of violence, that children who dislike fat learn to eat fatback; and when they moved north during the war and were given beefsteak as war workers, . . . they still resisted it for a very long time. It might be worth while considering that whenever you find a very rigid diet for a very long time, it may be due to a limitation of dietary sources. It is possible that this is responsible for some of the South American rigidities.

MOVIUS: It seems to me that here is a big opportunity for interdisciplinary approach to prehistoric situations.

BREW: I would like to introduce another one of these horrible questions of definition. I have just concluded a course in primitive technology. I noticed a certain amount of confusion on the examinations concerning the meaning of the term "food-gathering," between the concept of food-gatherers who are only vegetable eaters, i.e., the acorn and berry pickers, and that used by Forde and by a good many of us to mean fishing and hunting as well as collecting vegetable foods.

As far as I could tell from the discussion so far, there is no evidence of what you might call a vegetarian culture. The Australians have already been said to eat flesh. Certainly, the Bushmen eat meat whenever they can. I think it is general practice to apply the term "food-gathering" to the whole pre-agricultural complex, but every once in a while, in a discussion like this, we start thinking of food-gatherers as people who live on acorns, etc.

SENYUREK (statement revised after session): I think that the study of the teeth may throw some light on the diet of early man. With the exception of the seeds . . . found at Choukoutien, we do not have any information about the plant food of the early fossil hominids. . . .

It is known that the teeth of modern primitive peoples are usually strongly worn, and this is generally attributed to the grit contained in food, especially plant food dug up from the ground. The teeth of early fossil hominids are also usually strongly worn, the attrition appearing quite early. This may suggest that, in addition to meat, they consumed plant food, such as roots, etc., dug up from the ground.

I agree with Dr. Oakley that the evolution of posture and teeth preceded that of the brain in human evolution. By studying the geology, climate, fauna, flora, etc., of late Tertiary and Pleistocene times in Asia, Africa, and Europe, we can throw a lot of light on the evolution of man.

CLARK: . . . It is well known that bones and other remains of animal food are much more likely than plant foods to survive in archeological sites. . . . It is only under very special conditions, for which I think one ought to look, that one hopes to find indications of seeds and vegetable remains.

In view of the antiquity of the sites about which Dr. Oakley was talking, I think it is possible that the hunting aspect does bulk rather larger in our minds, for the very simple reason that it is evidence of this aspect which has survived. But I do not think anything that has been said really disposes of Dr. Oakley's main point, that, however important the role of plant food among very primitive peoples living today, or recently, or even during prehistoric times may have been, you do seem to have had the addition of meat in certain areas, particularly during the periods of which he is speaking.

NADEL: May I steer away from the very luring subject of food and talk about something more general in Dr.

Oakley's paper? . . . I would like to make sure, first, that I understood him correctly when he discussed the interrelation between the development of the intellect and the changing environment. Were you thinking of these as independent evolutions, or do you believe that change of environment had something to do with the evolution of intellectual faculties?

OAKLEY: This is a question of selective value. The individuals who were more intelligent in that environment would have been the ones most likely to survive, because life is very precarious in the open, and primates are extraordinarily defenseless creatures.

NADEL: I was going to emphasize the other aspect, because there is some evidence, although not very conclusive, that intelligence increases with changes in environmental features and decreases in a stable environment. . . . If one takes the definition of intelligence which is widely held, that it is the ability to extract resources from the environment, then there seems to be some stimulation of this abstractive faculty with changes in environment. Intelligence tests show a marked difference between children brought up in the relatively stable, uniform rural environment as against children brought up in an urban environment. . . . An even more striking difference has recently been discovered [in studying] . . . the general decline in intelligence which sets in at about the age of thirty to thirty-five. It was found that the decline is greater among individuals who have followed the same profession and never changed their environment, always staying in the same place, as against those who have moved about a great deal and have changed their occupations frequently. Obviously, this is not a rigid, deterministic influence, but I think there may be something in change of environment which reacts on intellectual faculties. Of course, you have constant cross-influences.

ECOLOGY AND THE SIZE OF POPULATION

MOVIUS: Population pressure may have been one of the factors making for changes in environmental pattern, by forcing people into marginal areas or regions of unfavorable environment, where there was an insufficient supply of eatable vegetables or game. It is very hard to conceive, but nevertheless true, that the people living at Kostienki in South Russia subsisted almost entirely on the Arctic hare, while the people just to the south were eating big, fat, juicy mammoths. The question of population size may be involved here, but it is a problem for archeologists to know how to get at it. Certainly, this is another aspect of reconstructing man's past experiences on the basis of ecological studies. . . .

CLARK: We are all agreed, I think, that this question of prehistoric demography is very important. It is important, I imagine, from a genetic point of view; it is certainly very important from the point of view of trying to understand cultural history. But, like many important things, it is a very difficult matter, and we ought not to underestimate the difficulties of our task. . . .

It is striking, I think, that if you discuss population trends during the historical period in Europe, you will find it is a subject to which very little attention has been paid, partly because it is so difficult and so controversial. I have even heard people wrangling about the population of Ireland during the eighteenth century, for example. It is not until you get an accurate census that you can really have any certitude.

If you go back as far as the Roman Empire, in a province like that of Britain, you find extremely permanent structures. You can count the number of villas pretty accurately. You can also

count the public facilities, the theaters, the number of seats in them, and so forth. And yet the leading authorities cannot get closer to agreement than between 500,000 and 1,500,000 for the population of this Roman province. . . .

When we come to try to estimate the prehistoric population of an area, we find ourselves in an extremely difficult position. The importance of this problem and, at the same time, the possibility of its solution rest precisely in the close interconnection between population density, . . . modes of subsistence, and technological equipment. In the prevailing darkness, the prehistorian naturally looks [to the ethnologist for information on this interconnection].

Obviously, there are certain safeguards we can take when we are trying to use ethnological data. We must turn to groups at a comparable level of technology, practicing a similar form of economy, and living as far as possible under similar ecological conditions. But, however careful one is, one has to remember, in the first place, that all analogies . . . are very approximate and to a large extent subjective. Second, when one examines the study of population among modern primitive peoples, one finds a situation which, at any rate in the earlier literature, is not quite satisfactory.

For an example I shall turn to a man called Krzywicki,[1] who wrote a very interesting book on vital statistics of primitive peoples, and shall consider his figures on Tasmania. The original Tasmanians lived more or less under Lower Paleolithic conditions, in a climate which must have resembled fairly closely, say, those of the interglacial or postglacial periods in temperate Europe. If we could discover the density of population in Tasmania at the time of discovery, we should have some sort of basis for calculating the size of population, say, for interglacial France or Britain. But Krzywicki's estimates range from 1,000 to 20,000, and the average is about 4,000. Even that figure, of course . . . gives us a sort of order of magnitude, though there is a very great range in it.

Now, to become a little more specific, Professor Forde pointed out how much prehistorians could learn from the evidence which is now being collected in more detail by cultural anthropologists about (1) the basis of subsistence, the number of people who could be supported over a given area by the various forms of economic activity, and (2) the numbers of people involved in certain types of construction, the number of man-hours required to build such-and-such a type of tool or such-and-such a type of building. But, in the last resort, the prehistoric archeologist has to practice his profession. . . . One of the ways in which he tries to establish the size of population is by mapping all the traces of early settlement onto physical maps which depict altitude, soils, and so on.

A considerable amount of work has been done, particularly in Europe, in plotting a series of distributions on the same physical base map. If you plot distributions for successive time periods, you get some sort of picture of the extension of the area of settlement and also of the progress made in taming the environment. You get technological development; for example, the introduction of better types of cutting tools. You find larger areas of forest cut down, more difficult soils cultivated, etc., and all that, of course, reflects the increase of population.

Where you are dealing with farmers who leave behind recognizable traces of fields, possibly under favorable conditions, you can make highly detailed maps, using air photographs, etc. . . .

[1] Ludwik Krzywicki, *Primitive Society and Its Vital Statistics* (London, 1934).

Then, with knowledge of the yields which one can obtain by practicing that type of agriculture, you might make some sort of calculation about the number of people who could be supported on an agricultural basis, taking into account the extent to which hunting, fishing, gathering, and herding were carried on by the community.

Mapping of sites, if it is accurately carried out, allows you to assess the number of settlements and cemeteries. Of course, one has to have a check here. A good deal of these sites are masked by recent land utilization. Also, one has to test the age of each site; otherwise one cannot be certain that they were contemporaneous.

When you have the number of sites occupied at any particular time, there comes the final point of excavating selected examples. In the case of cemeteries, it should be possible, if one can establish the time over which a cemetery has been used, to calculate on actuarial lines the size of the population group which used that cemetery. Here, of course, one has to make certain assumptions, for example, that all people who died were, in fact, buried in such a way that you get archeological evidence for their burial, either cremation or inhumation. . . . It is also important to know whether one or more villages have used an individual cemetery.

If, in excavating settlements, one can count the number of houses, of rooms, and, even more important, of hearths, one should be able to calculate the number of family groups and, by reference to modern peoples living under similar conditions, to form some sort of idea of the number of people who occupied the site. There are certain correlates to this. Obviously, one should excavate sites totally, as many German archeologists have been doing of late, in order to get at the total social group represented. If this is impractical, one must at least establish the dimensions of the site and then excavate a known, defined proportion of it.

When one is dealing with peoples who leave no structures behind them, it is necessary to measure the area of occupation, and one should certainly count the number of hearths, for example, in the caves and rock shelters of France. . . .

The final thing to be remembered is that in prehistory we are dealing with extremely tenuous, extremely vestigial evidence; and therefore I suggest that, if one really wants to get an idea about density of population in a given area, one should pursue all the lines of inquiry. It is only when a series of approaches, based on mapping of fields and excavation of both cemeteries and settlements, give you figures which are of the same sort of order that one can feel fairly confident that one has, in fact, reconstructed the density of population for any given period.

BEALS: On the question of appraising or evaluating data from a cemetery . . . I think that, unless you establish the age and sex distribution of the population, your figures can be very misleading.

There is another approach to the population, which I do not know whether I should put on the record because it is not my research but that of one of my colleagues, who has been working on some correlations in Australia. By eliminating tribes located on permanent watercourses, lakes, shores, or rivers . . . he has found a correlation of 0.93 between tribal size and a mean annual rainfall, and this extends from the western desert, with a rainfall of 4 inches a year, to the Cape York Peninsula, with a rainfall of over 160 inches a year. This sort of thing would obviously apply only to peoples on a given level of technology and is perhaps influenced by other factors that we still do not know about, but it does

suggest a very promising kind of approach for establishing a relation . . . between characteristics of the environment and the normal population density.

STEWARD: . . . I should like to suggest here that we might broaden the concept of ecology. . . . I must say I am a little bit puzzled as to just how this term has been used—whether it means environment or the adaptive processes by which culture is adjusted to environment. You may study a given technology in different kinds of environments, or you may deal with successively different technologies in the same environment. If you look at what I prefer to call "cultural ecology" as the adaptive processes by which the nature of society and an unpredictable number of features of culture are affected by the basic adjustment through which man utilizes a given environment, then it seems to me there are a number of hypotheses that archeology could take from ethnology.

I am thinking concretely, for instance, that among the pre-agricultural, essentially hunting tribes, where you have more or less the same technology of, say, bows, arrows, spears, and traps, there is a very profound social difference whether large herds of more or less migratory animals like bison or caribou are available, so that large aggregates of people are able to follow the herds over vast distances, or whether, with the same technology, though in different environments, the people hunt game which, like the guanaco, is scattered, so that large numbers of people cannot live or stay together for long periods. I should think that some of the data of archeology—the very animals found in the sites—might be approached from this standpoint. . . .

BREW: This reminds me of another very important point in the same line. In desert areas you can get a much greater congregation of people during the rainy than during the dry season; that is, you have instances where people come together in good-sized bands during the rainy season and then split up during the dry season into smaller family groups. Once again, this bears a direct relationship to the game, but in this case you could find it out from climatic history rather than from the records of the bones in the site; so you have a check on the other approach.

HEIZER: . . . I want to mention something that Professor Cook . . . and I are doing at Berkeley in an attempt to look at this matter from a slightly different angle. We have gone over the literature and collected about 400 instances of large aggregates of human skeletal material, which are divided on the basis of sex and age, and we are going to analyze these burial lots. Obviously, they come from different environments, ages, periods, and so on, but certainly some important vital statistics should result from the analysis. . . .

ROWE: I would like to . . . call your attention again to the matter of the influence of disease on population. . . . In trying to make estimates of previous populations, we have a tendency, I think, to assume that disease conditions were constant all over the world. I wonder if this is not a very dangerous assumption in the light of suggestions that have been made of the effects of diseases like malaria, for example, on populations in certain areas. I do not know what the current status of thought about the occupation of the Italian Campagna is, but I recall that the decline of population in that area in the Middle Ages used to be attributed to the introduction of malaria and that the eradication of malaria in recent times has made it possible to reoccupy a very large area which had been heavily populated in Roman times. . . .

CASO: I am reminded of the introduction of smallpox after the conquest of Mexico and the depopulation of the valley of Mexico by this disease.

MANDELBAUM: The recent demographic history of India, I think, can shed some light on the point that Dr. Rowe mentioned . . . and may also give some clues which the archeologist can use. . . . If there is any crucial factor there, it is the availability of food rather than the incidence of disease, and the evidence is, very briefly, this: It is well known that the population of India has spurted mightily in the last eighty years . . . and it has not been a steady increase. There have been ups and downs. The last down was during the influenza epidemic of 1918–19, but in the last thirty years the population has gone up in a very sharp spurt. However, two demographers, Geddes of Edinburgh and Suvani of Poona, have broken down the population increase by regions and found that there has been relatively little increase in the Ganges Valley, which is among the most densely populated parts of India [but where the per capita food supply has remained constant]. The great increase has come in other areas, where, because of the availability of tapioca or because of double cropping or something else, there has been an increase in the food supply.

There has been little increase in the Ganges Valley, despite the fact that all the things which tended to lower the death rate in the rest of India were also operative there . . . namely, smallpox inoculation, the control of cholera, the suppression of banditry, etc. In this particular example, at any rate, the availability of food seems to have been a more important influence than disease. . . .

CLARK: This subject is a very long one indeed. It is one on which we need a better book than Mr. Krzywicki's. A number of people have mentioned evidence which, I believe, if pulled together, would form an extremely valuable book on population in modern simple societies, and I hope some ethnologist will write it for us.

About this matter of trying to reconstruct prehistoric populations, it is true that expectation of life and the distribution of sexes will enable you to check whether all people are being buried. A very great deal of research has been done on this. I might mention some particularly interesting work on the remains in cremation cemeteries, where you might despair of getting this information. A Swedish anthropologist named Guya was able to determine sex and, to a certain extent, establish age, even from very highly cremated bones. He has tested out his theories by making friends with some of the crematorium officials in Stockholm and seeing what portion of wins he scores and has got it down to a point where he makes only a very slight error.

As for this question of disease . . . I believe that the interrelations of disease, food, and other matters are very highly complex. Take the stock example in the schoolbooks of the Black Death, which ravaged a large part of Europe in the fourteenth century. Some medieval economic historians at home are becoming very fully aware of it. They cannot begin to understand the history of the fourteenth century unless they have some kind of clue as to the increase and decline of population. There are certainly some cyclical movements. . . .

The interesting thing is that in periods of increased population you get, for example, great forest clearances occurring over large parts of Europe. In periods, on the other hand, when the population has gone down, which more or less coincide with the Black Death and other pestilences and plagues, you get the reversal: great areas of tillage, where we know there had been manors,

churches, etc., were converted into sheep-grounds. . . .

So there are these interrelations. The difficulty is, particularly when you know more about written sources, that you begin to realize the very great complexity of the subject. When you project it back to prehistoric times, there are so many variables that you really begin to despair.

MEANING OF "CULTURE"

BREW: My assignment this afternoon is rather in the nature of "shock tactics." To introduce the subject for discussion, I am going to call rather largely on my own experience. Those of you who are Americanists will understand what I mean when I say that I am considered by a large number of my colleagues to be anti-Mogollon. I am not anti-Mogollon, as I continually assert, because, obviously, that term has reference to an area in the Southwest which has some differences from other areas. What bothers me is that, whereas in the Southwest we have two and possibly three rather well-developed prehistoric assemblages of traits called "cultures," we have in Mogollon a culture which is clearly distinguished from the others only in pottery type, with a few other traits possibly added. This leads me to the consideration of what archeologists mean by "culture."

Now, fortunately or not, though I am mainly classified as an archeologist, I have been brought up as an anthropologist and have worked in conjunction with cultural anthropologists, even to the extent of being on jobs with them. There is a considerable difference, which, I believe, affects our thinking quite a good deal, between the concept of culture used by the cultural historian or the social anthropologist and that generally used by the archeologist. This is particularly apparent when you are dealing with recent times, such as I have just mentioned, where you have some rather highly developed and detailed archeological assemblages.

To project this point into earlier times in this country, I find myself completely unable to think and operate in terms of what is quite generally called the "Folsom culture." I can only see the Folsom point and its distribution over the United States. . . . So I present this for discussion, with the bald statement that I am not at all sure that our researches and our thinking, particularly at the interdisciplinary level, are assisted by the insistence of anthropologists on thinking of a collection of one or two or three distinctive artifact types as a culture.

KLUCKHOHN: I agree entirely with what Dr. Brew has said. I would like to add just two footnotes. Dr. Kroeber and I have just recently put to bed in Dr. Brew's publication series a five-year labor in which, among other things, we examined every published definition of culture that we could find. Somewhat to my surprise, . . . we did not get too many explicit formal definitions from archeologists—about eight or ten, if I recollect correctly. But of these, in our opinion, seven or eight are, perhaps, the most sophisticated from the point of view of a "cultural anthropologist" that we encountered anywhere. That is, from our sample, there seems to be evidence that thoughtful archeologists [e.g., Taylor, Ford, and Rouse[2]] were really rethinking this whole problem, worrying about it, and producing some definitions which were more precise, in our judgment, than those pro-

[2] Walter W. Taylor, "A Study of Archeology," *Memoirs of the American Anthropological Association*, No. 63 (Menasha, Wis., 1948), pp. 97–112; James A. Ford and Gordon R. Willey, "Surface Survey of the Virú Valley, Peru," *Anthropological Papers of the American Museum of Natural History*, XLIII, Part I (New York, 1939), 38; Irving Rouse, *Prehistory in Haiti: A Study in Method* ("Yale University Publications in Anthropology," No. 21 [New Haven, 1939]), pp. 15–18.

duced by most ethnologists and so-called "social anthropologists.". . .

My second point I think is by no means as true as it was ten or fifteen years ago, but in certain quarters I still detect a tendency to confuse culture and period. In my own publications in archeology the one thing that I would claim was worth some attention was pointing out that, if tree-ring dates are dependable, there is no doubt that a Pueblo II "culture" and a Pueblo III "culture" were going on at the same time, and on opposite sides of a canyon about 300 yards away. Now I do not have the faintest objection to calling one site Pueblo II and another Pueblo III, so long as these are considered as cultures; and here the difference in artifact complexes is sufficient that I think they can be called cultures. . . . But they are not periods in the sense of sequential units.

STEWARD: It seems to me that the use of a term like "culture" should fit the purpose and that we need not be overly worried about trying to achieve a universal definition that will at the same time be an operational definition for the archeologist, the area student, etc.

If you mean by "culture," inherited modes of behavior, common understandings among people of a given group, then an encampment of Folsom hunters had a culture, although you know only the point and a few other things about it. That, however, is not the archeologist's only purpose. His next step . . . is to arrange his material in time and space; and [this leads him to] the cultural area, used also by ethnology. . . .

When you consider whether Mogollon should be distinguished as a culture area, as a subarea, or as something peripheral, your judgment, it seems to me, will depend on the historical relationships. . . . That is not to say that such an area is a functioning, integrated social-cultural whole, in the sense that a single band or encampment might be. . . .

Then we come to . . . these so-called "interdisciplinary" studies of world areas. The people working in this field have chosen as their units areas in which there is a similar culture, from an operational point of view. That kind of area is very different from a nation, in which the phenomena, as studied by different disciplines, . . . constitute a single, organized whole that is integrated, so that you can use a holistic approach to it. It seems to me that "culture area," "sociocultural whole," "small group"—all these are different concepts that may serve different purposes.

OAKLEY: I would like to put a small diagram on the board, following on from what Dr. Steward has said. This animal [*Iguanodon*] will be familiar to a lot of you. It was described on the basis of a single tooth by Gideon Mantell in the 1820's, but I think one would say that the creature is now known as a whole. One knows almost all one can know about this fossil organism, and yet, when it was found, it was just a single tooth. But the name was attached to it.

Now, you would not say that that tooth was just a thing in isolation and should not be regarded as part of an organism. If you take the definition of "culture" which I find it convenient to use as a paleo-anthropologist—"the sum total of what a given society produces, practices, and thinks"—and you take that [drawing circle on board] to be the culture of a given society, and all that is left of it, shall we say, is that [indicating a dot], the Folsom point, you would not say that the unknown part does not exist. I do not know whether the analogy is clear, but it seems to me that it is perfectly scientific to talk about the Folsom culture, even if you know only one point in it, just as it is legitimate to give the ge-

neric name *Iguanodon* to a single tooth, as if it were a whole organism.

REDFIELD: I want to ask Dr. Oakley if this is not true: Once you had the tooth of the *Iguanodon* and could distinguish it from all other teeth, you knew that that kind of a tooth would always be attached to the same kind of beast. It is not quite so clear that the Folsom point will always be attached to the same culture.

OAKLEY: There are a number of cases in paleontology where indistinguishable pieces have been given a single name, and only with the growth of knowledge do we find that they actually belong in separate families.

LINTON: Were there not various species of *Iguanodon*?

OAKLEY: Several species.

STRAUS: As a complete layman, I timidly would like to venture some definition of culture. . . . It is, of course, of great interest to those of us who are interested in paleo-anthropology, whether such forms as the Australopithecines had a culture. . . . It has occurred to me a number of times that one of the prerequisites of a culture . . . is that the assemblage of habits and things which a particular society has should be inheritable. Baboons, for example, have social behavior, but there is no indication that this is passed down in any way, by means of language, written or spoken.

I realize that definitions can vary a great deal, and I might say that a month ago I was privileged to go to a meeting of the Committee on Development of the National Research Council, which took up terms and concepts used in developmental biology, and it was rather startling to a great many people to find that the very first term this group of twenty-five had to define was the common term "growth." There was no agreement at all on its definition among an assemblage which included every sort of "-ologist,". . . plant physiologist, pathologists, bacteriologists, and so on. If they were unable to agree on such an apparently simple Anglo-Saxon term as "growth," what can we expect with "culture"?

It seems to me that the important thing is that, when people use the term "culture," they should define what they mean. . . .

NADEL: Might I protest very briefly but very strongly against the use of "a given society," which was said several times? The one thing that is certainly not given is the society, when you discover certain artifacts or relics from previous times.

LOWIE: I think some of the speakers have not quite understood Dr. Brew's meaning. If I understood him correctly, he did not mean to deny that Folsom man had a culture, but simply that from the Folsom points it was impossible, in our present ignorance of correlations in culture, to deduce what particular form of, let us say, social organization or religious beliefs were associated with the Folsom culture.

BREW: Therefore, the concept of "Folsom culture" seems to me quite worthless. . . . The thinking that these tiny assemblages are cultures is a definite danger, I think, at the prehistoric level.

STEWARD: How much do you need to know to call it a culture?

BREW: Well, that is a difficult question.

BASIC STAGES OF CULTURAL EVOLUTION

KRIEGER (statement revised after session): This is a very brief and general sketch of basic stages in the cultural evolution of the New World compared with the Old World. Considering the discussions of the past hour, some of the terms I use may be objectionable.

For present purposes, I will consider a "stage" to be a segment of a historical sequence in a given area, characterized by a dominating pattern of economic

existence. The general economic life and outlines of social structure of past peoples can often be inferred from archeological remains and can be related to similar phenomena, whether the dates are known or not. The term "period," on the other hand, might be considered to depend upon chronology. Thus a stage may be recognized by content alone, and, in the event that accurate dates can be obtained for it in a given area, it could be said that the *stage* here existed during such-and-such *period*. Further, the same stage may be said to appear at different times or periods in different areas and also to end at different times. A stage may also include several locally distinctive culture complexes and minor time divisions. A great deal of discussion is needed on these points, as Kluckhohn and Brew have just indicated.

The four stages which I wish to outline are simpler than those used by Julian Steward in his study of cultural causality and law.[3] First, we have in the New World what is commonly called a "Paleo-Indian stage.". . . To employ an economic term, [the people of this stage] may be called the early hunters of the New World, living during the Wisconsin Glacial Age and hunting many large mammals now for the most part extinct. They must also have consumed some other foods, but it is difficult to say just what. These New World hunters probably lived the same general sort of life we associate with Upper Paleolithic peoples of Europe. . . .

In Wisconsin times, population density may have been far greater than most anthropologists assume for the New World. While exceedingly few artifacts have been published for this stage, I have made a rough estimate of some 25,000 in existence, mainly in the hands of private collectors. It is necessary to visualize how many thousands of camp sites and perhaps hundreds of thousands of artifacts may lie buried under vast areas now blanketed with alluvial and aeolian deposits, and the infinitely small chance of these being exposed by erosion. Yet these camps and slaughter-sites have been exposed in considerable numbers in arid regions.

As to the original physical stock, Birdsell has advanced the hypothesis of an Amurian stock in northeastern Asia, available for migration to the New World as early as the Third Interglacial stage. Unfortunately, there are still far too few skeletal remains either to prove or to disprove this theory.

The second stage I have called "food-gathering," basing it primarily on the widespread appearance of manos and milling stones in North America soon after the retreat of Wisconsin glaciers. Radiocarbon dates on several sites in the Southwest and Great Plains suggest that these implements were present 7,000 or 8,000 years ago. Undoubtedly, the collecting and grinding of plant foods was at first only a supplement to intensive hunting but increased as the Pleistocene fauna disappeared. In the space of only 5,000 years or less, we have this incredible situation of many species of large grazing animals becoming totally extinct in the New World. During this time, with increasing utilization of various food resources, New World economy may be roughly compared with that of Mesolithic peoples in the Old World.

Perhaps about 6,000 years ago, several innovations took place in this food-gathering stage, principally the appearance of polished-stone implements and ornaments. In my opinion, this technology provides a marker quite similar to what used to be called the Neolithic stage in Europe; that is, when

[3] Julian H. Steward, "Cultural Causality and Law: A Trial Formulation of the Development of Early Civilizations," *American Anthropologist*, n.s., LI (1949), 1–27.

it meant the "new stone age" rather than its present peculiar application to food-producing economies. In the New World the appearance of polished-stone technology is more or less accompanied by the growth of shell heaps along the coasts and along the major rivers in forested areas, showing that the use of fish and shellfish had become a very important addition to dietary resources. Indications are that population increased greatly and life was quite sedentary at these shell-heap villages. In most of Anglo-America, the term "Archaic pattern" has been applied to such nonceramic, nonagricultural peoples who practiced a polished-stone technology, among other technologies [although the term is used differently in Mesoamerica]. . . .

About 4,000 or possibly 3,000 years ago, the Woodland or Burial Mound pattern became established in the Great Lakes and Middle West regions. The use of cord-marked utility pottery undoubtedly was transferred to these regions from a source in northeastern Asia, probably accompanied by new artifact types and possibly by use of cold-hammered copper tools and ornaments. However, food-producing did not enter any part of Anglo-America until somewhat later, and then from the south.

My third stage is that of food production. Two hypotheses may be offered to account for its appearance in both the Old and the New Worlds, even though it lagged by some thousands of years in the latter. First, the transplanation of the idea by one or more trans-Pacific historical connections cannot be ruled out, as botanical researches are making more evident all the time. Second, agriculture may have invented independently in both worlds, but for a similar reason, namely, that of population pressure forcing man to increase the yield of nature in order to survive. Such pressure could result either from expanding population in a stable environment or from decreasing food supply due to climatic change, or possibly from some combination of such factors.

The problem of Old World influences on the development of the higher American civilizations is highly complicated, and I think that many anthropologists have been rather foolish to insist on its impossibility without any real work on it. The origin of maize is a case in point, Anderson claiming that maize raised by many hill tribes of eastern Asia is indistinguishable from that found in early horizons in Peru. However, he by no means claims that this solves the origin of maize, stating that this is fully as complicated as discovering the origin of man himself, involving numerous races, varieties, and hybridizations in many parts of the world. Botanists do not as yet even know what maize is, other than an artifact, a creation of man from some unknown wild ancestor. . . .

Agricultural practices must have diffused from Mesoamerica to the southwestern United States as early as 1500 B.C. or so, as shown by the "primitive corn" in caves of western New Mexico. Charred corn has also been found in western New York by Ritchie in a context dated as early as 1000 B.C. by the radiocarbon method; but the relationship of the eastern corn to that of the Southwest, if any, is unknown. Plant domestication is known in Peru as early as about 2500 B.C., but maize is not known until much later, about 850 B.C., when pottery also appears.

It is interesting to note that the agricultural peoples of Anglo-America and northern Mexico never advanced beyond the general circumstances of the Pre-Classic cultures of Mesoamerica, that is, beyond what is now commonly called the "Formative horizon" as this is defined in Dr. Caso's inventory paper. It has been my contention for some

years that the first diffusions of agricultural knowledge from Mesoamerica to the southwestern and eastern United States took place while Mesoamerican cultures were still on a Formative level, in all probability long before the time of Christ. Radiocarbon datings on maize in the United States certainly bear this out, but we do not know as yet what associated traits may have been diffused with agriculture; I would think that certain Mesoamerican ceramic styles and perhaps even temple-mound construction and fertility rituals would bear serious consideration in this regard, whereas most archeologists insist that these appeared in the eastern states many centuries after Christ.

My fourth stage is that of urban life, with very large settlements, complex stratified society, a high degree of division of labor, and extensive trade. This "stage" combines Steward's two stages of regional florescence and cyclical conflict. Aside from Mesoamerica and the Andean region, the rest of the New World never attained this level, a matter which contains much food for thought. Was this limitation on the cultural growth of other agricultural peoples entirely one of time; that is, would they have attained such a level eventually where environmental resources were adequate, if not interrupted by European conquest? Or were there some inherent social or psychological factors which limited their population increase and attainment? In conclusion, it may be suggested that both the Old and the New World passed through very similar economic stages, and in the same order, although in the New World food-producing and urban life lagged considerably in time.

INTERPRETATION OF ARCHEOLOGICAL MATERIAL

CASO: It is almost a matter of course to say that the task of the archeologist is not limited to the careful recovery of implements and their classification but that his real task is the interpretation of archeological material. He almost never finds an object which is, or can be, dated by means of some representation or inscription that can belong only to a particular period. Most probably, he must date his discovery by its stratigraphic position or by statistical correlation. This gives him a date that can be fixed in a relative chronology or interpreted in terms of our own chronology. This is the first step . . . but it is not all.

The second step seems to me to relate the objects he finds with others of the same culture, of contemporaneous or earlier horizons, and with objects that came from other areas or cultures, earlier or contemporaneous. These relations are very important, because they show the technical and artistic development of a culture or the cultural influences that have occurred in the area in such a way that it should be possible to say whether, in one period, one culture was influenced by another, whether it influenced other cultures, or whether there were interactions among them. Commercial or war relations, influence by aristocratic groups, migrations, and conquests can be detected with relative accuracy by the more or less frequent finds of foreign elements, but their interpretation depends in a large part on the ability of the observer and on his knowledge of the areas or the cultures put in relation.

The third step, and the more important, is the reconstruction of the social life on the basis of the objects found. An object in itself is only a bit of material, and it is the idea and the function, with all their implications, that interest us.

We have heard in the first part of this session many important considerations concerning the ecological approach, the patterns of subsistence, and

the role of population in relation to the soil; but it is a question whether we can, and in what way, utilize modern ethnographic data for the reconstruction of the economic, the sociological, and the political aspects of a culture.

We archeologists are like detectives, in that we have to make the reconstruction of the drama with only very scanty and poor material evidence. Sometimes, perhaps, like the heroes of detective stories, we go too far in our deductions. But I do not know what is worse—an archeologist with unrestricted imagination or one with a complete lack of that quality so necessary in life, art, and science.

The reconstruction of a culture is not an easy task nor, I believe, one that can be solved by general rules applicable to all cases. It needs the application of the techniques of many natural and social sciences and a kind of ability at reconstruction or re-creation that seems to be nearer the aesthetic than the scientific mind. But the archeologist who pretends to reconstruct a culture or a period or an aspect of the culture can utilize the reconstruction that has been made by others, amplifying or correcting the previous point of view. His end is not to create beauty but to make us understand the reality that has disappeared. . . .

INFERENCE AND ANALOGY IN ARCHEOLOGY

WILLEY: I would certainly agree with Dr. Caso's good statement that contextual reconstruction or descriptive integration, the translation of cultural and human fossils into an image of life, is now the primary problem area of archeology. I believe that, although time-space systematics have not been thoroughly won, great strides have been made in that direction in the last thirty years and that the next great series of problems will be in the matter of the reconstruction of contexts.

I think we should realize the all-pervasiveness of inference on archeology and anthropology and how it creeps in at the very beginning levels of analysis. Immediately after the empirical recognition of the data . . . when we say, "Here is something made by man," we are beginning to infer and to use analogy. . . .

With the help of Drs. McCown and Movius, I have jotted down what I have called "categories" and "levels" of inference. One might begin with empirical observations on natural phenomena and, from geology, topography, present climate, pollen counts, etc., reconstruct a specific past environment. This would constitute a first level of inference. Similarly, we could take an ecological category and, from faunal remains, traces of vegetation . . . zones of ash from the burning of forests, etc., we could reconstruct something of past environment, as it was modified by man.

The third major category would be the cultural remains themselves, and as an example one could take architectural remains as the empirically observed materials and, from these and the groupings and nature of the buildings, arrive at an inference of settlement patterns. Similarly, artifacts and wastage material could be analyzed to arrive at a general technology, and so forth.

These compose, as I say, a first level of inference. From this level, we could go on . . . to [combine the] environment, ecology, settlement pattern, and technology, in order to move up to a higher magnitude of inference. This would give us subsistence economy or something on this very tricky matter of demography.

We could continue to make these combinations and extend this inference higher and higher. . . . Demography, settlement pattern, subsistence economy, and burial practice would give, I

think, certain outside limits for socio-political outlines. We could say an urban society, a rural society, or a nucleated rural one, such as the Maya tropical forest settlements are supposed to have been. . . .

The inferences that I have been talking about are, for the most part, made with reference to general comparative analogies. As I said this morning, I think there are two types of analogy, a general comparative one and a specific historical one, the latter consisting of analogies in an area where you have a continuity from the prehistoric into present ethnological horizons. Certainly, insights into things like social structure, matriliny, and patriliny have to be dealt with in terms of specific historic analogies. We are not yet ready, and perhaps never will be, to pick up a flint scraper and say that here we have evidence of a matriarchal social organization.

Inference and analogy are extended in space and in time . . . when we combine a number of sites or a number of periods into a culture, a tradition, or some other abstraction for handling data over a wider range. . . . We say, for instance, that Hopewell culture, which covers several hundred miles in the eastern United States and probably several centuries, had a divided agricultural-hunting economy.

As to what generalizations we can make about inference in archeology, I quite agree with Dr. Caso that we cannot formulate any set methodology now, that it depends upon specific problems. I think, though, that we can always say something about ecological and cultural inference; but, of course, this is with great variance in degree of success, depending upon the nature of the available data. . . .

I have mentioned general comparative and specific historical analogy and should like to say that I think that we will make the most immediate progress in the matter of descriptive integration along the lines of specific historical analogies. That is, we cannot directly compare potsherds with political institutions, and the first task is to translate the archeological materials and evidences into human institutions or into terms whereby general comparative studies can be made. Although I would agree with Dr. Kluckhohn that abstractions as to process, as to cultural development and interaction, are of great value, I do not believe that the archeologist can go very far along those lines now. I think we first have to take care of this other thing, the translation of our data into more general anthropological terms.

One final point, which is really neither inference nor analogy, is the matter of documentation. Documentation stands apart from inference and analogy and serves to check and supplement these. You might think of intra-cultural documentation, such as pictorial representations in art, something which is often very tricky and has to be studied and intepreted with caution; or of native systems of recording, Maya writing, and so forth; or of verbal history, passed on by word of mouth. Then there is the extra-cultural documentation, gathered by persons outside the culture. . . .

ROWE: . . . There are a few additional points that might, perhaps, be made, especially with regard to the use of ethnographic analogy. The archeologist finds material objects, and, starting with these objects, he can make a whole series of inferences. First of all, in trying to get at cultural problems, he can tackle the relatively simple problems of the technical processes which went into the production of the objects that he finds. Relatively little ethnographic data come in here. The reconstruction of technical or construction processes usually depends on technical knowledge which the arche-

ologist has at his disposal, either directly or through his friendships with laboratory people. You usually start by setting up a range of possibilities, how the thing might have been made, and then try to eliminate the possibilities. There are difficulties here, because the archeologist is likely to assume that his technical knowledge is greater than it is.

A second type of problem which is relatively easy to handle is the use of implements, buildings, etc. Here you can get at the general rather than the specific use; that is, by ethnographic analogy or historical records or something of that sort, you may be able to identify a knife, but you will not be able to tell what it was used to cut.

Ethnographic analogies may also be used beyond this sort of thing, to talk about the nature of settlement, the social organization, religious cults, and matters of that sort. And here is where we are most likely to get off the beam, because there exists a body, or several bodies, of theory in the anthropological literature which offer us correlations between material and nonmaterial cultures, and it is very easy for the archeologist to take these bodies of theoretical material and simply apply them to his artifacts.

We have had a number of references in our discussions to the use of evolutionary theory for historical reconstruction. The correlations between material and nonmaterial culture which are given in evolutionary theory are a priori ones and have not been documented by statements that the whole range of ethnographic information on this particular subject has been combed and that we always find such-and-such a type of cult associated with such-and-such a type of artifact. If we are going to use ethnographic inference for this sort of problem, it seems to me we have to start by establishing ethnographic correlations on a much better-documented basis than we have at present.

MURDOCK: One thing that seems to have cropped out again and again is that the archeologist needs from the ethnologist certain summaries of existing knowledge. . . . Actually, where ethnologists have make comparative studies, this has been done mainly in connection with some theoretical problem, to shed light, as I have done, for example, by the use of the statistical methods. But, obviously, the archeologist is going to need a great many correlations made for the purpose of discovering what goes with what rather than for the testing of hypotheses. The Cross-Cultural Survey, now the Human Relations Area Files, is one technique which in time should enable archeologists to make summaries of the correlations in ethnographic data that they require in order to make their inferences.

I would like to make one other point, and that is that the ethnologist can offer more than the correlations that exist within cultures. The ethnologist sees culture change on the hoof, so to speak; he sees processes of culture change about which the archeologist does not know. I just want to give one example of a very significant fact that comes out of our knowledge of diffusion, which, I believe, is likely to be of considerable use to the archeologist.

When we think of diffusion, we commonly think of diffusion on the adult level, of the people of one society coming into contact through trade and otherwise with adults in another culture and borrowing, imperfectly perhaps. However, in many parts of the world—this is very common in Australia and on the Northwest Coast—we have a phenomenon which we call "local exogamy," where the people of an area regularly marry between different villages, so that each child is brought up in two cultural traditions—that of the father and that of the

mother, who come from different communities. In these cases the child is, so to speak, learning aspects of two cultures at the same time. . . . If this is operative over a considerable geographical area, then it can result in very perfect diffusion, that is, the diffusion of form, function, and everything in its totality, whereas diffusion on the adult level will perhaps lead to a much more imperfect kind of diffusion.

These distinctions, when used by the archeologist in the light of the kind of distribution that he gets, may be able to tell a good deal about the social life and social interactions of the society whose culture he is studying.

MOVIUS: In other words, instead of using these traits only in a time-space sense, should we look at them distributionally and see whether they form any patterns from which social phenomena, such as extended family organization or something of that type, could be inferred?

PROBLEMS IN ARCHEOLOGY

CLARK: A good deal has been said during this symposium about the importance of a sense of problem. . . . The limitations of time and resources, of trained men and money, make some sense of problem an absolute necessity. At one time or another, all of us sit on bodies which attempt to allocate grants, and I think it is inevitable that we tend to give these grants to people who are going to, or who are likely to, solve problems. We want to know: Why do you want this grant? What are you going to dig for? How are you going to advance knowledge? . . . In archeology there is a special urgency about this, because . . . so many ancient sites have been destroyed by more recent historical activities. . . . In most countries, though it has been resisted in England, . . . there is usually a system of licensing, although I have noticed that the licenses are often not given, really, on the basis of problem, but on the basis of the technical competence of the investigator, which means whether he has a Ph.D. or something like that.

Having stressed this aspect of the matter . . . I would like to put in a plea on the other side. When you are aware of problems, then you must establish a priority. You are going to give grants only to people who want to solve the problems which are high on your list. I think we ought to realize that we should be very careful about this sort of planning, of which we have a good deal at home. (We have an organization called the Council of British Archeology, which takes itself seriously and has to be taken seriously, because people as individuals are so short of money. . . .)

It is a commonplace that in any science some of the most outstanding results have accrued by accident. I will just mention the word "penicillin," because it illustrates the fact that when such discoveries are made, they are normally a by-product of activities which have been directed to the solution of a problem, even if it is another problem. The relevance of this to our position, I believe, is that, when we are thinking about furthering different kinds of archeological research, we ought not to be too tight in making grants. We should leave individuals whom we consider worth supporting free to exploit the opportunities which open up, very often quite unexpectedly, in the course of their work. . . .

I think we also have to recognize that there are individuals who get "hunches" and that these individuals very often do more to advance knowledge than some more methodical people who are able to present their cases logically. . . . If Sir Arthur Evans had put in and said, "I want a grant to go and dig on an island called Crete," I

doubt very much whether anyone would have given it to him. Luckily for him, he did not have to worry; he just went to Crete, bought the site, and spent twenty years digging it. . . . Of course, as the Germans always say, Evans was an amateur, but he did, after all, discover Minoan civilization. . . . So I think that we ought, in our various capacities, to give great weight to temperament and to the fact that there is such a thing as a personality insight and that the people who have this quality are not always the sort of people who cut very much of a figure when it comes to putting up a case.

On the question of actual problems, I think that the definition of problems in archeology is less difficult than it is in some other sides of cultural anthropology, because archeology is like a sieve. We have so many gaps that there is no difficulty letting water out. . . . There are still, of course, certain geographical gaps, like large parts of Arabia, about which we know very little. . . . Another type of gap has, I think, been made very apparent to us by speakers from both sides of the house, as you might say. As soon as we juxtapose the so-called archeological cultures and the culture of any living community, or even if we walk through the Peabody Museum at Harvard, for example, we realize that we have only a very slight portion even of the material culture. The parts we do not have, the soft parts, so to speak, are the things we must find. There is a problem automatically.

Our problem is, in fact, to enlarge . . . our knowledge. We know many cultures which play a very important part in textbooks, the earliest Iron Age culture in my own country, for example, where we do not have a single burial. There is a problem. We can go on digging and digging at houses and forts, and this will only increase our knowledge by about ½ per cent; but, if we find one grave, we have increased our knowledge of burial practice 500 per cent.

My last point is, Where to dig? . . . I feel that reconnaissance is quite as worthy of our support in the present state of knowledge as actual excavation. One kind of reconnaissance might, perhaps, be mentioned, and that is the use of scientific methods like phosphate analysis. Phosphate analysis, of course, was introduced in agricultural science. I think it was first worked up by a Swedish scientist called Arrhenius. It rests on the basic fact that the local phosphate enrichment of the soil which occurs when food refuse decays, as it always does around where people are living, persists, although in small amounts, and can be measured. You can, in fact, contour refuse by taking samples at intervals. . . . This has been carried out very extensively in many parts of northwestern Europe and, I imagine, in North America. . . .

I will close my remarks by mentioning another field where we could employ science, in the discovery of sites. The material objects that we have to deal with, as Dr. Fejos pointed out and many other speakers have emphasized, are simply residue. Many scientific ways have been suggested whereby we could recover things which have virtually disappeared. Still, it would be nice to find these things under circumstances in which we could lift them out of the ground and, with a little scrubbing, put them in a case. If we only knew more about the chemistry of decay and about the various physical factors which make for the survival of different kinds of materials, we should be able to say, "We want to discover what these people's arrow quivers were like, what their woodwork was like, what their leatherwork was like. You work on such-and-such a mass and you won't find this, or in such-and-such a place

and you won't find that." We do it now by hit-or-miss methods. I think a really intensive study of the chemistry and physics of decay would be of very great assistance.

CO-OPERATION IN ARCHEOLOGY

BATES: Sitting on the committees that Dr. Clark was talking about, you come across the kind of project in which a man is going to hire two biochemists, one geophysicist, etc., to attack a problem. In that case the biochemist and the geophysicist are being applied scientists, in Dr. Mead's definition of the term, and are simply hired technicians. In the case of the archeological and ecological objectives we are talking about here, however, it is unlikely that we are going to be able to deal with that sort of teamwork. [Instead, we must develop] co-operative objectives. . . .

If, for instance, we look at the problem of archeological-ecological cooperation in terms of evolution, it seems to me that we find a common denominator in trying to understand the evolution of man, of culture, of plants, of animals, and of landscape. . . . All of them can be brought in here. The problem is one of persuading the people in different fields to get interested in objectives which are related to the objectives in which you are interested.

I would like to append a slight note upon the nature of specialization. It seems to me that we have altogether too much faith in experts. In looking around over biological sciences in particular, I note that the most interesting things are done by people who did not know much about what they were doing before they started. This seems to me very striking even in archeology, if you look at people like Sherbourne Cooke, who give you a new approach. If you want a botanist to work with you on an archeological problem, I do not see why you have to find an ethnobotanist. What you want to find is an intelligent, alert botanist who will get interested in the sort of problem that you have. . . .

KIDDER: I would like to say a word as to the practical working-out of an attempted piece of teamwork, namely, the project of the Carnegie Institution of Washington for the study of the cultural history of the Maya area. . . . The core of the investigation was, of course, archeological, in that the study of the Maya had been sold to the Carnegie Institution by Sylvanus Morley, who was an epigrapher. His early work was purely epigraphic, but he very soon realized that putting the flesh on the skeleton of history which he had been able to work out from the initial series dates could only be done by excavation; so he picked three sites which covered the then known extent of Maya history—Uaxactun, the oldest; Chichen Itza, which covered the middle period; and Tayasal, which was occupied until well into the seventeenth century by a going Maya city. The results of those excavations were very interesting and voluminous. To interpret them, it was felt that we needed the assistance of workers in many other disciplines, and so what was called—not by us, but by a commentator—a "pan-scientific project" was inaugurated.

I will run very briefly through the studies that were made. The Maya people still exist. There are probably some 3,000,000 of them in Guatemala and Mexico, Maya speakers, a great many of whom have carried on a great deal of the ancient culture, particularly on the subhierarchic levels. The hierarchies were destroyed, but the popular forms of life, beliefs, and practices have continued practically to the present day; so that we felt the most important work that we could do would be to get information for the interpretation of archeological finds from the living

Maya. A group to study the ethnology of the Maya was organized under the auspices of Robert Redfield, with the University of Chicago co-operating. Redfield, Tax, and others worked on the ethnography of the living Maya, and one of the most important products was Redfield's study of the folk culture of Yucatan.

Linguistics obviously were of importance. Studies of linguistics were undertaken by Andrade and, after his untimely death, by Halpern and later by McQuown, who is now working on the same problem at the University of Chicago. Studies of health and disease were done in co-operation with the Harvard School of Public Health by Dr. George Shattuck and various collaborators, who published on health conditions in Guatemala and Yucatan. The physical anthropology was begun by Steggerda and later carried forward by Dale Stewart in the highlands of Guatemala. The results are only partly published as yet, but they seem to indicate, according to Stewart, a very distinct difference in physical type between Yucatan and the highlands of Guatemala.

There was a large body of native literature in the so-called "Books of Chilam Balam" and a considerable amount of historical material in the accounts set down by the conquerors and missionaries. The native literature, the Books of Chilam Balam, were studied by Ralph Roys, who has published extensively on them. The conquest and colonial history were obviously subjects of great importance and are being worked on by Scholes, Chamberlain, and Hanke. Only a very small part, unfortunately, has been published, but a great amount of material has been gathered which is principally in the Library of Congress.

Lastly, a study of the environment . . . was undertaken by a group from the University of Michigan, working on both the fauna and the flora. A number of papers have been published on this, the most important of which, perhaps, was Londell's book on the phytogeography of Yucatan. In enlisting the aid of these scholars from other disciplines, it was felt that they would get the best results if they worked on their own problems, and so no attempt was made definitely to steer them into any particular groove which was of prime interest to the archeologist; but we found very shortly that they became interested in the same general problems that we were; for example, the medical people realized the importance for us and for all historians of the New World of the question whether certain diseases, such as yellow fever and malaria, were present or absent in prehistoric times. Agronomy was another subject on which we had a number of workers—we received a great deal of help from the United States Department of Agriculture, who sent men down, because our late ex-President was very much interested in the origin of corn . . . —and the agronomists and the biologists produced a great amount of environmental material which has not been synthesized but which is almost all of record. . . .

I hope to write a paper shortly on this program, bringing out the many errors and failures which resulted and some of the successes. The successes, I think, were that we gave a great many men in other disciplines an interest in archeological and cultural-historical problems and an opportunity to do a great deal of publication which they might otherwise not have done. One of the great errors was that we bit off a great deal more than we could chew. We can see now that we should have confined ourselves to the Old Empire region, the Peten and north Yucatan, instead of going wandering off into the Guatemalan highlands. . . .

Another difficulty was the fact that the program attempted to follow the history of the inhabitants of the area from the earliest time to the present; in other words, it was a vertical program. Our great trouble all through was that the workers, myself included, tended to go off horizontally. The people interested in the Pre-Classic cultures did not want to work on up into the early Classic, they wanted to go off and see what was happening in Pre-Classic times in Mexico, South America, and elsewhere. . . . The same was true of the . . . students of colonial history, who became interested in the *encomienda* and refused to work up into the later periods, such as the pre-revolutionary time and independence, because they wanted to study the *encomienda* all over Latin America. Naturally, that was a great mistake.

The depression came, the war came, the administration changed to one which was not interested in these problems, and the work has been very much cut down. But it is still being carried on in Yucatan, under the very able direction of Harry Pollock. Like Prohibition, I think it was a noble experiment.

CHAPTER XV

Physical Anthropology and the Biological Basis of Human Behavior[1]

Chairmen: T. D. McCown and Henri V. Vallois.

Papers discussed: Carter, "The Theory of Evolution and the Evolution of Man";* Greenberg, "Historical Linguistics and Unwritten Languages."

Speakers: Tanner, Oakley, Eiseley, Movius, Senyürek, Bidney, Beals, Straus,† Linton, Hoijer, Greenberg, Brew, McCown, Washburn, Lévi-Strauss, Mead, Bates,† Majumdar.

DIFFICULTIES OF INTERPRETING MORPHOLOGICAL CHANGE IN THE BRAIN

TANNER: There is one point which ought to be made here. It has commonly been assumed—and this appears in Carter's paper—that no morphological evolution has taken place over a very considerable period. The opinion is held that man has evolved morphologically up to a certain stage and then ceased to change, and there has been only "cultural evolution" since. I would like to point out that ignorance of the actual morphological structure of the human brain is quite astonishingly profound, from the point of view of an anatomist; and I think it is a questionable assumption to maintain that there have been no small changes of the sort that are, in fact, the basis of microevolutionary change. I will not say that these have occurred, by any manner of means. We have no evidence whatever about this matter. But I am saying that it is going too far to assume that they have not occurred.

We have many skeletons of early man, but from the skeletons we can tell nothing about the bits of morphology of the brain, and particularly from the quantitative morphology of the brain, the things which would be of interest in this connection. Even if the brains of Australopithecines were available, in our present state of knowledge the neuro-anatomist would not be able to tell much about them.

OAKLEY: Dr. Tanner has raised an extremely important issue. Weidenreich said that interpretation of intracranial casts was no more reliable than any other form of phrenology, and it is for this very reason that we now, within paleontology, look to cultural evidence for the evolution of the human brain. The purely physical approach to the fossil skull and the intracranial cast, does not really tell much about the anatomy. We have to look to this other source.

Another point that I think is most desirable is that, in the discussions on the evolution of man, one should keep quite clear these distinctions between

[1] Separate sessions on "Physical Anthropology" and on "The Biological Basis of Human Behavior" were originally planned, but during the symposium they were combined to make room for the session on "Anthropology as a Field of Study" (chap. xix), which was not originally in the program.

the Hominidae and man. I do not know how really agreed this is: that one could have a creature whom one can describe as a member of the Hominidae who is not man and who is prehuman. The ancestors of early Pleistocene men, such as *Pithecanthropus,* were below the level which we take to be human from the point of view of brain size or from the point of view of function, that is, tool-making, and yet one would not say, "Well now, those are another family, those are Pongidae." Whether one should have some subgeneric subfamilial name such as "Prehominidae," I do not know, but it seems to me it is very important to keep that idea in mind.

EISELEY: I would like to express myself as in agreement with Dr. Tanner in connection with this whole matter of the brain. It seems to me that what we do is to assume that, when we have reached a certain cranial capacity in a primate fossil, we must therefore automatically be dealing with precisely the same kind of primate brain that is represented, let us say, in the existing gorilla or chimpanzee, at that particular gross size level. Man is, however, a somewhat different creature, who has made peculiar adaptations to an unusual environment. I think it is quite conceivable that qualitatively, even far down on this level, there may already have been distinctions which are not manifested in terms of gross cranial capacity.

I am simply speculating, for example, as to whether one of the preadaptations, which may even have encouraged man to venture into the ground environment, may have been incipient speech beginnings which could possibly have begun to occur while man was yet arboreal. We know little about that phase of his existence, and we also know that certain of the existing primates are not far below the speech threshold. I sometimes wonder whether we have, because of the fact that we are now ground animals, concentrated too heavily upon the notion that man became a speaking animal, completely, only after he reached the ground and began to walk upon it. After all, the matter of vocalization is tremendously stimulated among many animal forms in the trees, most notably in the birds, as well as in the existing primates themselves. Perhaps when we have the length of man's Pliocene history and his ancestral size, we will be able to gain more insight into the problem.

VILLAFRANCHIAN OROGENY AND FAUNAL CHANGE

MOVIUS: It seems to me, from the point of view of time perspective, that there is one very crucial interval, which is urgently in need of broader consideration, and that is this interval that has now been included in the base of the Pleistocene, the Villafranchian.

In the past, the vertebrate paleontologists have hesitated to go into this as too recent, and they tapered off their studies with the end of the Pliocene, where the Villafranchian was formerly placed. But the salient fact remains that in the Villafranchian, there took place one of the biggest changes in the animal kingdom since the base of the Pliocene and the very late Miocene. This is well indicated by the differentiation of the Archidiskodon elephants from the *Stegodon-Mastodon* prototypes, and the coming of *Equus,* the coming of camels from the New World, the proliferation that took place in the deer family, and the differentiation of many other forms. These events hint of dramatic changes and acceleration in the tempo of evolution. Still, from the anthropologist's side of the fence, we deal simply with the primates, and we do not go into the background of vertebrate paleontology, the evidence in that field.

Turning to the Villafranchian itself,

we are dealing not with just a stage comparable to the other stages of the Pleistocene. All the evidence we have indicates that this interval was, perhaps, as long as all the rest of the Pleistocene put together. In it, tremendous natural events occurred on the face of this planet in the form of orogenies on a world-wide scale, tremendous diastrophism, coupled with mountain-building processes, erosion, and downcutting. A new cycle of sedimentation was initiated, along with a new climatic cycle, which induced glaciation for the first time in the Northern Hemisphere, or the northern part of the Northern Hemisphere and the high mountain areas. In those media we have the appearance of all these new genera, not just new species.

I would very much like to see anthropologists take the data and look at the problem of the differentiation of our particular line, the anthropoids, in the light of this evidence. Some of what is known to us today came from this very crucial interval in evolution, and the possibility, at least, should be entertained that these events may have a significant bearing on the human story.

THE AUSTRALOPITHECINAE AS STRUCTURAL ANCESTORS

SENYÜREK: . . . Referring mostly to the later phases of human evolution, I would like to state that a great many of our difficulties in assessing the phylogenetic position of various fossil forms comes from the lack of precise data upon their relative chronological position.

If *Plesianthropus* and *Paranthropus* are Pleistocene in point of time, as they appear to be, they would be too late to be the direct ancestors of Pleistocene and later hominids. There is no doubt that the Australopithecinae are closely related to man, and they should be included in the family Hominidae. However, as some forms, at least, appear to be late, they may at present be regarded as the ancestors of Pleistocene hominids in a purely structural sense, that is, as representing modified survivors of a group from which at an earlier date, probably in the Pliocene, the Pleistocene and later forms of man arose.

It is hoped that the improvement of carbon 14 technique will eventually give us reliable dates of early Pleistocene and late Tertiary forms. When we have sound dates, then we will know whether a fossil form occupies the right position to be the direct ancestor of a later form, if its morphology also is in a suitable state.

I would also like to refer to the question of the place of origin of man. Père Teilhard de Chardin has stressed the importance of Africa. I do not want to be on the record as saying that Africa is not the place of origin of man. Still I would like to point out that morphologically there is a wide gap between *Proconsul* and the Australopithecinae in Africa. This gap is to a certain extent filled by *Ramapithecus* from the Nagri formation of India. In the upper jaw of this genus we have features (such as the parabolic palate, reduced canines, etc.) which we would expect in the ancestors of the Australopithecinae (in *Plesianthropus* the upper canine is small, with a larger breadth than length, as in man). Thus I would consider it safer to say that man probably arose somewhere between India and South Africa (India, through western Asia, and East Africa to South Africa).[2]

TOOL-MAKING, TAXONOMY, AND THE EVOLUTION OF THE BRAIN

BIDNEY: I want to ask a question of Dr. Oakley. He made a very interesting point: that we should distinguish between the general class of Hominidae

[2] See M. S. Senyürek, "The Dentition of *Plesianthropus* and *Paranthropus*," *Annals of the Transvaal Museum*, XX (1941), 293–302.

and man. I should like to raise the question as to whether he would identify man primarily by the evidence of culture.

OAKLEY: May I say that tool-making is the most reasonable criterion to take, but that one may be devoid of evidence of tool-making. . . . I do not think one should just rely on a single criterion, although, theoretically, tool-making seems to be the most useful one.

BEALS: I am a little disturbed by Dr. Oakley's wishing to put tool-making as a measure of evolution in society. In a very gross way, this may serve to identify hominids from nonhominids; but, when you carry this out logically, you get to the point of tremendous rates of evolution in the human species since the Industrial Revolution. The notion leads logically in this direction if you are not careful with it.

STRAUS (statement revised after session): I would like to say something about the neural factors involved in the evolution of man. First, however, it is necessary to define the term "man." In this connection, I would like to express my agreement with Dr. Oakley's definition of true man—of the genus *Homo* —as a "tool-maker." This is but another way of defining man as a primate possessing a "culture." But, in so far as the geological record is concerned, tools—at least in the earlier Pleistocene deposits—would be the only evidence of a culture. I also agree with Dr. Oakley in being unable to conceive of the existence of such a custom as deliberate tool-making unless it is socially heritable. The term "culture," as herein used, therefore implies social inheritance from one generation to another. This, in turn, implies an effective means of communicable symbolism or language. Man, or the genus *Homo*, may then be defined as an animal possessing a culture, for it is this feature that particularly distinguishes him from all other animals. Since the development of culture is undoubtedly linked with, or dependent upon, the great development of the highest centers of the brain, the definition of man as a "tool-maker" or as a primate possessing a culture is really a biological one.

There have been various attempts to decide when a primate brain would be far enough advanced to be labeled "human." Relative size of brain, expressed in terms of body weight, is not a good standard. According to Schultz, the body-brain ratio is slightly more than 2 per cent in adult man and less than 1 per cent in adult great apes. In gibbons and certain Old World monkeys, however, it can be well over 1 per cent, even close to 2 per cent. In some lemurs this ratio is considerably more than 2 per cent and hence relatively greater than in man; but the relatively largest brain of all, at least among the pithecoid primates, is that of the stupid little squirrel monkey of Central America which has a body-brain ratio of 3 per cent or more.

Absolute brain size has sometimes been used as a standard. Sir Arthur Keith's recent pronouncement of an evolutionary Rubicon that must be crossed in order for a primate to become a man represents such an attempt —in this instance, an arbitrary brain size of 750 cc. Such a criterion, however, is likewise unacceptable. For it is quality of brain rather than quantity, absolute or relative, that is all-important.

We have, however, no reliable morphological standard of brain quality or intelligence. Herein lies a riddle whose importance for paleo-anthropology is apparent, for example, when one attempts to assess the zoölogical status of the Australopithecines. Endocranial casts, despite the claims of Schepers and others, tell us very little. Some of the recently discovered Australopithecines apparently had brains of a size that carried them across Keith's Rubi-

con, but we know nothing of their cerebral quality. We cannot admit them to human status on brain size alone. Dr. Ernst Mayr includes the Australopithecines in the genus *Homo* on the basis of their apparent terrestrial bipedalism. This criterion is not, in my opinion, a good one. For one thing, it is often quite difficult or impossible to establish the precise posture of a fossil primate. There also are other objections that cannot be discussed here. Applying Dr. Oakley's definition of man, the Australopithecines must be denied a true human status; for, whereas they *possibly* used natural implements, there is not the slightest indication that they deliberately manufactured tools. Even baboons use natural implements that happen to be at hand, but they do not fashion tools. Thus, while the Australopithecines might be regarded as members of the family Hominidae on the basis of bipedalism, full-time or part-time, under the present criterion they cannot be regarded as members of the genus *Homo*.

It was Hughlings Jackson who pointed out that the nervous system is a hierarchy composed of several functional levels. The highest of these resides in the cerebral cortex. Whereas the lower levels operate on a reflex basis, the cortex operates to a large extent on what may be termed a voluntary or selective basis. In its most primitive condition, the cortex consists largely of two major portions, "motor" and "sensory," connected with peripheral structures such as the skeletal muscles and the integument via lower neural levels. In the course of cerebral evolution, other cortical areas, the so-called "association" areas, have undergone progressive differentiation and expansion. Their role in man has been studied consequent to tumors and other lesions, including surgical removal. They are concerned with the higher intellectual functions, involving ideation, memory, initiative, and symbolism (including language in its broadest aspects, both motor and sensory), acting via connections with the motor and sensory projection areas. In man at least, therefore, they might be termed "ideational" areas.

It is obvious, as has often been stressed, that man's great brain size is due chiefly to the development of the association areas. For example, one need only look at the maps of the motor projection areas of the great apes in the publications of Leyton, Sherrington, and others to realize that the motor region actually is relatively larger in these animals than in man. If the motor area be likened to a piano and the association areas to a pianist, there is no evidence that the piano itself has become decidedly more complex or better during catarrhine evolution; but the pianist has. It is the difference between what comes forth when I play the piano and when someone such as Rubinstein or Horowitz plays it. This, as I see it, is really the fundamental difference in behavior between man and other catarrhine primates.

In emphasizing the prime role of the higher cortical centers in human evolution and behavior, one must not ignore the importance of the character of the peripheral effector mechanisms—particularly, in the instance of man, the hand—that are operated by the cerebral cortex. If the peripheral effector apparatus is deficient, the central guiding mechanism is functionally frustrated. I have often thought that my dog, if she had a hand like that of a monkey, would prove to be far more intelligent than she appears to be. She has, I suspect, a brain that is, so to speak, better than her paw. The latter does not do justice to the qualities of her cerebrum. She lacks the musculoskeletal apparatus to express her "ideas" properly.

Hence, with the development of the

brain, there was required an adequate hand in order for human status to be attained. There seems little doubt that the evolution of the hand preceded that of the highest cerebral centers. For most of the catarrhine primates possess those manual prerequisites for readily evolving to a human status, and the hands of many of them structurally are not markedly different from that of man. This might well be regarded as an instance of functional preadaption that permitted, with the expansion and development of the cerebral cortex, the combination of an effective centranervous mechanism and an effective peripheral effector that led to the emergence of man. I have spent considerable time studying the hands of primates, both anatomically and physiologically. I have devoted particular attention to the rhesus monkey, and I am convinced that there is not a single primary movement that can be made by the hand of man that cannot be made by the hand of this monkey. However, the rhesus monkey has the prefrontal and other association areas of the cerebral cortex but poorly developed as compared with man. It does not have the ideas to synthesize the primary manual movements into the intricate series of co-ordinated movements required in such an activity as tool-making; nor does it exhibit any great prescience. On the contrary, the chimpanzee has larger association areas than the rhesus monkey, better ideas, and perhaps greater foresight; but it has a poorer, degenerate hand.

The production of tools and other manifestations of culture depend upon a competent hand attached to a brain in which "ideation" is developed to an appropriate level. But more than this is involved. True culture, as noted above, is socially heritable. This requires an adequate communicable symbolism or language. It is well known that considerable parts of the cortical association areas are concerned with speech, and the existence of motor speech, visual speech (understanding of written words), and auditory speech (understanding of spoken words) centers has long been established.

Elliot Smith and others have stressed the great importance of vision in primate evolution, particularly in the earlier history of the order, and the role of vision and visual speech in the life of man certainly needs no discussion here. But in the evolution of true man (*Homo*), and hence of culture, the development of the association centers of hearing must have been of special importance. One learns only so much by observing the performance of a complicated procedure. The ability to ask intelligible questions and to understand the answers is of at least equal importance. Hence, while admitting the prime importance of vision in earlier primate evolution, I doubt that it was the predominant sense in the final emergence of man. The development of the auditory speech centers was probably at least as decisive as the development of the visual speech centers —indeed, probably more important, for without doubt the spoken word long antedated the written one.

Therefore, it seems to me that the crucial event in the passage of the hominid line of descent over the threshold from a prehuman to a human stage—from some other hominid form to the genus *Homo*—must have been the final development of the association areas of the cerebral cortex, particularly the areas concerned with speech. It is inconceivable that even the most primitive sort of culture could have evolved until these regions of the brain had developed far above a simian status.

ORIGIN OF LANGUAGE

OAKLEY (statement revised after session): I agree largely with what Dr. Straus has said on this subject of the use of functional criteria in distinguish-

ing *Homo* from prehuman hominids. But I see no need to hesitate about using, in this case, criteria which are rather different from those normally used in paleontology. We are dealing with a functional development which is simply a reflection of a structural one; namely, an increase in the ideational areas of the brain. This cannot be reliably measured by studying the endocranial casts of skulls; but if it is reflected in some behavior which leaves permanent traces—for example, the fabrication of tools—these have evidential value comparable with parts of the skeleton. Thus I have suggested, as a working definition of man, "the toolmaking hominid."

Unfortunately, modern languages grew up in a prescientific era, and consequently it is easy to get in a state of confusion through having to use words whose meaning is no longer sharp and varies with the user (e.g., "man").

Again, take "speech" and "language." Neither word conveys exactly what Dr. Straus and I want in connection with defining man. His expression, "a communicating system," is much more useful. I do think it is extremely important to make a distinction between "communicating system" and language or speech as we understand it now. Quite recently, in British circles, great interest has been taken in a suggestion first made some years ago by Sir Richard Paget, that speech as we understand it now is very different from the original communicating system, which may have consisted much more of manual gesture and vocalized copying of manual gesture by the tongue and mouth (you may have noticed, when children are writing, they sometimes perform similar movements with hand and tongue).

Paget developed this gesture theory of the origin of language through studying voice production, and, by a remarkable coincidence, a philologist, Professor Alexander Johannsson, has now reached the same conclusion by an entirely different route—the comparison of six unrelated families of languages. A review of Johannsson's book was published in *Nature* about two years ago. He reached the conclusion that modern language families are of comparatively recent origin and have a number of roots in common, all with a probable origin of gesture.

The original crude communicating system, perhaps gesture accompanied by cries and grunts, may have lasted for hundreds of thousands of years. It may have required some geniuses to invent a system of verbal symbolism, of using particular sounds and tongue gestures to represent things and ideas, dividing up thought into quanta.

Paget, in developing this theory, suggested that in the original communicating system, perhaps lasting throughout the Lower Paleolithic, men had not formed the habit of thinking of one thing at a time by *means of names.*

If you try to remember how we dream: it is continuous, like a film. You cannot, in a dream, turn back in your mind and pick out items and rearrange them; and it is this trick of verbal abstraction or of thinking of one thing at a time, past, present, or future, which Paget regards as the basic "discovery" which led to the invention of language. Such an advance in communication would have resulted in an acceleration of cultural evolution, without the quality of men's brains improving, simply because inventive ideas would spread more rapidly. Previously, any new ideas arising sporadically did not circulate rapidly enough to cause any fermentation of thought. But, once you had a really efficient communicating system, such as language based on verbal symbolism, then you would expect a spurt of cultural innovations, such as, in fact, accompanied the spread of the

blade-burin tradition at the beginning of Upper Paleolithic times 20,000–30,000 years ago (new dating).

This conception of verbal symbolism as possibly not being original communicating system, but a comparatively late invention, like a new tool, say a burin, is something that might form a basis for discussion between linguists and archeologists. I am merely attempting to pass on the ideas of Sir Richard Paget, which are developed more fully in an article which he wrote in *Science News* (*20*)—a Penguin publication—just over a year ago.

LINTON: There are a couple of things I would like to say in connection with this language proposition.

In the first place, as far as I know, the anthropoids may be the closest parallels to our ancestors, relying much more on sound communication than they do on gesture communication. There are very large numbers of specific sounds which they make and which have quite specific meanings. I see in this idea of early man's being unable to think about one thing at a time shadows of the Durkheimian–Lévy-Bruhl "primitive mind." As a matter of fact, I would say that probably the trouble with early man, if one may conjecture, was that he thought too completely of one thing at a time, in distinction to later man, who is constantly taking into account both the past and the future. This also applies to the problem of tool use that has come up here.

There are a surprising number of animal forms that use tools. The distinct thing with humans is that they *make* tools; in other words, that they are able to *anticipate* the situation in which the tool will be needed and to prepare for it. This indicates, I think, a distinct type of psychology, the realization of operation in the time stream, which no other animal shows. I think this is the point, actually, where the human mind emerges, even more than in the capacity for reorganization of experience which we call "thinking."

HOIJER: We could, indeed, in a very short time invent or rehearse at least a dozen hypotheses of equal validity with Paget's hypothesis. There is certainly no evidence for the precedence of gesture language over verbal language that I know about—and the whole business strikes me as being one of those "belief" statements of origin in a field where hypotheses of origin are far more numerous than facts.

GREENBERG: I am in agreement entirely with what Dr. Hoijer said. . . . I think it is very naïve to assume that gesture per se means something different structurally than language as we have it in speech. Language as a structure involves two basic units, namely, a sound unit which in itself does not mean anything, but only in certain permutations and combinations has meaning. That is, a *p* sound and a *k* sound by themselves mean nothing. But certain combinations do have meanings, and that allows you to have a great many meaningful units.

The meaningful unit, however, does not stand alone. This is the unit called the "morpheme"—not quite the same thing as words—but let us say "words" for the nonlinguist. In other words, man speaks in sentences, and this is tremendously important. You can have symbols which do not involve sentences, single gestures, or any other kind of things, which are symbols but do not involve symbol systems. It is only when you get a symbol system . . . that it becomes so important. You can do the very same thing with gesture as with language. You would not be so efficient in certain ways, because gesture would not work in the dark and it might take a little longer, but I give this example sometimes to my class to illustrate it:

Suppose we found a species from Mars which had four straight antennae on their foreheads. We could, by per-

mutations and combinations, get all four of them out, a particular three at a time, two, or a single one. Offhand, we would have something like twenty-five or thirty elements. We could then have sequences of such elements to make up words, and we would have pauses, to make space between words. We would have a perfectly good language.

It seems to me that this is the important thing that is involved, not whether gesture came first—a thing we cannot find out because there just is no evidence.

With regard to this matter of Johannsson's six unrelated languages, this, I think, again is very naive. The theory has been offered again and again by people who know nothing whatever about historical linguistics. It is a matter taken up in my inventory paper, where I discussed what explanations we have for similarities among languages. If we take a series of languages, unrelated as far as we know, and compare them, I think we will get more than chance similarity among these six languages. Why? It might conceivably be partly due to an original unity of language of mankind, which would, however, be of very considerable antiquity, but partly because of the lack of complete arbitrariness between the sound and that which it signified, that is, onomatopoeic words, nursery words, and so on. These are the bases of our knowledge of recorded documentary linguistic history and reconstructional history of almost invariably very recent chronology. Words like "bow wow" for "dog" are made up again and again, words like "mama" for "mother," made up independently again and again. It has nothing to do with an original language of mankind.

BREW: I do not think we have to call on men from Mars. I am thinking simply of the system used by the deaf and dumb, where you get a complete language in manual symbols.

MCCOWN: Dr. Oakley, perhaps, misrepresented his views, because it is always dangerous to speak of origins when you are dealing with phenomena which are really continuous. It seems to me that the question of origin is, in a sense, highly arbitrary, because you are dealing with phenomena which continue. It seems to me unreasonable not to expect that you would have a continuum with regard to the development of a sound system, the superimposition or addition or integration of symbol systems. Therefore, any idea of origins and the implication of genius as being a finite point in time always creates a series of red herrings which vitiate what we can reasonably assume about the ultimate, the general, nature of these phenomena.

NATURAL SELECTION AND THE HOMINID BRAIN

WASHBURN: . . . I think if we ask whether a genus is defined by a tooth, a pelvis, or a tool, it is a barrier to further research. In evolution we used to think of the brain as a progressive force. The brain was something which created man. Edinger's magnificent monograph on the evolution of the brain brings out very clearly that, at least as far as most groups of mammals are concerned, the reverse is the case. The brain actually follows rather than precedes other morphological changes. It organizes anew something which has happened somewhere else in the animal.

If we look at the evolution of the primates from this point of view and think of the size of the brain of the apes, we can regard it as not too different in size throughout the Miocene and much of the Pliocene. During the Pleistocene epoch, however, we have the picture of a rapidly increasing brain size.

If we believe in natural selection—in other words, if we really take the point

of view Dr. Carter's paper outlined—our problem is to find out whether there is a new selective factor in the Pleistocene and not to ask whether there is a new genus. If tools are of great significance in human evolution, as soon as tool-using starts, there will be selection for large brains, which was not present before.

I think the idea that for many millions of years the brain size was more or less the same and that there then followed a change in locomotion which was practically completed prior to the change in brain size allows us to approach the problem from a functional point of view and not from pure taxonomy. I would further argue that, if we look at the canine teeth as a definition of man, or at the molar teeth as a definition of man, or at the ilium as a definition of man, in each case there will be included different forms with man. . . . If structures are varying independently and if the primary force working on them is natural selection, we should anticipate that a classification based on locomotion, teeth, and brain should give contradictory results and that, If we want to understand process, the last thing we want to do is to argue which goes into which genus. This will destroy the whole argument. . . .

If we believe seriously in natural selection and see the tremendous increase in size of the brain during the Pleistocene period, we can only conclude that natural selection has a tremendous importance for man and it is our job as scientists to determine its importance. . . . I would argue that we should take our genetics and our evolutionary point of view seriously and that, if we take genetics and the evolutionary point of view seriously, we will cease to be disturbed about whether we define man as having a brain of over 750 cc. or being a tool-user or any of these other definitions, and will want to know instead the detailed functional interrelations between time, distribution, culture, and the human organism.

LINTON: I would just like to say a word in connection with the possible effect of tool-using and the increase in brain size. I think we also have to take into account the possibility of evolutionary drift, whatever it was or is, which has resulted, particularly during the closing phases of a family's evolution, in hypertrophies of various sorts. This idea is not my own. Its originator contended that *Homo* was really an anthropoid suffering from cerebral hypertrophy, as a phenomenon of the closing phases of primate evolution. . . .

VALUE OF INVESTIGATING COMMUNICATION SYSTEMS IN OTHER FORMS OF LIFE

LÉVI-STRAUSS: I am very much surprised that this problem of the position of speech as evidence for the evolution of man has been considered exclusively in relation to human phylogeny and not to other kingdoms of nature. After all, the closest approximation we have to human language is not to be found in gesture or vocal behavior of apes but in a quite different part of organized life, among insects, and I am thinking of the recently published work of Von Fritsch on the language of bees. . . .

. . . This so-called "language" at least makes use of the combination of constituent units. It seems to me that the problem of speech is not a problem which should be considered inside the evolution of man but as a general problem, involving a certain level of organization of living matter, and this, of course, would change the issue entirely.

MEAD: . . . We ought to experiment more with the question of bringing up chimpanzees and orangutans, or other primates, together. One could take two

groups of anthropoids, each which has a very limited range of communication and undoubtedly a very limited range of significant sounds but slightly different ones, and see if the young in such a situation would imitate not only the elders of their own group but the elders of the other group. Then, if you could maintain that over three or four generations, you might see whether you would not begin to get a more complex symbolic communication. In other words, one of the most significant things that happen in this early period where we are postulating origins is contact between groups with very limited, but very different, meager forms of communication, groups which had the capacity to learn a little bit more than they were using.

GENETICS AND SOCIAL SELECTION

McCOWN: We will move on to some remarks by Dr. Eiseley, who has consented to talk on the problems which were adumbrated in Dr. Tanner's earlier remarks; that is, this consideration as to whether the problems of phylogeny can be considered to have reached a sort of termination or whether it is simply a function of our ignorance or lack of techniques that prevents us from seeing phylogenetic processes in operation.

EISELEY: It seems to me—and here, I think, is one of the places where the physical anthropologist who is cognizant of culture can be of some help—there has been a tendency for biologists who are unacquainted with cultural problems to extrapolate their data into human situations, where it actually does not apply or does not apply in quite the way that they visualize. I note in Dr. Carter's excellent paper this comment: "What are likely to be the effects of removal of all effective forces of selection by social legislation in the most recent stages of man's evolution, or in the future, as discussed by Muller, who points out that it must lead to inevitable accumulation of deleterious mutations and to ultimate degeneration?"

This sounds in essence remarkably like some of the things that I used to hear back in the twenties from some of my professors. I think one of the things we have to be very careful about is the tendency to associate particular genetic traits with specific social classes. There is a kind of implication, somehow or other, that, because of the declining birth rate in the section of the population in which all the advantageous genes—artistically, intellectually, and so on—are supposedly concentrated, these favorable traits are becoming lost and that there will be an inevitable decline in the level of intelligence.

The vast human population constitutes a gene pool potentially far more extensive than has been realized phenotypically by any specific class, for classes are, because of social upheavals and other crises, often a very passing phenomenon in human history. The result is that class selection is not always genetically significant. I do not mean to suggest that selective forces do not operate. What I am trying to emphasize here is that, as physical anthropologists who are also acquainted with social problems, we must not easily fall into the kind of extreme eugenicism which holds that the problem can be simplified to the assumption that good genes are likely to become concentrated in certain social strata and to become drastically reduced in others. One of the amazing and interesting things, as one examines the actual history of genius, is the number of such people who have flowered out of the most peculiar and unlikely environments.

Another thing involved in this whole problem is that of the perpetually dying city. Somewhere around 1933 our birth

rate had become greatly depressed, and, as a consequence, a particularly gloomy view prevailed at that time as to the future of the Western population—the urban one in particular.

This perpetually dying city, which seems to be such an accompaniment of Western industrial civilization, not only has to be considered carefully in the light of the mortality rates of the varying groups within it, but it also has to be surveyed from the standpoint of how long this situation is likely to continue. We want to know whether this failure to reproduce completely is a constant and, so to speak, eternal accompaniment of urban society or whether, to a certain degree, it involves the social structure of our own society as it exists now or as it has existed in the past. In addition, there may be certain lags in adjustment as people make the transition from rural to urban society. We must also realize that man in general may not yet be adapted to the city. At best, he has only lived this life through a few millennia. Although differentials in the birth rate suggest a certain loss of what we may call talented individuals (who may or may not be carrying unusual genes), some of this striking differential may decrease along with the social adjustment of cities and the decentralization currently going on in some aspects of industry. In other words, I do not think we are in a position at the present time to assume that there is some kind of constant law, so to speak. There may be even some question, as other cultures urbanize, as to whether they will urbanize, from this standpoint, in precisely the same manner.

But the other thing that we would like to know and that we know very little about at the present time is how much in the way of human talent and ability is actually moving cityward. That some kind of movement goes on is quite apparent, but, as to what extent it is draining superior genes cityward and to what extent this is a deleterious situation, there is really little or nothing in the way of adequate statistics to go upon.

Furthermore, this decline in the birth rate during the thirties was considered cause for serious concern by many at that time; the shadow of the depression years more or less influenced some of the speculation. Now we have had a remarkable upswing in the birth rate. It is not comparable, of course, to the kind of increase taking place in certain of the other areas of the world. How far it will go, and how much it represents new changes, new adjustments to a different set of social circumstances and a greater degree of social security, it is difficult to say.

Here, again, the eugenically minded are in a serious dilemma. On the one hand, some contend that our population should be increased; on the other hand, there is the suggestion that any kind of social legislation which improves the lot of the common man may contribute to the elimination of advantageous selective forces. I repeat what I said before, that selective forces do not cease to operate in human society. There may be changes, shifts in direction, both of a physical and of a mental nature. Again, we know so little about what genetically constitutes a superior brain or what kind of genetic factors (doubtless multitudinous) are involved that it is difficult to say very much at the present time. But we would like to know how much more about this movement of genes in our urban world and to know, furthermore, again from the standpoint of mutation, how long a time it takes in terms of a given social structure for any real social selection of a significant character to take place.

Our society at the present time, from the standpoint of vertical mobility, is an intensely fluctuating affair, much

more so than certain older societies. Yet many changes are now becoming world-wide; so that again, from the standpoint of trying to analyze briefly a few of these situations, we have to remember the problem of an aging population and what that does to the breeding groups. In addition, we have the problem, turning to racial factors for a moment, of the northward urban movement of the Negro, which, while reducing his birth rate, undoubtedly places him in a position where many more genes from his particular minority group within the population will move into the general structure. All these things—these intricate matters of social activity on our part, linked with even such apparently distant things as social regulations, social security, the problem of how much certain kinds of social security control horizontal mobility of labor, and so on—we as human biologists must recognize and look at in a different light.

LINTON: It appears to me that we do have a certain check on the extent to which the abler variants in the population, at least abler intellectually, can be selected out from the population without serious damage to the general level.

After all, we must remember that Western cultures over a period of about, roughly, fifteen hundred years have had celibate clergy under situations where this was the main escape for the able man at all the lower brackets, at least, in the society. . . . You can imagine, therefore, that our society has been exposed to a dysgenic selection over a very long period without . . . much damage—unless we take Galton's idea that the classical Greeks are above even the British in I.Q., on the average.

The other thing, on the cities, you suggested that this phenomenon of the nonperpetuated city population is something connected with recent Western mechanization. This is certainly not the case.

EISELEY (submitted in writing after session): I simply brought this up as a problem having industrial aspects. Our earliest consistent and reliable statistics are Western. Furthermore, contraceptives distributed on a wholesale scale are a Western, industrial invention of the nineteenth century.

LINTON: It seems to me to hold everywhere. I have some little information on this from China. It is exceedingly significant here that, with the aristocratic, large-family pattern, which actually is constantly being re-created, very few last more than four or five generations at the outside; there are practically no large family headquarters in Chinese cities. Those families simply do not last long enough.

The new thing that has come into the situation here is the sedentary suburb. This should be studied demographically. Here you have room to spread, and perhaps increased repopulation tendencies. It would also be interesting to note the predominantly matrilineal society which develops in the suburb and what effect it may have on the pattern configuration of individuals born and brought up in the suburb and then jettisoned into the technological world, which is largely male-controlled. I do not know that anyone has approached the possible interrelations between the rise of the suburb and the rise of, shall we say, the onset of phychoses and psychoanalysis. That is a point.

BATES: . . . I belong to a minority —perhaps not a minority either, but to a group—of biologists who are resisting the "pecking incidence" of the geneticists, because there are a few of them who are very dominant, dogmatic, and determined personalities. They bamboozle their ideas across to keep the rest of us in our place; but you can always go back to people like

those Eiseley was citing and remember that they were very dogmatic about something in which they were obviously very wrong, and you begin to question some of their present dogmas. But, aside from that, the problem of genetics or what genetics is involved with is the problem of what I would call the mechanism of inheritance. . . .

If you talk with biologists and talk with, for instance, Sonnerborn rather than Dobzhansky, you get a quite different idea of what we know about the mechanism of inheritance, even in organisms that they can deal with easily in the laboratories. If you go back and reread Darwin's *Origin of Species,* which I do periodically, it is an interesting experience. One of the things that impresses me is how contemporary the *Origin* still sounds. In one aspect, this can be attributed to the genius of Darwin; in another way it is an indictment of what has happened to biology since 1859. Darwin did not have an understandable mechanism of inheritance, so he hedged on this point; and you can notice all the way through that the book still makes perfectly sensible reading if you just keep in mind the growth in our knowledge of the mechanism of inheritance.

In general, I tend to be more preoccupied with what we do not know than with what we do know. To go back to the last and truly very excellent book of Simpson's on the *Meaning of Evolution,* there, again, I differ from him only in that he stresses what we know; I am always thinking about what we do not know. It seems to me that from Simpson's book one comes out with too positive a feeling, a feeling that we know more than I would think we do.

The point, in relation to the things we were talking about here, lies in the fact that contemporary man offers great possibilities for the study of evolution. I do not think for a minute that evolution stopped with man at the beginning of the retreat of the last glaciation or some such time. Today, with man, we have statistics available that we do not have for any other animal. As men ourselves, we are able to have some idea of what the sense-perceptions, what the interactions, of men are. When I am working with my mosquitoes, I always feel that I have landed on Mars or somewhere. I have no idea what the impact of the environment on the organism is. With man, I can begin to visualize this. We know a great deal about death, birth, and things like that in man, population forms. In no other animal, I would say flatly, do we really know the order of magnitude of the different kinds of causes of mortality, which is at once involved in any problem of selection of causes for evolutionary change. Man, although a difficult animal to handle in the laboratory, a difficult animal to analyze genetically, is in some ways also a very advantageous animal; and, as physical anthropologists, I do not see why you should not all be right in the forefront of studying evolution in the broad field.

MAJUMDAR: The matter of differential fertility and its relation to the gifted individual is a very controversial subject. When we read the literature, we find that no two biologists agree.

We are positive, you see, that when conditions change, there is a sudden rise in the frequency of the gifted population in the community. . . . Opportunity makes it possible for some members of the lower strata of society to ascend, and we find in India that in most walks of life, even in the profession of teaching, there are many people today who originated in the lower strata of the society. Thus the environmental aspects of the problem complicate the genetic point of view. I think that the environment may be more important than the genetic pattern, but this is a problem for further research.

CHAPTER XVI

Linguistics

Chairmen: Harry Hoijer and André Martinet.

Papers discussed: Lounsbury, "Field Methods and Techniques in Linguistics";* Schapiro, "Style."

Speakers: Hoijer, Voegelin,† Lounsbury, Forde, Jackobson, Kluckhohn, Kroeber, Rowe, Mead, Bidney, Lowie, Beals, Rouse, Martinet, Lévi-Strauss,† Nadel, Brew, Greenberg, Mandelbaum, Washburn, Haas, Held.

INTRODUCTION

HOIJER: Linguistics is often regarded as "marginal anthropology," partly because of its special and apparently complicated techniques and partly because there are so many linguists who are not also anthropologists. It will be part of our purpose to show that linguistics is only a specialized branch of anthropology, no more forbidding in its techniques and procedures than the study of social organization, technology, or economics.

Language is a part of culture, and its study a part of cultural anthropology; in several ways, notably in its emphasis on historical methods and its use of the concepts of pattern and structure, linguistics has much to contribute to the study of culture generally. We will concern ourselves here not with linguistic techniques per se but with the principles and methods which linguistics shares or may share with cultural anthropology.

TYPES OF LINGUISTIC APPROACHES

VOEGELIN: With respect to the place of linguistics in anthropology curriculums, one gains the historical impression that Boas in his training had a tripartite division of race, language, and culture, which shifted to the University of Chicago's bifurcate division of human biology and the rest of anthropology. Whether or not this is related—as Cole quoted in his reminiscences—to the fact that Sapir did not like physical anthropology, which he was inclined not to follow at Yale, and Cole did not like linguistics, I do not know, but the anthropology curriculums appear to have started out as a tripartite division and now are largely bifurcate. Nothing that I know of has gone on theoretically to change the division from tripartite to bifurcate, unless it is this matter which Hoijer discusses under the name of "cultural linguistics," that we will come to.

There is a certain theoretical reluctance among some linguists to be too closely allied with anthropology or to feel that language and culture are part of one and the same thing, because they want to analyze the structure of language in such a way that culture may be considered external to linguistics. There is a certain technical advantage in doing this. One alternative, then, is to consider language and culture as one, and the other alternative is to consider culture as external to language. These may not, of course, be the only alternatives.

Another possibility is to say that, in

the widest possible sense, linguistics is a part of human biology. The human animal is the only animal who speaks—indeed, the only one who can learn a human language—because of something built in the machine, in the same way that Von Frisch's bees have something built in the machine and can do certain things, or in the same way that birds have something built in the machine so they learn the songs of the parent-birds. If birds are switched from one nest to another, they will learn the songs of the other bird, but always songs. If human beings are switched from one place to another, they will learn the language of the other place. But you could argue in each case that something genetic enables them to do these various things. The ability to learn these things is built into the machine. That would follow our usual thinking about machines these days, when we try to construct machines to do things in order to understand ourselves.

Such problems could constitute a type of work which a linguist could carry on instead of the present state of analysis of languages and still be a linguist; in that case he would be closely allied with the genetic type of biologists. There is another sort of thing he could do, and that is, note that, whatever his language, the child at first does not speak to communicate, but privately, in social monologue, as Piaget has shown. I spent one summer with the Blackfoot, trying out the Piaget material, and found the same sort of monologue.

Rugotsky has suggested that this social monologue does not stop at age eight, as Piaget said it did. Can we assume that it goes underground, as it were, and reappears in the stream of consciousness? Harris is going to use some of the Rugotsky and other materials for his *Foundations of Linguistics* work, and I cite this merely to show that a linguist can work in this area—that is, as a psychiatrist, on the biological side.

I want to point out one other approach in linguistics which I see in modern nonquantitative mathematics. Some higher mathematicians think of themselves as being humanists rather than scientists. (The mathematicians in the engineering school want to be scientists.) So there is a possibility that the more abstract or theoretical side of language is best approached as part of the humanistic interests also.

Whichever of these possibilities seems the best approach, we must realize that linguists are as aware as ecologists that they can work with external factors but that they are behaving most characteristically as linguists when they work with the inwards of a language.

The comparative method continues as strongly as ever, and so does dialect geography, and linguistic probability genetics, as Kroeber's and Swadesh's and Greenberg's work shows. The external historical half succeeds in handling meaning. I am struck with how you can really handle meaning widely, narrowly, and all sorts of ways, if you follow it out historically; but when we take the internal or single-system half, we do not handle meaning too well, so far. We are beginning now, perhaps, but you get controversies; you generally follow Korzybski or Whorf, if I may say that without offending Whorf.

I have four queries. The first concerns the relationship between bilingualism and borrowing. If we are monolinguals, we speak one language; some of us speak more than one. I think it is useful to distinguish between what I would like to call "community bilingualism," such as you have in Switzerland, and "memory bilingualism," such as the Navaho veteran learning English and then refusing to talk it, but having some memory of it. In memory bilingualism you have some forgetting

of the language not spoken, but you have ample opportunity for either language to influence the other. You have never, or hardly ever, complete learning of the second language.

Community bilingualism is the same as being twice monolingual. Then you have the criterion of perfection, and that used by linguists is the monolingual staying at home and so on. The community bilingual will do that, too.

These observations of the relationship between bilingualism and borrowing have some cultural analogues. There is the possibility of having a bicultural equilibrium—a community in which each individual is equally at home in both cultures, so that, in terms of the Navaho veteran study, he would interiorize both cultures—but does he?

In so far as borrowing is concerned, my main point is again concerned with bilingualism. First of all, what happens linguistically in the acculturation situation? Obviously, borrowing. Ralph Beals says that few theoretical orientations come out of this—the study of linguistic acculturation—and this is true, probably because the explanations would come out of the precondition of borrowing, namely, bilingualism, which I spoke about; or theory would come out of something about structure; so you would have to take a whole range before anyone could get anything interesting. Now what you actually do get is not really uninteresting. You get a precision of statement.

Borrowing in linguistics, of course, corresponds to diffusion in cultural anthropology, but you can have a very simple formula saying almost everything that can be said about diffusion in language, as follows:

1. Borrowing Language imitates Model Language in all sounds (BL sound $<$ ML sound).
2. Borrowing Language combines Model Language and Borrowing Language after fashion or order of Model Language (BL = ML + BL $<$ ML).
3. Borrowing Language keeps its own morphemes; Model Language contributes meaning of homophone (BL homophone $<$ ML meaning).
4. Borrowing Language combines Model Language and Borrowing Language after fashion or order of Borrowing Language (BL = ML + BL $<$ BL).

I cite this to show that you can have such explicitness and comprehensiveness in this kind of linguistic statement. I agree with Ralph Beals, nevertheless, that this is not a theoretical orientation. It is just a way of compressing things, though a way which may be useful to preceding or following statements in theory.

The second query concerns the relationship between geographic spread of dialect features and differences in dialect between communities. When people say "a linguistic community," they do not mean a community in the cultural community sense, or the English-speaking world as a linguistic community; it is used in quite a different way, to mean something like the "isolate."

The third query concerns the different uses of closed corpus and open corpus as a point of departure for making transformations between one linguistic structure and another. I begin with Sapir's much-cited statement that the grammar of the lowliest African speaker is in essence as rich as French, and I ask, "How would you attack that?" I say, if you approach it from the closed-corpus point of view, you would have an awful job to show that Bushman is as rich as French. Quite a few philologists challenge this. If you do it with an open corpus, you can keep playing with it and playing with it, and, of course, you always end up showing that there would be equivalences between the two languages, and they would be just as rich.

There are some cultural analogues

of that for the closed corpus, and justification of it is coming back again into linguistics.

The final query concerns the relationship of the Weltanschauung theory —Whorf's meta-linguistics—and, the converse of this. If you take anything in the flora or the fauna, does it cause a language, as it were, to categorize it? For example, if in the external world you have a great cultural utilization of snow, does this then inevitably influence the language? You could do this in the reverse. As a matter of fact, you could take Hoijer's very clear exposition of the matter and start with the perception and then say, "With these perceptions in a culture, you would have this sort of language"; that is, you could work it back and forth. Boas, in the 1911 Introduction to the *Handbook*, stated a reversible meta-linguistics: he moved back and forth all the time.

CORRELATES BETWEEN LANGUAGE AND CULTURE

LOUNSBURY: Dr. Hoijer's paper on cultural linguistics sets forth the assumption that some of the work which has been done on what he calls "cultural linguistics" but which Mr. Voegelin has called "Weltanschauung" is basically trying to find correlates between linguistic behavior and nonlinguistic, cultural behavior.

This is something which many anthropologists feel is extremely important. Somewhere along the line, students pick up almost an emotional sort of dedication to the proposition that there is some sort of significant relationship between the structure of language and the structuring of other forms of cultural behavior.

I have no such emotional dedication to the proposition . . . nevertheless, I do feel that this is an important line for investigation. It often happens that people with intuition can get some ideas, see some problems, and open up a whole new area of inquiry, even though the particular things which they think and say on that particular subject may prove untenable.

This is the way I feel, for example, about what Whorf wrote and about what Dorothy Lee wrote; much of what they said is untenable. Basically, as I recall, Dorothy Lee puts it this way, that underlying cultural behavior are certain fundamental premises about the nature of the world, from which cultural behavior draws a sort of logical validity. Well, one might argue that. Then she says that underlying language structure are also certain premises, a sort of a world view. These are one and the same with the premises which underlie culture. Therefore, she says, by studying a language, one can discover significant things about the culture. I would disagree with her proposition there. I do not believe that you find any such ready connection between the sort of things having to do with what Sapir called "concrete relational classification" and so on, and other aspects of cultural behavior. But there is something to the problem, after all.

Both Whorf and Dorothy Lee have dealt vaguely with what they call "language structure." This is not what the linguist calls "language structure." What the linguist means by this term is the structure of the form of the language—phonemes, morphemes. What they are talking about is not formal structure but semantic structure. One thing which has not been made clear in the writings on this subject is that there are two levels of classification in language.

It has been stated repeatedly that languages classify experiences differently, and somehow this is felt to have significance. Let us look into the kind of classification which is made.

There is a primary level of classification, the classification of objects of one's environment, of one's experiences,

and so on—of nonlinguistic phenomena into lexicon. Certain languages classify colors differently from other languages; some languages lump blue and green together. The same way with kinship and with items of the natural environment and experiences. All this is the primary level of classification of experience into lexicon.

Then there is a secondary level of classification of lexical items themselves, or of whole propositions, into obligatory frameworks of classification of the type Sapir called "concrete relational categories." These are our gender, number, tense, aspect, and so forth.

In the writings on correlates between language and culture, this secondary classification into the obligatory concrete relational categories has occupied the center of attention. This is unfortunate, because it is so easy to disprove or to quote examples which would argue against the proposition. For example, Whorf makes much of the point that Hopi has an aspect classification rather than a tense classification; therefore, their concepts of time must be different; therefore, if they were to develop a physics, let us say, it would turn out to be different from the physics which has developed in Indo-European languages, and so forth.

All these concrete relational categories are assumed somehow to have correlates in other forms of behavior. Obviously, the question of relationship is meaningless unless you can put it in terms of correlates in other kinds of behavior.

In some cases it is quite easy to find things which, in the single case at hand, look like correlates. In Korean there is a five-way obligatory inflection in verb endings, and you have to choose one or another of those five endings every time you use a verb, or at least once in every sentence. These endings specify social relations in terms of distance, direction, and so on between the person speaking and the person spoken to, so that, even if I wanted to say, "The house burns down over there" or "The house is burning over there," I would have to choose one or another of these endings, depending on the social relationship between myself and you. If we look into Korean nonlinguistic behavior, we find an emphasis on social stratification, social distance, etiquette; and we might say that this is a correlate. Yet there are plenty of examples where you can find no such correlate.

In Oneida there is a gender classification in the third person in the inflection of the verbs and nouns. There is a masculine gender, then there is a gender which is used in referring to young girls and old ladies, then a general or indefinite reference to human beings—for example, somebody, a person, etc.—then there is another gender which is used for women of childbearing age and for animals. One might ask, is there a nonlinguistic cultural correlate to this?

I spent quite a bit of time among the Oneidas and never was able to discover anything that could conceivably be construed as a correlate to this particular classification. I assume, therefore, that it either does not exist or, if it does, is something so subtle that it would take some very sophisticated psychological test to discover it.

My own theory is that the secondary level of classification, in terms of concrete relational categories, probably usually has a correlate when this is an innovation in language but, being mixed up in intellectual systems, may long outlive the functional, nonlinguistic conditions that gave rise to it. I could even speculate what the factor was that gave rise to this classification in Iroquois.

However, I am much more interested in inquiring into the primary classifi-

cation made in the lexicon, which I feel is more significant and more likely to be actually correlated with nonlinguistic forms of behavior. Take a number system, for example—I am more interested in what the limitation of lexicon is than I am in what the concrete rational categories had to do with the subject.

In the Amazonian region and in the central highlands of Brazil, in languages where I have seen any documentation, there is a word for 1 and a word for 2, and beyond that there are linguistically complex circumlocutions.

Now this does not mean that they cannot conceive of higher numbers. It does not mean that they cannot express higher numbers linguistically. They can. For example, in Bororo, the phrase for 3 means, "This 2 and this 1 which lacks a partner." It is interesting to me, by the way, that a Carib language, away up on the Rio Branco north of the Amazon, makes use of the same circumlocution for 3. Four is "This pair and this pair"; five is "This pair and this pair and this one without a partner"; and so on.

What kind of limitations do lexical resources of this sort impose upon thinking? Obviously, we can say that in Bororo arithmetic, which consists mostly of a single kind of proposition—"One and one make two"—you would have not equations but identities. If you say, "Two and 1 make 2 and 1," you are not saying anything new, as you are when you say, "Two and 1 make 3."

Furthermore, it means that ordinary thinking about these subjects becomes difficult, because the linguistic expression of such concepts is grammatically unwieldy. . . . We find this in the development of scientific technical terminologies. We may have a concept and express it linguistically, but we can do productive thinking with it only when we get a simple grammatically manipulable substitute for it, so that we do not have to deal with clumsy circumlocution.

I feel that the investigation of subsystems within the lexicon is a more profitable line of investigation than the investigation of the concrete relational systems of classification. In kinship, we have one of the best examples. I can do . . . a semantic componential analysis of a semantic system in a manner quite analogous to the way in which we do a phonetic componential analysis of a phonemic system. When we get such a componential analysis, we can find correlates in nonlinguistic cultural behavior much more readily than we can in dealing with these others.

I realize there is still a good deal of interest in the concrete relational classification, and there is another whole aspect to this: How does this influence perception? I would be willing to say that the difference in lexical classification and the difference in phonetic classification on this componential level go back, perhaps, to one and the same fact from perception psychology—that perception is not given simply by sensory receptors but has to be learned. We learn to respond to certain clues in stimulating situations and to ignore others; and it depends upon which we respond to, differentially, and which we ignore, and respond to, equivalently, that we get different classification systems, for both chronology and in semantics.

COMPONENTIAL ANALYSIS OF KINSHIP SYSTEMS

FORDE: I wonder if Mr. Lounsbury would give us the reference to his work on the componential analysis of kinship systems?

LOUNSBURY: I have an unpublished paper which is to be given in Indiana this summer, and the one that I referred to of Ward Goodenough's in the series,

"Yale University Publications in Anthropology."

There are two ways of defining a class: either by naming . . . the members of the class or by stating the defining features of the class. In phonetics we do it both ways. We may listen to allophones of a phoneme, or we may listen to defining features of the morpheme, which automatically would give us any member of the class. Just so in kin classes, anthropologists, unless they are dealing with a very simple Hawaiian system where it is quite easy to define the components, generally define the class by naming the members in terms of kin types. Yet it is possible also to redefine these classes in terms of semantic components. [For illustration, take] the nine inherent differences, as Professor Murdock calls them, which derive from a publication of Professor Kroeber's. In working with any one system, you cannot take those nine as they are. You have to discover what the basis of classification is for a given system. For example, in the Pawnee system, which is of the Crow type, one cannot deal with a thing like bifurcation as a semantic component. It does not work. It turns out to be something in the same dimension of variation as the distinction between consanguinity and affinity, and you have to redefine a component which fits the given system and which you can use in the system.

FORDE: You mean that where, for example, in a Hopi system, you override generations with regard to the major kin of a person's father, the term "son" would be defined as member of the father's matrilineage? You mean something as concrete as that, or are you dealing with the more abstract?

LOUNSBURY: You have uterine kinsman as one, or agnatic kinsman, or, in other systems, you can take something broader, such as consanguineal.

FORDE: Are you not projecting into your linguistic analysis the cultural data which you are later going to find correlated?

LOUNSBURY: One might or might not. One might make use of hints from it in order to know what to look for. As I worked out the Pawnee case, it was just dealing with what was given in the vocabularly, and definitions in terms of kin types by itself. I admit that I did get a couple of hints from the marriage system which Alexander Lesser once described, but mostly it was just trying to find what components would work and what would not, as defining features for a class.

LEXICON MEANING AND GRAMMATICAL MEANING

HOIJER: I am interested in the point made . . . with respect to syntactic or structural categories. It is difficult to give these meanings in terms of their cultural implications. However, I find it mystifying to regard them simply as formal classes, with no meaning at all, just going on, as it were of themselves and given presumably by some mystical force.

JAKOBSON (statement revised after session): I agree with Dr. Lounsbury that for any semantic analysis a consistent distinction between lexical and grammatical meanings is of great importance. But in my opinion the significance of the categorized grammatical meanings for the cultural behavior of any given speech community should not be minimized. The compulsory character of these categories, as pointed out by Boas, is of consequence. A Russian speaker must connote with any verb whether or not it implies the concept of completion. Whereas in a language without the dichotomy of perfective and imperfective aspects, one can, but is not obliged to, mention whether he conceives any given process

with or without reference to its termination. When speaking English, I am obliged to reveal whether I go to the friend or to a friend, while in Russian, devoid of articles, I can, but need not, expose this distinction. The grammatical pattern creates a hierarchy of items on which we are supposed to focus our attention. This does not mean that the speaker cannot convey anything whatsoever in any language whatsoever. No code may hamper the cognitive level of the message. The cognitive function, however, is not the sole function of language, and in jest, in rhetoric, in poetry, in dreams, in magic, in what I should call everyday verbal mythology, the grammatical categories carry a high semantic import. Even such a category as grammatical genders, often cited as particularly formalized, plays a great role in the attitudes of the speech community possessing them. In Russian the feminine cannot denote a male person, and the masculine cannot specify a female person. The ways of personification and metaphoric interpretation of inanimate nouns is strongly indicated by their gender. When as a child I read a German fairy tale in Russian translation, it seemed absurd to me that Death, obviously a woman, was represented as an old man, since "death," masculine in German, is feminine in Russian. A test in the Moscow Psychological Institute showed that Russians with an inclination to personify the days of the week, inevitably represented Monday, Tuesday, and Thursday as males and Wednesday, Friday, and Saturday as females, without realizing that this is induced by the masculine gender of the former three against the feminine gender of the latter three. The difference in the Friday ritual within the folk tradition of different Slavic peoples is promoted by the difference in gender of this calendar term. The widespread superstition that a fallen knife announces a male guest and a fork a female one is impelled by the masculine of "knife" and the feminine of "fork" in Russian. Since Russian has no copula of present tense, the formula "Deus bonus est; ergo Deus est" loses its point in Russian, and hence the efficacy of this argument was vitiated in Russian theology.

KLUCKHOHN: Professor Lounsbury's remarks seem to obscure a point of view of culture generally . . . and that is, when one deals with lexicon, one is dealing with the things which the speakers of the language and the carriers of the culture have raised into vocabulary status, so that they are conscious of it, and one can see in certain well-documented points of history when and where this came about.

If we are interested in covert culture . . . the morphology of languages and projective tests are our best hopes for getting at this passively perceived, this unconscious—in the strict Watsonian sense of being unverbalized—view of experience, which is inevitably there, because of the obligatory nature of these forms.

KROEBER: Kluckhohn's statement implies a premise that things that are unconscious must have a great deal of effect, apart from the area in which the unconsciousness exists. That may well be. Certainly, since psychoanalysis, we have a sort of a prejudice in favor of the strength of the unconscious influences over the conscious. However, I do not think we can rationally assume that premise as general, and it may be that in this case the unconscious factors are not particularly influential outside language, just as well as the reverse.

I cannot agree entirely with Jakobson either, because what role in life do these things play? He points out that 40 or 90 per cent of Russians construe Monday, Tuesday, and Thurday as male when they personify, and the other days as female because they are

feminine in gender in Russian. What has that to do with the role of the weekdays, which organize our time and tell us when we have a day of rest, and which enter into our behavior in thousands of other ways? It seems to me that he is picking out a rather special case, which undoubtedly holds true, and I would certainly not deny that unconscious categories of language can have some effect, but I think it important that we do not look upon this question as an all-or-none one—that, because there is something in Whorf's meta-linguistics, we therefore believe it is all-important and dominant in all our behavior. Or, on the other hand, if we feel that Whorf has gone too far, we should not say that there is nothing in it and dismiss it. It is a question of how much.

I am inclined to think there may be some influence, but that it is a minor influence. I agree entirely with Lounsbury's point that what Whorf dealt with was not structural or grammatical features that seem correlated with culture and cultural behavior but semantic features that seem to correlate.

Semantics, the meaning of language . . . is culture. That is, of course, an overstatement. It is not merely culture-and-not-language, but it is that part of language which is most nearly cultural; and consequently it is in semantics that, if there are influences and relations, you should expect to find them, and there they do seem to occur.

Certainly, Whorf's suggestion is stimulating and may be important, but it remains mainly unsubstantiated.

ROWE: Procedurally, it is going to help call attention to the distinction between lexical and morphological aspects of classification. The lexical type of classification has been rather neglected in language and culture studies, except as it crops up in the practical work of translators. Anyone who uses a foreign language soon finds himself comparing meanings in one language with meanings in another.

One way of breaking down the lexical comparisons would be to consider in one class those which can be compared on the basis of area of meaning; that is, for example, the fact that English has one word for "wall," where Spanish has two. You can make the same distinction in German that you can in Spanish, but we do not normally make it in English. If we do, we modify it by phrases.

Some measure of the difference in the way in which the outside world is classified in language can be reached by examining cases in which you can compare area of meaning of words in one language with area of meaning of words in another. Second . . . there is comparison of metaphors, and I have the impression that metaphors sometimes get credit for more influence on people's thoughts than they actually have. Nida points out that the metaphor is used in discussing time in Inca and in ancient Greek and gives the impression that the Inca or the Greek considered himself facing the past, with the future behind his back. However, depending on which expression you pick in English, you can say the same thing about our relations in time in English. When you say "before," meaning the past, you are suggesting that you are facing the past. And yet there are other expressions where we speak as though we had our backs to the past, and I do not think that this particular type of metaphor is of any great influence in the way we look at time, and I doubt very much that it was to the Greeks or the heathens either. . . .

DIALECT COMPARISON

MEAD: One point seems to be neglected, and that is the advantage of comparing dialect.

There is a difference between, for instance, the Irish version of English

and the southern English version of English, and, say, the northern English version of English, or English as spoken in the American South and English as spoken in New England.

If you take a sufficiently large sample and then compare such matters as the preferential use of transitive and intransitive forms . . . that sort of difference is very sharp if you pay attention to English as it is spoken in different cultures.

A curator in one of the museums in Tasmania has been working on the disappearance of passive forms in contemporary Tasmanian English; these can be correlated with a good many other features of adult Australian culture. Here one deals with usage, and it is not necessary to postulate the original, grammatical forms. They represent something that exists in English culture today. You take your language as given and then see how it varies, corresponding to other cultural variations.

We do the same thing with the myth, very profitably. If you take a myth like, say, Jack and the Beanstalk and compare it European version with its American version, see what happens to Jack. He used to be a little boy of four or five, who dealt magically with situations, and ended up with his father nicely eliminated, playing the harp to his mother on a long summer evening. This is the characteristic European version, as compared to the American one, in which Jack is a ten- or eleven-year-old boy, his mother sends him out to the beans to get him out of the way and get something done, and he overcomes the giant and goes on with life in a quite different style. And in a very modern radio form the giant's wife, who in the European version was a mild and friendly character, has become a terribly dangerous, unattractive female.

What is important here is that Jack and the Beanstalk, when it was first made up, might have had a precise and beautiful correspondence to the theme of a given culture at a given time. It then traveled and took on all sorts of forms, which you study and correlate with the contemporary cultural usage. That is one point.

The second point is that we do not know enough to extrapolate into other kinds of behavior. If you say that here is a child learning some things linguistically in a society, some things in motor terms without linguistic support, some things almost in autonomic terms without linguistic support, some things in motor, linguistic, and autonomic support simultaneously—what is the effect on the individual when the linguistic category is contrapuntal to the motor category?

If we only can understand that the entire communication between older and younger and peer groups is learned and can take all the sensory modalities into account and see to what extent they reinforce or are contrapuntal to one another, then we will begin to tackle this problem, which presently gets tackled by an elaborate analysis of one sector without any relation to the others.

BIDNEY: I simply would like to indicate that, from the philosophical point of view, the primary role which is assigned to semantic structure, i.e., semantic classification, involves the theory of cultural idealism, which limits our social reality to cultural reality. The moment you deny this basic premise that the only reality we know is a cultural reality, the proposition that our culture is entirely influenced by our linguistic structure falls likewise.

LOWIE: I should like to call attention to the fact that in literary French a certain tense is used for expressing a past event that would never be used colloquially. In speaking, a person would only say, "je l'ai vu," but in writing, he

could say "je le vis." In north Germany, even a relatively uneducated person would use the imperfect tense, whereas in south Germany and Austria, it simply does not occur colloquially. One does not say "Ich sah," but "Ich habe gesehen."

ON THE CULTURAL EQUIVALENT OF THE PHONEME

BEALS: The relation of what Mr. Lounsbury called "compulsory" categories has some bearing on the question of perception, and I think this has considerable bearing on the relation of the individual to the world and to his own culture. A good deal of work is being done by the social psychologists on the problem of perceptions. Some work shows clearly that what the individual sees in a motion picture is not what is in the motion picture but what he brings to the perception of that motion picture, and you can multiply this example through many aspects of culture.

I am not saying that all these perceptions are created by compulsive categories in language, but it seems to me these must have some part in the process.

I want to mention the point in Mr. Lounsbury's paper in which he refers to some of our discussions of acculturation. . . . It seems to me that the problem of how to relate this to cultural problems, such as diffusion, lies in one simple thing: What is the cultural equivalent of the morpheme, or one we can agree upon?

LOUNSBURY: I have heard that question asked by anthropologists very often, and I have always felt that they did not have a clear idea about what sort of unit a phoneme is.

Let us think of it in very simple stimulus-response terms. First of all, any situation contains many more features than may be responded to by a responding organism. Let us think of the difference between a defined situation and an undefined situation. A defined situation would be one which contains some features to which the organism in question has learned to respond in some sort of consistent, predictable manner. An undefined situation would be one which among its infinitude of features contains none to which it has learned to respond in this manner. If it responds to such a situation at all, it would be in a random and unpredictable manner.

The linguist starts with phones. A phone is a unique event. It happens but once, and, like unique events, each is different from the rest. It contains many phonetic features. To some of these features, hearers will respond differentially; to others, they will not. Dr. Kennard gave a nice example when he spoke of the Hopi children's question, "You mean this kind of a *p* or the other one?"

Some linguists go directly from phone to phoneme. Others prefer to put the working phonetician in between and insert a category—phone type. Well, phones, we say, are unique and, upon instrumental analysis, are shown to be such; yet the phonetician with a trained perception does perceive some as identical; and just what the phone types are depends in part on the training of the phonetician, of course.

Phones, or phone types, are classified into phonemes on the basis of differential responses to them. Those phone types which never enter into contrast, which are never responded to differentially, can be classed into a single phoneme; so a phoneme is a class of things essentially different, but for stimulus purposes they are stimulus equivalents.

Now we go ahead further and define our class of phone types in terms of phonetic components, and these phonetic components of the phonemes are those particular features of the situa-

tion which are responded to differentially. For a given phoneme, we can take a bundle of them, and this defines the class; so we can speak of the phonetic componential structure of a phonemic system.

And then we go on and speak of a sequential communicatory structure, and that is how they combine with one another in the one dimension of time.

We do have analogues to just this type of situation. For any such class, any of the classes which end with *-eme* in linguistics, there are two classes of stimulus equivalents. Now anywhere in anthropology and cultural anthropology, where, in terms of some sort of response, whether it be linguistic or nonlinguistic or any other kind you want to fix your attention on, where in terms of such responses you can set up classes of stimulus equivalents, you have classes which are analogous operationally to the phoneme.

I have mentioned kinship before. Let us just come back. A given Ego, in his social environment, has kinsmen. They are unique. Then we have kin types. A given individual will have 6 primary kin types; 31 or something, secondary; and 157—is that the number?—tertiary; and so on, and a kin type is a class of kinsmen which for certain purposes are stimulus equivalents. And what are these purposes? Linguistic response—no, this is for the anthropologists' analysis. It is just like the phoneticians' analysis here. Then, further, the kin types are grouped into kin classes, and this is what the anthropologist deals with. This is the class of kin types which, for certain purposes, namely, linguistic responses of a certain order, are stimulus equivalents.

Now, we can go further and deal with the semantic componential analysis of the kin classes. These do not combine with one another in sequence, in the order of our phonemes. Yet they combine with other things in our language. We have to go outside this one neat little system which I have outlined here and deal with a larger system and then even speak of a communicatorial structure of a sort. . . .

Wherever in anthropology, for purposes of responses of a given order, whether they be linguistic, nonlinguistic, or anything else, you are dealing with stimulus equivalents, you have or can have units of the same order operationally as our phoneme.

ROUSE (submitted in writing after session):

LANGUAGE

1. Phone
2. Phone type
3. Phoneme
   a) Phonemic combinations
   b) Segmental combinations

MATERIAL CULTURE

1. Artifact
2. Artifact class } Some reverse these two
3. Artifact type } terms
   a) Combinations of artifacts = cultures
   b) .....

The above seems to me to be another example of analogy between linguistic categories and categories in other aspects of culture. I mention it because it brings archeology into the discussion.

JAKOBSON (statement revised after session): It seems doubtful whether outside of language there are real equivalents of phonemes, because the phonemes are purely distinctive means without any positive content. The difference between two phonemes functions as a mere indication of the difference between any two morphemes where these phonemes, *ceteris paribus*, occur. The question, "What does a given phoneme mean?" (e.g., the high, front vowel in English) makes no sense. The equivalences to other than linguistic patterns appear to begin with morphemes.

MARTINET: In relation to what Pro-

fessor Jakobson said, the main difference is probably due to the fact that in language there are two planes all the time. You have not only the phoneme, but you have the morpheme, and that makes the whole difference.

I am just wondering whether the difficulty would be not to find, as shown by Dr. Lounsbury, similarities between the phone and the kinsman, for example, in the whole series, but similarities between this dual pattern of language, this double articulation of language. Do we find it anywhere in the rest of anthropology?

LOUNSBURY: That is why I began by saying that we should put this in stimulus-response terms, and wherever you have stimulus and response, you have two things.

LÉVI-STRAUSS: I think we really have two levels. We do not have kinship systems only; they are of no use by themselves. A kinship system permits certain types of marriage exchanges; and I would say that a kin class is perfectly meaningless, as the phoneme is, except when you find out how the distribution of the kin into certain kin classes permits certain types of marriages and forbids certain other types of marriage. This is the semantic level.

MARTINET: Yes, but do we not come here to the problem of a game of chess, where there is an identity but no analysis. You cannot have double analysis in a game of chess, whereas we have it in language. Professor Jakobson pointed out that every meaningful unit in language can be submitted to a second analysis on the plane of nonmeaningful units, which are just distinctive components, and I am wondering whether you can find that.

The existence of a pattern of this sort will explain the marriage comportment of the community. But you do not have this necessity for a double analysis on the plane of pure description. I would say that when you use a pattern like this in anthropology, you are giving the system which regulates the behavior in that community; that is, you complete your description by means of a system like this, whereas in linguistics we have to have two successive descriptions, or two successive stages in the description.

NADEL: The phoneme is said to be a stimulus equivalent. An equivalent for what? What kind of response? And if so, what is the corresponding or analogous response to the stimulus equivalents of kin class?

LOUNSBURY: I know of a language where every prenasalized voiced stop —*d*—and flap—*r*—are members of the same phoneme. I say they are stimulus equivalents because the differentiation of meanings in the language never hangs upon the difference between these two sounds. . . . Or take an example from English. I am speaking English. I will say, with an unreleased *p*, "stop," or I will say "stop" with a released aspirated *p*. Those two words do not have different meanings—that is, within units of word length, under certain conditions. This is not too good an example, but I am trying to choose the next best one, from English. Distinctions do not hinge upon that particular difference. In a larger context, you can argue in English that they do, but I am just trying to frame an example here.

NADEL: This is a leading question. The point is that in your linguistic example these variations objectively recorded of phonetic sounds do not make any difference in behavior or response of the people hearing those particular phonemes or words. In kin classes this, I suggest, is not true.

JAKOBSON (statement revised after session): There is a fundamental difference between the phonemic and morphemic level. If we confront two pairs of morphemes, we observe that they may signal different items, e.g., the first

two morphemes denote opposite grammatical numbers, and the second two, opposite verbal voices. But when we deal with two pairs of phonemes, both pairs have the same merely distinctive function. When, other things being equal, two different morphemes familiar to us appear in two unknown words, we infer that the two words are different and that they comprehend, moreover, the semantic value of these two morphemes. When, however, other things being equal, we recognize two different phonemes in two words unknown to us, we infer nothing but the fact of difference between these two words.

BREW: Basically, this discussion presents one of the greatest difficulties we have in what you might call general scientific epistemology. It seems to me that the error here is in the question which Dr. Lounsbury said he is always being asked.

In archeology we have considerable difficulty because of the application of the Linnean classification to cultural objects, and it occurs to me that this is another example of it. Because a classification works very beautifully for a specific purpose, people want to take it over wholesale and transfer it to another type of phenomena, which it does not and cannot fit, by the very nature of its specialization.

FORDE: In your phonemic analysis, what you are dealing with, I take it, is a set of patterns of motor character, a bit of physiological activity, and we well know with regard to animal behavior that animals develop habit formations in which there are alternative muscular actions for dealing with the same situation, and you can train rats and other animals to deal with a particular response by one path or by the other, and they achieve the same result.

Morphemic analysis, on the other hand, rests on a level of association which is different and which involves a more complex psychological background.

Now, as between culture and social structure, the difference is in the level of analysis, and the great problem of linking the two together is the one which became most apparent when Malinowski pursued, to its logical inconclusion, the idea that you could have a one-for-one correspondence between a cultural item and a social function. I do not want to take up time in the discussion, but it seems to me that we have a parallel situation here as between phonemic and morphemic analysis. . . .

NADEL: If I understand you correctly, the phoneme is a stimulus equivalent when it is uttered by any speaker, the response being on the part of any hearer. If we take kin classes or, on a somewhat more general level, social role, which is much the same, it has not that general equivalence of stimulus. It implies that a particular individual, or class, is the stimulus equivalent to a correlative individual or class.

LOUNSBURY: The second point which Professor Nadel has raised is the difference between an absolute system of classification and one which is relative only for a given individual or given time or place. Professor Jakobson gave a paper at Yale in which he discussed the difference between didactic and nondidactic systems, the didactic being the relative one; so that that question does not bother me particularly.

The other question which Dr. Nadel raised had to do with this: Obviously, one does not respond identically to all the different people that one may call "father" in a given social system. That is why I said responses of a given order, namely, linguistic responses in this given case. If you choose another kind of response as a basis for classifying kin types, you would get quite a different classification of kin types.

I might point out also that anthropologists have consistently failed to discover that in almost every society there are alternative sets of kin terms. For example, a society may have an Iroquois type of kinship system; yet, if one looks farther and finds terms which are used in certain contexts, one will discover that the society also has a Hawaiian type of kinship system, and, if one looks in still another context, one will find linguistic responses which give a Siamese type of kinship system; that is why I said responses of a given order.

PROCESS IN LINGUISTIC CHANGE

GREENBERG: The term "process" seems to be of rather strategic importance here, and there have been numerous discussions of process, but at no point did language come up. Yet I think language is worth considering in this respect.

I am really trying to work out a kind of semantic point; probably most linguists use the term "process," referring to change in language over time, without making clear to themselves just why they call these things "process." I think they all felt that the term "process," as used in a number of different fields here, was really used very vaguely. . . .

Let us start by giving a number of terms which would normally be called "processes." If you take any general book on language or a handbook of historical linguistics, you will see such terms as "regular sound change," "analogy," "borrowing," "semantic change," and what not. If you enumerate all of them, you get an exhaustive description, in this sense: There is no change in language which cannot be put under one of these rubrics. Also, some of them obviously have analogues to culture; that is, the term "borrowing," in language, corresponds to "diffusion" in culture. But there is such a thing as an invention in language, when a new term is made up, and so on. There are a number of such equivalents.

We have greater precision than in culture in general, because there are a finite number of processes. Every change in language can be put under one or the other. It comes down again to the question of units. The fact that we have units such as the phoneme and the morpheme means that the change in language has taken place over time and can be expressed in terms of these units.

When we make a synchronic description, we make use of these units. Suppose a change has occurred in the language. We can look at it this way: We can compare our synchronic descriptions from two different chronological stages, and we can put one against the other and say, "What is the difference? What has happened to the description in the meantime?" Since the description was made in terms of these units, the difference must be statable in the same terms. It may be that a certain thing has dropped out, so a statement we made in an earlier description no longer has to be stated at all; that is, we can have, in a certain sense, loss. We can have replacement—replacement of one unit by another similar unit. We can have addition—something added that was not there before. It is this, really, which enables us to compare processes.

I found the dictionary definition of "process" not too enlightening. As we use this term in language, we mean the following: A process takes place in time, first of all. It is a historical diachronic concept. It involves similarity; we would not call something a process unless we could find other instances of it. In this case we can ask ourselves what the similarity is, and the similarity is that it refers to the same unit and to similar process, either replacement, addition, or what not.

What is more, when we look at these things, we can and we might think of

it in a number of what one might call "axes of change"—that is one contrast that we can make, and it has an analogue with culture as between external and internal. In language, this is very neat. When something new has come in, whether replacement or addition, we can ask whether it is made out of linguistic material which previously existed in this system. If it is not, usually look somewhere else, and we find that in some form or other it is in a neighboring system. In that case we can make a distinction between internal and external.

In regard to linguistic change in form or meaning, there are some changes in languages which refer to form only. Regular sound change would be the classical instance of that. There are some changes which refer to meaning only—a simple shift in the meaning of some term, semantic change, without any change in the formal expression used.

A third thing might be the degree of novelty involved. The least degree of novelty is simple persistence, and I think this has not been realized. The most important process of change is persistence, because you take a language at one period, and in another period, not too far off chronologically, most of the statements are identical, and this requires an explanation just as much as if you replace something with something else. So you might say the one extreme pole is persistence, but very near it is simple, regular sound change, which only changes the phonemic units in a regular way, without changing anything else.

The most marked novelty occurs when a few morphemic units is made up out of whole cloth—and I took it for granted that this process exists, but, in talking with Dr. Lounsbury the other day, he was surprised that I assumed so. We certainly admit it exists in English. I think he was surprised because I assumed it was universal. This is coinage, just making up a new term as you might do in slang; but even in this case we can see a neat example of the importance of cultural pattern, because, let us say, some new term is made up in English tomorrow; there will still be certain things I can predict about it. There is a bond with the past. It will certainly not contain a phoneme which does not exist in English already. It will probably not contain any sequence of phonemes which is not in the pattern of English up to now.

Of course, the term "process" is also used for some more widespread things, such as drift. "Drift" is another one of those ambiguous terms and has been used in two different ways: one, to refer to coherence of change, and the other to refer to the fact that we may assume in some cases that change is conditioned structurally. That is, that there is in the structure of a language something which makes the next thing come along in a certain way. We take a process like sound change. We can subdivide it into smaller elements; we can compare the change of *b* to *p* in a language in one part of the world with one in another; but it is also true that we sometimes see a kind of wide-scale change. Grimm's law would be an instance of that, where a whole series of phonemes changes in a kind of coherent way. You get the same thing in a morphological system.

The loss of case system in Indo-European languages is a kind of comprehensive process which takes place over a long period of time; and so we say the drift in the Indo-European languages is toward the loss of the case system. In that case, we use the term "drift" for some such comprehensive changes.

Another way is this matter of immanence. It is very easy to mix up the

matter of immanence with how you test immanence, and I suppose it is all right if you do it. Sapir, in his book *Language,* was really testing a hypothesis of immanence by taking, for example, English and German. He says they started out the same way; they were the same thing. Hence, if there are any similarities at the present day, they cannot be explained by the external circumstances, which were different; at least, that is the plausible hypothesis. Hence, if there were certain similar or analogous or identical changes in English and German, these took place because there was something immanent, there was something in the structure originally Germanic that made similar changes possible, and these are two different things.

Now, you might say, what is the use of all this? It is very beautiful. In a way, you get all these processes, which can be described with much greater precision, though they are similar in most ways to cultural changes. As a linguist, instead of priding myself on how beautifully these can be stated, I precisely want to think drift is impressive in the sense of this differential immanence change. But the important thing is that there are a lot of things that cannot be explained in this way. English and German are still different. Just starting out with the structure of German, proto-Germanic, is not enough to tell you what English or German would be.

In the same way, we may describe something like Grimm's law, involving comprehensive changes in the sound system, but nobody could have predicted beforehand that it would take place. It is just a kind of generalized statement. In other words, in linguistics, we can make highly generalized statements in certain areas of change, and we have a comprehensive system of classification which enables us to compare changes in different languages in different parts of the world, because they come under the same general rubric of change.

This is one reason why I think the linguists cannot remain in their linguistic foxholes. If you just describe language in terms of certain elements which are arranged in certain orders and so on, fine; you have to do that. That is the beginning. But it is obvious that, since immanence or structure does not completely explain change but only a small part of it, there must be nonlinguistic factors involved—very likely, a number of different factors. It is getting like genetics in a certain way; that is, if I took people speaking the same language and put them on two islands and said, "No one on this island can ever speak to anybody on that island," I know that in ten thousand years not only will they not understand each other but not even a linguist will be able to prove that these languages had a common origin. Now, why did they diverge so? Unless the linguist can explain why linguistic communities split along certain lines, why the languages are distributed the way they are, he has not fulfilled his task. I maintain that linguistics is the science of language; so the linguist must strive to explain linguistic phenomena. He seems so impressive to the nonlinguist because he tackles only the easy problems.

His work is aesthetically beautiful. I appreciate it from that point of view. But we are really only in the beginning of linguistics, and there is no reason why the cultural anthropologist has to hold his head in shame in front of a linguist. His achievements are impressive because the subject matter is so nice to deal with. I think we are one step beyond in linguistics, that is true. We have these processes; we can describe them in an objective way. But

what we do not know we cannot predict; we can reconstruct, but we cannot predict. That makes us a respectable science, about the level of geology, let us say.

But these nonlinguistic factors have to be investigated. The linguists have not really begun to see what is involved there, and for that reason all kinds of things outside language are eventually going to be brought in, before we can really explain what happens in language. I might say I am very poor in hypotheses in such matters. For example, geographical environment is an old warhorse in this matter, and it would be easy to show that it has probably very little relevance. It is unlikely to explain any particular change in language.

MARTINET: Another important aspect is the influence of one language on another, and this is, of course, strictly the domain of the linguist. Linguists have not been bent upon explaining too much—of course, there are a few theories, like the substratum theory, which are meant to explain linguistic evolution. But I have a feeling that we would find that the important nonlinguistic factors in linguistic evolution have been overrated. I would add that when Dr. Greenberg spoke of the two islands and said that the two populations would speak different languages —well, they probably would. But I do not suppose the thing really has ever been realized, at least in such a way that we could be sure that the islands were not influenced by people from outside, which might account for the divergencies between the two languages after, let us say, two millennia.

GREENBERG: As far as the influence of other languages is concerned, we presume that they will not produce similarities. As a matter of fact, I thought it was provided for in the framework when I said that there were external sources of change. These are linguistic, although again we can just say that a certain thing was borrowed.

This means we can classify them in a certain set of changes; we can compare borrowings in different languages, in different parts of the world; but we know very well that, when we start to do that, we begin to bring in other cultural circumstances. We want to know why people A borrow words altogether from B and what particular words they borrow, and that brings up such questions as the varying cultural levels of the two people and the kind of contact they have had.

In other words, I think linguistics is very important in the acculturation situation, both present and past, and that it does bring in nonlinguistic factors. I think there is no real disagreement. We admit there are linguistic factors and nonlinguistic factors. Perhaps the linguist had better tackle linguistic factors first. He has not finished that yet, I know.

MANDELBAUM: The analogy between linguistics and geology is valid; also, an analogy might be made between linguistics and the present state of a large branch of economics. In economics there is very powerful, logically consistent theory which does not have to be particularly pertinent for most purposes of the economists. The same thing may be said in respect to linguistics.

I am not clear as to what is meant by "one language influences another." It seems to me that that must be mediated through people and that introduces a number of extra-linguistic factors.

MARTINET: We probably have to think that language cannot influence another language except through bilinguals. To what extent culture, excluding language from culture, enters this picture is, of course, a problem, but it really boils down to the problem of bilingualism.

LOWIE: I should like to revert to the

concept of drift, which has been bothering me ever since I encountered it in Sapir's book on language. It seems to me that there is an analogy here between drift and orthogenesis in evolution. A mystical flavor clings about the concept of drift, as it is used in Sapir's book. But I am impressed that there are undoubtedly certain linguistic correlations. As in culture, so in that subpart of culture which we call "linguistics," certain aspects are functionally correlated, and one might deduce, then, the occurrence of a particular phenomenon from the occurrence of some other phenomenon. Sapir gives illustrations—I think in connection with the recurrence in quite unrelated families of such a phenomenon as the dual number. . . . He seems to suggest that with this phenomenon there would be other phenomena.

WASHBURN: I would like to go back to this question of the condition under which people borrow, and present a case that Dr. Greenberg and I have been discussing.

How did people like the Zulu get the clicks in their language? The people they apparently got them from have a much less elaborate way of life than the Zulus. If we go into the Zambezi Valley and get into the area of high civilization which is described as Bantu by Caton-Thompson, and then look over the skeletal descriptions by Galloway, we find that the skeletons of the people were not Bantu at all but Hottentot. If we take aerial photographs of this area and look at the terracing and extent of this civilization, and charts of the number of mines that have all been done elaborately, one gets the picture of a high Hottentot civilization in this area. I am now extrapolating from two small series of skeletons; but, if it is true that the Hottentot people were a people of high culture, then it is clear under what conditions this borrowing could have taken place.

It seems to me that it is in this kind of situation that language and culture and archeology are all working together at the historical level. If the Hottentot premise is true—and it needs much more archeology to prove it—our whole conception of the recent history of the southern part of Africa would be changed, and the racial situation would be much clearer. The present situation does not make sense from a biological point of view.

One remark on drift from the point of view of anatomy. Some of the sound changes presented are, to anyone acquainted with the structure of the mouth, the easiest changes, anatomically speaking, that can be made. If one has a fashion, it is the fashion that the next easiest thing is determined by the structure. I suggest one reason that drift may be peculiar is that perhaps not enough consideration is being given to the form of the animal which is making the sounds.

I have been a little discouraged by the hopelessness of how far back we can carry language. If a person is anesthetized and his brain cap is taken off and he is under local anesthesia so that he can talk, you find out about his brain in a way that we never knew before. One discovery that has come out is that aphasia can be produced by stimulation anywhere on the dominant hemisphere and not on the nondominant hemisphere. The dominant hemisphere is anatomically larger than the nondominant hemisphere, so that the language ability is associated with one of the very few things which can be found in an endocast. Now we do not have, since this is a new finding, enough casts to know the distribution of these asymmetries. But if we found symmetrical hemispheres through to the middle of the Pleistocene, and after that asymmetrical hemispheres and combined knowledge, tool-using, and so on, I think one could make out a much

stronger case for the role of the dominant hemisphere, not only in understanding things like stuttering but in getting some kind of guess that perhaps these people really had some of the attributes of speech.

GREENBERG: In regard to Hottentot, it is not just a question of clicks. After all, the clicks are in words, and I want to see what kind of words are involved. I was very puzzled when I classified the languages in this area, that Hottentot is not even a genuine large subdivision of the Khoisan languages, which are a portion of a larger family of languages which have clicks. There are northern languages which are all Bushmen; there are southern languages which are all Bushmen; there are central languages which divide into two subdivisions, one having two languages and the other containing two subdivisions which are Bushmen and Hottentot. In other words, the Hottentots are a subdivision of a subdivision of a subdivision. Now this suggests, if you also look at the cultural and physical material, that the Hottentots are just Bushmen who have cattle. That is the thing that is essentially different about them, and a lot of things go with that. The question is, If they did have the cattle, where did they get them? I answered it in a simple-minded way. I think it is a good illustration of what the linguists get from archeology or physical anthropology. In this case, they got a very nice hypothesis based on the archeological data that Washburn has indicated. I thought the cattle terms should be borrowed into Hottentot from Zulu; that is, they must have gotten the cattle, which the other Bushmen people do not have, from the Zulu. It turned out to be just the opposite way around, that the Zulu and other Bantu-speaking people got cattle from this high Hottentot civilization; that the Bushmen are just benighted people of this group who were pushed into the Kalahari Desert, where they could not keep the cattle. As a matter of fact, there are words with clicks for cattle in the Bushman language, too, in spite of the fact that they do not have cattle at the present time. All this is very tentative. I would not want anybody to say this was true as yet.

JAKOBSON (statement revised after session): The interlingual aspect of linguistic changes is a most pertinent problem. This notion might be directly deduced from the fundamental definition of language as communication. The primary tendency of communication is its diffusion, so that the communication becomes still more generalized. One of the most harmful scientific isolationisms has been intralingual isolationism. Because, in point of fact, there is no "iron curtain" between languages. Whether two peoples are at peace or at war, cold or hot, linguistic interrelations continue, since in the latter case it is necessary at times to spy, at times to discuss and negotiate. When interlingual relation exists, then there occurs perpetual accommodation, a tendency to find a common code; and there emerge various forms of bilingualism. In particular, we face the problem of the diffusion of phonemic patterns and grammatical categories. Again I must mention the pioneering importance of Boas' observations in this respect. Parallel efforts were made in the interwar period by European linguists, and it is a great pity that the preliminary work on a world atlas of linguistic structures, begun in Olso, was interrupted, only temporarily, I hope, by the German occupation. We began there with the mapping of the distribution of relevant phonemic phenomena in Europe and a part of Asia, and we were struck by the fact that a great part of such phenomena does not stop at the boundaries of a single language. We observed, for instance, how around the Baltic Sea languages

of completely different origins developed, through different ways, the same traits in phonemic and, especially, prosodic patterns. Certain properties display a wide capacity of diffusion which was frequently overlooked only because of our usual regional, particularistic attitude toward any given language or language family. Morphological and syntactic studies reveal a similar picture. Such a world atlas, dealing with various structural linguistic phenomena spread irrelevant of the language boundaries, is one of the most urgent problems both for linguistics as part of anthropology and for anthropology in general, and it promises, in particular, to throw a new light on the study of linguistic typology and mutations. Coming back to the problem touched upon by Dr. Lounsbury—the essential difference between the lexical level and that of grammatical categories—we must state that, for the most part, the mass diffusion of words is a part of the expansion of higher cultures and stronger societies. This does not, however, apply to the diffusion of phonemic and grammatical structures. Here it is much easier to state the affinity than to interpret its origin. In many cases the diffusion of structural phenomena goes from culturally, politically, economically weaker societies to the stronger ones, as, for instance, the expansion of glottalized consonants from the languages of the Caucasian mountaineers to the Armenian, Iranian, and Turkic languages of the Caucasus shows. Ruling and expanding nations often borrow structural features from the languages of the peoples they aspire to dominate, making, in this way, the language of the dominators more available to the "dominees."

MINIMUM UNITS IN VARIOUS ASPECTS OF CULTURE

LÉVI-STRAUSS: Dr. Hoijer has asked me to discuss whether, aside from language, aspects of culture, such as social organization, religion, technology, etc., can be described like language in terms of minimum units and their relations? I shall try to investigate relative structurability on these different levels.

My first example is taken from biology. I would like to call your attention to a paper which was published by Dr. Ledyard Stebbin, Jr., in 1949.[1] In this paper, he considered more than four hundred subdivisions of the angiosperms, and he showed that it is possible to classify exhaustively all these four hundred subdivisions by using eight distinctive features, which are:

1. The presence of calyx and corolla, versus one or the other, or none
2. The petals, separated or united
3. Corolla, regular versus irregular
4. Stamens, numerous or not numerous
5. Carpels, separated or united
6. Ovaries, numerous or not numerous
7. Eggshell presentation or different form
8. Upper ovary versus lower ovary

He showed that this makes 256 possible combinations, out of which only 79 are being realized, and that all the families of angiosperms can be classified according to these 79 combinations of eight differential features.

Now I shall immediately pass to a different level and cite a passage from the paper on style by Dr. Meyer Schapiro. The passage refers to a book by a German historian, Wölfflin, and Schapiro explains that, in analyzing the art of the High Renaissance and the seventeenth century, he devised five pairs of polar terms, through which he defined the opposed styles of the two periods. These terms were applied to architecture, sculpture, painting, and the so-called "decorative arts." The linear was contrasted with the picturesque or

[1] Ledyard Stebbin, Jr., "Reality and Efficiency in Selection in Plants," *Proceedings of the American Philosophical Society*, Vol. XCIII, Part VI (1949).

painterly; the parallel surface form with the diagonal depth form; the closed with the open; the composite with the fused; the clear with the relatively unclear. Wölfflin classifies all the forms of art for this period with these five pairs of polar terms.

It is striking to see that in two so widely separated fields it is possible to make use of the same kind of structural analysis which is being used in linguistics.

Now let us look at a few intermediary levels, in the first place, technology. It is quite unfortunate that technology has remained, so far, a field in which few people have tried to introduce this new type of structuralization. If one should try to do so, he would probably arrive at: (1) something in which all the technological activities of man could be reduced to three types—to strike, to rub, or to cut; (2) that the tools which we are using offer three main characteristics—they may have an edge like a knife, a blunt surface like a hammer, or a discontinuous surface like a saw or a file; and, (3) that when you use the instruments, you can use them in three different directions: perpendicular, oblique, or circular. By combining these three sets of distinctive features, it is probably possible to classify all possible technological operations which man has been using since the beginning.

Why has this not been done? Because, in trying to reduce technological form to minimum units, it seems that people were never concerned with function. There is a kind of correlation between the term "use" in the field of technology and that of "meaning" in the field of language.

To take the example of basketry, it is obvious that basketry offers a wide range of possibilities which cannot be realized all at the same time. A choice must be made. For instance, I may try to have flexibility; but, if I have flexibility, I am going to lose some other property of basketry. But if one should try to use these distinctive feature principles in analyzing technology, he could very successfully classify all the known types everywhere in the world.

A great deal of attention should be paid to the term "use," which I probably employed mystically a minute ago. Let us take as an example another technological field—costume or dress. Dress can be dichotomized into two different categories: the tailored costume, which fits close to the body and is extremely disrespectful, if I may say so, to the integrity of the material, and the dress, which respects the material but does not respect the human body, because the human body has to take whatever shape the dress wants. This opposition can be considered as that of a mask, the tailored costume being a mask for the entire body against something which is entirely different; so these functional principles we should use are not really utilitarian. We have to consider the complete function, the intellectual and spiritual level as well as the practical one, of the technological form under consideration. Through that means we are able to reach the deeper technological level, since these two extreme forms of establishing the relationship between the human body and its costume, correspond to two opposite techniques, one of which is to cut along the thread —this would be the tailored suit—and the other one, to cut across the thread, which would be the dress; so, really, we are confronted with a whole. It goes from the lower technological level to the higher form of social life and even religion, because there is myth and religion in our costume.

As concerns social organization, I shall limit myself to one example. In many societies there is a close relationship between the maternal uncle and the nephew, the so-called "avuncular relationship." Now I will venture to say that, in practically all cases which are

known, the relationship is either with the nephew alone, excluding the niece, or with the nephew and the niece, but there never existed a differential relationship between the maternal uncle and the niece, on the one hand, and the nephew, on the other hand. With such a differential position of the nephew and the niece, the picture becomes much more complicated. Instead of being limited to the elementary situation which I was referring to, where we have nephew and niece pulled together in relation to the maternal uncle, we have to introduce a symmetrical configuration, where the father's sister or the father's sister's husband plays a role in relation to the niece which is symmetrical and inverse to the one which exists between the maternal uncle and the nephew.

Now I turn to the last problem, that of religion and mythology. Professor Lounsbury mentioned the gender classification for female dichotomy among the Oneida and said that he could not find any correlative attitude. It is quite possible that the correlative attitude does not exist in the field of behavior; nevertheless, I am quite struck that this very special dichotomy should appear in a publication which has pushed the matriarchal system to such an extreme, and I would venture to say that there is some correlation between the dual position of women in Iroquois society and this distinction between two aspects of women, let us say, women defined as social beings and women defined as biological beings.

I would like to report another example. When we study the mythology of the Pueblo Indians, it is very remarkable that among the Hopi and Zuni the emergence is treated as a true emergence, that is, men emerging because they cannot stand the situation they have in the lower world, or because the Sun God sends divine messengers down to get the men. We have either one or the other; we never have these two features represented simultaneously.

If we look at Keresan mythology—and I am using Acoma material—the situation is entirely different. We are given at the start a polar position between the lower world, and it is through a joint action of woman and the sun that men are created and led to emerge. Here, instead of having a one-way system, we have a two-way system.

Immediately, this polar opposition multiplies itself, and we find it pervading the entire field of Keresan mythology. For instance, instead of having one type, one role, for the ceremonial clown in the emergence, we have two. We have a dual system, which entirely replaces the unilinear system of the Zuni and Hopi.

I venture to connect this with the fact that the Keresan people use a self-reciprocal kinship terminology, while the Hopi and Zuni do not; that is, the Keresan people are always thinking simultaneously of two kinship positions, grandfather and grandchild, for instance, through an intermediary, a middle term. If this correlation exists between the dual mythological system of the Keresans and the self-reciprocal terminology in kinship, I ask the linguists: Is there a corresponding difference between the Keresan language, on the one hand, and the Zuni and Hopi, on the other hand, which would correspond to the differences in the field of kinship and the field of mythology?

I would like to end with one more example. When we try to make a comprehensive classification of the emergence myths in North American mythology, we soon find that they boil down to a very few basic propositions. The word "emergence" can be conceived as emergence from down to up, as among the Western Pueblo; it can be a diving, as in the Woodlands; or it can be a travel-

ing along, as in the Northwest. We have three situations.

On the other hand, when we look at what is being created, we find two different situations. We have either opposition between water, earth, and sky, which is a ternary system, because we have upper sky, above sky, and the underworld. So we have, first, to classify according to whether we are confronted with a ternary or a quinary system. With water, shell is connected; with sky, star; with earth, rock or pebbles. Using this principle of organization first, and next looking at what the myth is, it is possible to account for all the variant forms which are found in North American mythology. It seems to me that we are in a position to use the same type of structural analysis which is being followed by linguists, even in as complicated a thing as mythology.

MEAD: I have been worrying about the terms "culture" and "the rest of culture." In the first part of this discussion we considered language as one form of culture. In that case, borrowing does not bear the same relationship to linguistics that diffusion does to culture. Borrowing in language would be a subtype of diffusion, and the word "diffusion" would cover it all. Furthermore, I do not believe that we are ever going to get anywhere if we try to look for analogues of morphemes and phonemes in the rest of culture.

On the other hand, what Professor Lévi-Strauss was trying to do—to take particular aspects of culture and then look for the analogue—is what we shall have to do, and not for this linguistic-nonlinguistic category but for a series of categories of culture, of which language is one. I think we suffer from the fact that people on a simple level of culture separated language out, and, because they were able to think about it and talk about it and say that they could learn the language of another people, we got the notion that language was separable from the rest of culture.

In the Foreign Service Institute, linguists and anthropologists are finding a great deal of parallelism between the gesture-posture system and the linguistic system. They find they can handle them in comparable ways. Many people have believed that you could learn the language of another people without the posture-gesture system, and we have had so many culture-bound, prescientific concepts of language cropping up that introduce this distinction between language and culture that it is impossible for us to think systematically about this material.

To go back to the two-level analogue, physical anthropology and cultural anthropology, it seems to me one important thing is the extended way in which the body is involved and how many sensory modalities we are dealing with. For instance, in linguistics we are continually involving both hearing and articulation, and the two are kept in balance. Whereas if you take pace as modality of the body in a culture that is sophisticated about cooking, the eater eats what the cook cooks and knows that that is happening. So there is a relationship between the motor activities of the cook, who brings and beats the eggs in a particular way, and the gustatory experience of the eater, who eats the beaten eggs. In a culinarily unsophisticated culture, on the other hand, it is possible for the cook to perform a set of operations which are differently interpreted by the eater, so that what the cook does is beat eggs and what the eater eats is an expensive cake.

HAAS: I want to make just three statements largely in respect to what Dr. Mead was saying a moment ago about language and culture.

Language is a part of culture. At the same time it is that which makes culture possible.

Second, language on a formal level

is a system which can be isolated and studied separately.

Third, you cannot learn a language without learning the total culture. Not just gestures and postures, but everything else. Professor Jakobson's remark about the atlas type of work is extremely important, and it is the very sort of thing that we need to have done before we can generalize. But we need much more material and many more linguists before we will get material.

BUHLER'S SPRACH-ORGANON AS A SCHEME OF REFERENCE

HELD (submitted in writing after session): The question has come to my mind whether Karl Bühler's conception of what he called the "Sprach-Organon" might be of use as a scheme of reference for further clarification. As indicated in Figure A, Buhler distinguishes between three part-systems in the linguistic situation. When a linguist uses the term "structure," he usually refers to the *Darstellung*, to the content of the speech, rather than to the speaker and the listener.

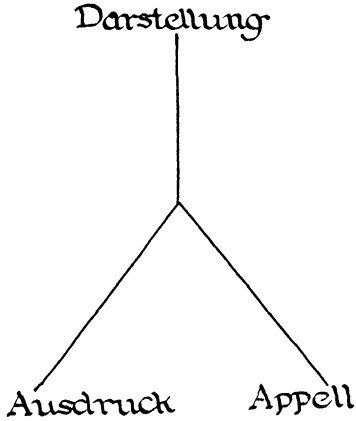


Fig A

In reality, the three part-systems of the speech situation are seldom completely integrated. Since a linguistic structure is a system of symbols, the principle of limited possibilities is operative. In other words, a speaker cannot express in his language everything he wishes to. In the Dutch language for instance, the word "sun" is grammatically female. Poets find it difficult to refer to the sun by using the female pronoun if they want to describe the strong or masculine power of the sun.

In the same way, since the Russian language systematically distinguishes between a perfective and an imperfective aspect, this grammatical category appeals to the Russian in a positive and definitive way.

However, since expression, representation, and appeal-systems form one functional whole, it is impossible to deduce one from the other. If a language makes gender oppositions, it cannot be said that this opposition is brought about by the wish of the speaker to make such distinctions, although the linguistic opposition has a conditioning influence on his thinking. The relation between language and culture is not a simple casual one.

If we apply Buhler's scheme to the social situation, a few differences become apparent. The first difference between the social and the linguistic structure lies in what I will call the "abstract" character of the last. A language as an isolated entity can be, and often has been, studied regardless of the people who used it. Apparently, this is difficult in a social structure where actual group behavior must be taken into consideration, as in the use of kinship terms.

In a social structure the relation of appeal is much closer than in a linguistic structure. A social structure is to a much higher degree a function of the group, with its inherent demographic and mathematical limitations, than a language.

Second, a social structure (in the sense of Professor Lévi-Strauss) is a system not only of symbols (as is a lin-

guistic structure) but also of values. If I say "I has gone," I make a grammatical error; and, if I make too many errors, I run the risk of being no longer understood. If, however, in a long-cycle society I refuse to marry my mother's brother's daughter, I run the risk of social correction or punishment.

Consequently, social structure, as described by Professor Lévi-Strauss, integrates both value and group (Fig. B).

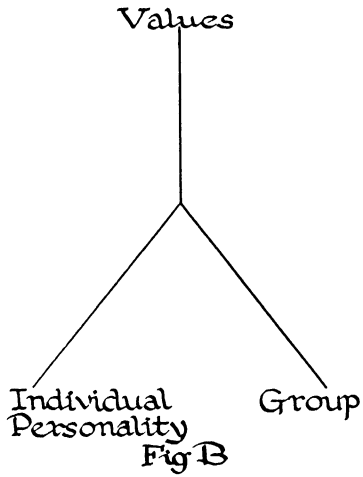


Fig B

Owing to this normative function, it is perhaps misleading to call kinship structures generalizations of group behavior. Values are not defined through the means of statistical calculation of behavior patterns. The way in which Professor Lévi-Strauss stresses such phenomena as hypergamy gives the impression that it is really the value and not the symbol that he has in mind.

Close analogies with phonemic patterns are perhaps misleading, since this leads to a discussion of the real, the mentalistic, or the fictitious nature of phonemes. Moreover, phonemes, being distinctive signals, have no ethical connotation.

In my opinion no reasonable doubt can exist as to the interrelatedness of religious and social structures. I do not mean that a religious structure is nothing but a social structure, so that one might speak of a cross-cousin religion. The fact that religious classifications are functionally related to social structure stands above doubt.

One may even go further, as pointed out by Lévi-Strauss, and find societies where a normative conception of a social structure exists which is not represented directly in actual group organization. The Geelvink-Bay Papuans, for instance, described their system as based on mother's brother's daughter marriage. Actually, this could not be deduced from genealogies. If asked to demonstrate that for a given marriage the relation between the partners really was as indicated, the usual answer was: If they were not exclusive cross-cousins, they could not have married, and therefore they must be mother's brother's daughter and father's sister's son.

In a social structure seen as a system of symbols and values, the influence of social contracts is more important than intra-familial behavior. Nuclear families are more often considered as given, natural entities, whereas clans and communities are seen as ideal, that is, potentially variable units. Many myths explain the origin of clans and communities, giving them a sacred, supernatural basis. To my knowledge the nuclear family, on the other hand, is seldom derived from a supernatural origin. Clans, classes, and communities are usually basic in social, normative structure.

To sum up: The term "linguistic structure" usually refers to one part-system in the speech situation.

The term "social structure," as used by Dr. Lévi-Strauss, however, comprehends both the group and the values, though, in its schematic form, not all the values and the totality of the group. Since the individual is given in the interaction between group and values, this concept of social structure is basically total in a sociological sense.

CHAPTER XVII

Pattern in Biology, Linguistics, and Culture

Chairmen: Clyde Kluckhohn and Claude Lévi-Strauss.

Papers discussed: Kluckhohn, "Universal Categories of Culture";* Martinet, "Structural Linguistics."*

Speakers: Kluckhohn, Washburn,† Willey, Chapple,† Lévi-Strauss, Beals, Thompson, Lowie, Linton,† Mead, Hoijer,† Jakobson,† Murdock, Voegelin, Northrop, Brew, Clark, Movius.

INTRODUCTION

KLUCKHOHN: That concepts of patterning are intuitively felt by most of us to be necessary is convincingly shown by the fact that every kind of specialist at these meetings has used the term once or more. At the same time, we all tend to use the term rather loosely. We feel this to be a rather amorphous concept, and I suppose the main purpose of this session is to see if we can pin it down a bit. At the same time, we hope that the discussion will not be just of the word "pattern" and whether this or that is a pattern.

We would all probably agree that one sense in which we use "pattern" is to refer to certain constancies or near-constancies in relationship, irrespective of dimension and/or content.

As Kurt Levine and Cassirer have noted, scientific progress is often correlated with the recognition of new entities or forms that are for the first time regarded as amenable to scientific treatment. I think we are really just beginning to treat forms as opposed to content in other than an impressionistic way.

The first explicit use of the word "pattern" that I encountered in anthropological literature was in a paper by Clark Wissler in 1916, I think, on ritualistic patterns. . . . There is an interesting British book, called *Aspects of Form,* containing a series of essays by biochemists, astronomers, etc. I have also just been reading a book by J. Z. Young, a British biologist, *Doubt and Certainty in Science.* Young talks a great deal about these things. He has one passage . . . in which he says: "However, the more we come to know of the flux of chemical changes in the body, the more one great weakness of the machine analogy stands out. The concept of a dynamic organization, such as that of a whirlpool, demands a consideration of time, of before and after, and of the gradual development and change of pattern. But the machine models of physiology allow no place for this element."

This is the same theme that is struck in a 1943 paper, published in the *Scientific Monthly,* "Structure, Function, and Pattern in Biology and Anthropology," by Dr. Kroeber. Kroeber says: "One significance of structure is that it yields classification by pattern, which in

turn has genetic or evolutionary significance; in other words, historical significance."

One of the reasons why pattern can demand our attention is because it brings together two kinds of approaches, varyingly referred to as historical and scientific or historical and analytic. It takes us beyond the naïve kind of functional treatment, because most patterns, both in biology and in language and culture, are in some sense the precipitate of historical accidents. At the time they were created they probably bore some sort of relation to the functioning of individual organisms or to the survival of groups; but, once established, such patterns often have extraordinary persistence, even if they go contrary to adjustive and adaptive effects. And so, in isolating patterns, it is often necessary to look at them in historical depth. . . .

Forms often have a significance which cannot be equated in any simple sense with function, and, until we are able to isolate patterns which are significant and persistent forms, complexes of ordered arrangements, we often do not know what we should measure or observe. Often patterns are inferential constructs.

An example of a patterned element in Navaho culture is the "avoidance of closure." In various objects—blankets, pottery, baskets, silverwork—there is always a particular element left incompleted. This is a directly observable pattern. But also, when a novice learns a Navaho ceremonial, the singer, even if he is teaching his son or his maternal nephew, always holds something back. This is regarded by the apprentice as protective for all concerned.

A Navahos discussion of the ideal human relationship between husband and wife or between friend and friend comes down to, "But in this really good relationship, something always has to be held back." Here is a recurrent thematic principle, not one the Navaho will tell you about, but just as much a part of Navaho culture as the more overt and directly observable pattern.

There is also the question Dr. Chapple is going to discuss—what kind of mathematics to use for phenomena of arrangement—where it is not so much a question of how much, but of a particular kind of ordering.

I suspect that one reason why we have made so little progress in dealing with these things, whose existence we would all concede, is because we have been so dependent upon a probability kind of mathematics, which is, in principle, not applicable to precisely patterned behavior.

I think it is not accidental that in the paper by Jakobson and Lotz on the French phonemic pattern, the picture which represents the various binary oppositions which they assert define the French phonemic pattern has a striking resemblance to pictures of giant protein molecules.

PATTERN IN BIOLOGY

WASHBURN: As one thinks of pattern in biology—and I am going to restrict this to the bony face of primates—the question is largely one of how you think, what question is in your mind when you look at bone.

The tendency, inherited from the past, is to think in two ways: one in dimensions, the other in the individual bones. . . . The pattern way of thinking is in terms of the interrelations of parts. There is a lot of this in anatomy, but the difficulty is to find methods which make these patterns clear to anyone who wants to study them.

The progress of the idea of pattern in the analysis of the skull is largely dependent on the development of techniques which show patterns. Traditional anthropometric measurements do not show pattern. . . . I think I could show you that there is a probability

that the difference in cephalic index of the Eskimo of Alaska and Greenland is due to differential growth in the squamous suture. But to do this, one has to do a pattern analysis of the whole top of the head; you can do nothing with the cephalic index as such.

A student of mine, Neil Tappen, prepared some analyses of the faces of man and chimpanzee. The analysis is based on the technique worked out by Benninghoff, elaborated by Seiple, and improved by Tappen. Solid lines indicate that the structure of the bone is telling you that this is an organized system along that line, in terms of stress and strain—that this is the pattern. Dots indicate lack of organization according to any essential system. If you compare the brow ridge of man and the brow ridge of the chimpanzee . . . the brow ridge of man is nothing but little dots and the brow ridge of the chimpanzee is all in the organized system.

There is not any way, by looking at brow ridges, that this kind of information can be brought out. Likewise, if you look at the nose of the chimpanzee, you will find that the stress is from the teeth. Long, sweeping lines go uninterruptedly through the nasal bones of the chimpanzee, whereas, in man, these come to the side of the bones, and the bones are organized in large part in a different system.

This cannot be told by looking at the bone by X-ray or any technique other than this one. Part of the difference in what one sees in this skull is due to this different mechanical pattern, which can very easily be brought out by this technique.

This, of course, is only the beginning, because these systems are the result of a variety of other things. For example, part of the pattern in the malar bone is related to the pattern of the angle of the jaws as worked out by Seiple; and the way this works out is due to the detailed structure of the masseter muscle, which we are now working on at Chicago; and the function of the muscle, in turn, is related to the way the jaws are used; so that one has a nontraditional association of a bit of brow ridge, a piece of malar bone, a muscle, a function, a piece of the jaw, with all of these forming a pattern.

I think I can see the same kind of pattern in certain lemurs, slightly developed in the apes, very well developed in one group of Old World monkeys and several groups of New World monkeys, and sometimes well developed in man, but usually not so. In other words, the same constellation, the same organization, cuts across all our traditional taxonomic categories. We have not, of course, had a chance to do more than compare these forms very extensively.

If one takes patterns and interrelates them, the resulting form is a balance of one whole pattern versus another, not a balance of the individual pieces of either pattern, as far as morphology is concerned. This is fundamental; to tell the detail about the nose, one has to do a complete analysis of the face. . . . Nose is not an analyzable thing in and of itself. To attempt to do genetics of nose, or measure nose as such, is a waste of time; that is what the pattern analysis shows.

One finds that these anatomical patterns in different animals have a different history, and I do not think this is predictable, because there are too many variables. If one wants to know about real history, regardless of how much experimental analytical work one does, one always has to have real fossils with real dates. No one—and I stress this—is going to avoid real history by elaborate analysis in the present. . . .

I will give one historical example. The ilium of *Australopithecus* is very human. It shows that perhaps these animals walked upright. But it does not

have one little bump on the side which we have. Is this important or not? In other words, what kind of interrelationships does this structure have?

We are working on that now and . . . if we are right, our work will show the following: This little bump is related to balance. The Benninghoff analysis I spoke of, done on the ilium of ape and man, is very different. The main difference relates to the little bump we have, that *Australopithecus* does not have. The muscles which are important in balance in walking are attached there. *Australopithecus* does not have them. So I suspect that, although these animals walked upright, they did not walk upright for prolonged periods; that they ran along upright and then stopped. In other words, this is really transitional. The only way one can get this kind of information out of the past is, first, to have the fossil, because nobody anticipated this combination of things, and, second, to think of it in terms of the whole pattern of locomotion and what goes into it, and not have a distribution of little independent items.

WILLEY: I was wondering if the nonpatterned areas are perhaps undergoing change, and, if that is so, if there could be any analogy, perhaps, to culture? Certain segments of the totality which do not seem to fall firmly into a set pattern would be areas for us to look to in the matter of change.

WASHBURN: What the dotted lines show, actually, is the result of a dynamic process. For example, when the canine tooth in the chimpanzee erupts, as Seiple has shown, all this pattern system breaks down, and it is all re-created. . . . It is something built up in a dynamic way in the life time of the individual.

The dotted parts are there in regions which are greatly stressed, but where the stresses are going in different directions, so there is no obvious pattern; and these we are trying to break down by refinements of the technique, so that we can get two patterns actually going through each other. Tappen has succeeded in doing this in one case.

There are also areas where there is so much material that the stress is unimportant, such as the top of the skull. In other areas bone is placed for entirely different reasons; for example, in the chin and the brow ridge, bone apparently grows under the influence of androgens at puberty, and this does not have anything to do with these other systems at all. This is entirely different; so there is a great variety of business in the dotted part.

KLUCKHOHN: I would like to make two comments. The first is that, in biology as well as in culture, we get convergences, and hence we cannot be sure that we have the same genetic patterns, if we do not have complete records. The other is that . . . what Dr. Washburn has said suggests that in cultural anthropology we might get farther in our comparative studies if we compared patterns rather than traits.

PATTERN AS A SYSTEM OF FUNCTIONAL RELATIONSHIPS

CHAPPLE: I have grave doubts as to the utility of the term "pattern" in anthropology. Ogden and Richards, in their book *The Meaning of Meaning*, had great sport in collecting different usages of such words as "meaning" and "beauty," and even more fun showing the confusion an author could get himself into by shifting from meaning to meaning, often in the same sentence, without being aware of what he was doing. I suspect that "pattern," from my observation in anthropology, is in the same category. Everyone uses it, but never with rigorous definition.

Most of our communication is primarily concerned with obtaining the proper emotional reactions from the other fellow, and pattern seems to be

one of the more powerful aids in achieving it. Most of the definitions of the word fall into two classes. There are the various aspects of what Malinowski called the "context of situation," the happenstance assemblage of words, actions, objects, environmental conditions, which derive symbolic meaning from the interactions with which they are associated. Such processes as equivalence, emblemization, and so on, which provide the mechanics of symbolization, can be described by mathematical logic, if anyone feels that it is worth while to make the effort.

The relative lack of importance of this is due to the fact that the order of these relationships in mathematical logic, that is, the logical categories that you use to describe the relationships of symbols in this process, are prescribed by the quantitative characteristics of the interactions from which they derive their meaning.

The second class of definitions is the one to which I should prefer to restrict the word "pattern"—the relationships of functional dependence which are verifiable through observation. These relationships in social anthropology involve interaction of individuals. They may be measured by the application of objective operations, based on units of time, and they exhibit the properties of systems in the mathematical sense.

Relationships of functional dependence apply only under situations in which we see that one happening is followed by another repetitively; in other words, we have to limit ourselves to the relationships between variables and make such statements as "When X increases in magnitude, then Y decreases." Where we possess means of measurement, we are able to write an equation, which tells us the precise nature of the relationship. From it, we can compute the value of X for any given value of Y.

Since the use of mathematical equations in this sense is conspicuous by its absence in anthropological textbooks, including my own, you may wonder what relevance these comments have to the problem. The fact is that the fundamental work in social anthropology utilizes quantitative concepts to explain the occurrence of change in social organization. Factors affecting the amount of interaction, such as occupation, residence, food supply, or a combination of such, have been shown to modify family and kinship systems. You are all familiar with the work of Lowie, Eggan, Spoehr, Radcliffe-Brown, Malinowski, and their associates, in roughing out relationships of functional dependence, using both comparative and chronological methods.

Their work has significant implications for developing quantitative mathematical descriptions of systems or relationship. Since the war, there has been interest in utilizing mathematical models, on the assumption that the process of reducing anthropology to an exact science can be speeded up. Anthropologists have been boning up on cybernetics and other mathematical tools, while a few physicists and mathematicians have made cautious forays into our field.

However stimulating and educational these queries may be, it cannot be too strongly emphasized that, except in rare instances, they are doomed to failure. The mathematics of anthropology and, hence, of pattern in this context must be built on established uniformities, that is, upon the empirical observation of relationships of functional dependence which are capable of measurement. The uniformities must come first, the mathematics later.

There is no guaranty that any existing mathematics can be used for the study of organizational problems. It may be necessary to develop a mathematics of our own. We have to determine whether the basic assumptions

underlying any branch of mathematics or, for that matter, any single equation, such as the normal probability law, apply to the data of observation, and not try to fit the data into the Procrustean bed of shaky mathematical analogies.

Certain leads, taken from our own work, which I will have to take refuge in, indicate that the process may not be so difficult as is commonly thought. Not only can we measure the interaction of two or more persons continuously as long as energy and financial resources permit, but from these measurements we have derived certain hypotheses or assumptions which permit us to sketch a mathematical framework for social organization.

We are able to show that each individual has a characteristic rate of interaction in quantitative terms and that a good working hypothesis about this interaction is that a person requires *X* amount of interaction per day, at *Y* ranges of intensity or adjustment, both of which are already capable of precise quantitative description. Moreover, this interaction manifests the properties of a system in equilibrium. It returns to its initial state after disturbance, and the magnitude of the deflection of the system is controlled by parameters, that is, constants, characteristic of the individual. Each individual is an interaction with others during the day and day after day. He is a point and a network of relations, large or small, and this network can be differentiated into systems and subsystems in terms of the frequency of interactions within them. Each system or subsystem therefore has quantitative properties. The interaction rates of its individual members are differentially distributed within it, and the total interaction may also be regarded as in a state of equilibrium.

What makes the mathematics feasible in the total set of hypotheses is the organic requirement to maintain a constant expenditure of interactional energy. Reductions or increases in available outlets through cultural change, birth, or death or the effects of close juxtaposition with individuals whose interaction rates do not provide the opportunity of adequate adjustment within the tolerance limits of the individual's own system produce disturbances both for the individual and for the persons with whom he interacts in the network. Some of these changes affect single individuals, while others, such as environmental or technological changes, may reshape whole populations of systems.

The assumptions here sketched allow us to apply relatively simple quantitative concepts to the pattern of relations we call a group of kin or a working team in a factory. We can compute the redistribution of interactional energy needed for any change, as well as determine the magnitude of the disturbance, by estimating the degree to which any single relationship or portion of a network provides a significant amount of the outlet needed for a given individual. We can, in fact, use a modified form of the kind of network analysis used in electrical work, since we can assign quantitative values to each link in the network, and we can determine the effects of any change in the quantitative values assigned to any link on its neighbors in the network pattern. Like an electrical network, too, we can isolate portions of the networks, so that we can add 2's and 3's and 4's, that is, people, as constituent units to network complication.

We therefore do not need to worry about the numbers of people included in any single institution, since, for purposes of analysis, like electrical circuits, it can be broken down into small additive units.

In short, pattern, defined as a system of functional relationships, seems to be

susceptible to quantitative treatment in the area of social organization, and we can look forward to the day when experimental changes in family structure can be carried out under scientific control.

KLUCKHOHN: I have some question about the phrase "functional dependence" in Dr. Chapple's proposition, because, to use an example from Sapir, it is an established uniformity that in English we say, "five houses," when, from a functional point of view, "five house" is an established uniformity; but I do not see that there is any functional dependence in it.

CHAPPLE: That is not it. The function I am talking of is in the sense of a mathematical function, not a statement of fact.

KLUCKHOHN: O.K. . . . The thing that excites me the most in phenomena of pattern is the possibility of cross-institutional types of patterning. If a culture is, as I think most of us believe, a system, we must have some concepts in terms of which we can translate between the various institutions.

I have a note from Dr. Washburn in which he says: "We should stress the point of view, not definition, regardless of whether what Dr. Chapple said about vagueness and many usages is true. I don't give a damn whether we call it 'pattern' or whether we call it *X!* Maybe we should call it *X*, or the point of view of trying to deal with the orderly, persistent relationships between parts, as opposed to dealing with items seriatim. . . ."

THE MATHEMATICAL APPROACH

LÉVI-STRAUSS: I confess I was quite disturbed by the first part of Dr. Chapple's speech and comforted by the second, because it seems to me that, after saying that mathematics can be of little use to anthropology, he made a very good case for the use of mathematics.

As to the question of the word "pattern," I do not feel that it is very important if we use the same words with quite different meanings. If, in the eighteenth century, Ogden and Richards could have elaborated upon such words as "gravitation" or "acceleration" or "mass," they would have found these words were used with quite different meanings and probably would have said that they were of no use for the development of science. We are certainly in a primitive stage of our science, and we are using words in many different and wrong ways. It does not matter at all. They are operational tools, and we may be confident that later on a correct meaning will be found for them.

Second, I am afraid that in what Dr. Chapple said there is a confusion between the quantitative approach and the mathematical approach, and this seems important.

We all agree that the quantitative mathematics of the nineteenth century can be of little use for anthropology, or can be used only in such limited cases as outlined by Dr. Chapple. However, there is a qualitative mathematical approach, which probably cannot solve all our problems, but where we are in a position to find a great many clues which we have not yet been using.

There is in topology a famous theorem that, in order to make contiguous areas distinct from each other by means of color, whatever the complication of pattern is, four colors are needed and never more than four colors. This is a useful principle in making maps. It seems to me that archeology cannot permit itself to know such a theorem because it shows that when we are studying painted portraits, for instance, there is what Professor Greenberg would probably call a drift, leading to a four-color portrait, but that above four colors there is no such drift,

and that a quite different principle of explanation is called for, in order to explain why people are using more than four colors or less than four colors. This is one point.

My second point, which will also be taken from topology, is about string figures. Old-school anthropologists were much interested in string figures, and they were not fools. They felt that string figures were important, though they did not know exactly why; and, when they found the same kind of string figures among two very remote people, they concluded that there was some historical relationship between these two peoples. Of course, string figures are important; they appear to be meaningless, but we know now that when something is meaningless, it only means that the meaning is on a different level.

It is quite obvious that string figures are nothing more than topological transformation. We have a continuous contour which is being deformed and transformed in many different ways. I think nothing would be more urgent in the field of cultural anthropology than to make a topological theory of string figures, because this would show that, according to the population which is being considered, certain sets of topological transformations were chosen while other sets were ignored, and it would be important to find out whether in kinship, mythology, or linguistics we are confronted with the same basic choices. These two examples, out of many, help to show how mathematics can be used to make advances in anthropology.

CHAPPLE: I did not want to get into a discussion of topology, which has intriguing aspects for many people in the field of social sciences. However, I might say . . . that in the area with which I am primarily concerned, the field of prediction of how people will behave, topology and similar qualitative methods of description are pretty much blind alleys. They may be useful for describing patterns on pots or string figures. But in the area I am describing we attempt to avoid what we already know about the way in which social organization changes, by trying to be more precise or as close to quantitative procedures as we can.

KLUCKHOHN: I would suggest that just as the fact that certain forms in string figures are meaningful, so is the fact that the word "pattern" at the moment, in this particular subculture, evokes a favorable response.

BEAL: I have some thoughts on the general problem of how you arrive at some consistency of pattern and the degree to which this can be translated into mathematical terms. The ordinary statistical techniques do not apply in this sort of situation. It is apropos that one of my statistical-minded colleagues pointed out that by using statistical techniques—sometimes quite refined techniques—it was possible to show a pattern in chaos, if you took a section of chaos.

The significance of this, I think, is that . . . you can use statistics with relation to patterning only after you have gone through some other kind of nonstatistical process of developing categories of some significance, and this significance is not arrived at by a mathematical procedure. . . . This suggests that we have to develop kinds of analytical and ordering techniques for understanding pattern that are pretty well divorced from our ordinary mathematical approaches. Then maybe we can apply mathematics to it. This may be the last step, not the first step.

KLUCKHOHN: Warren Weaver, in a paper in the *American Scientist* two or three years ago, said that, in the history of science and the application of mathematics in science, we started by devising a mathematics that would deal with systems of organized simplicity, and he

uses the simile of three billiard balls on a table. He said that from that we jumped to systems of disorganized complexity—three million billiard balls on a table—but that what we need is mathematics for dealing with systems of organized complexity.

THE AESTHETIC WITH RELATION TO PATTERNS

THOMPSON: More than forty years ago I happened, as a student of English literature, to be in a class of a professor who had been reading widely in the English anthropologists; and in this course in literary criticism he called our attention to the fact that, after all, the literary forms that we were working with in modern European literature were older than writing and that they were found among peoples who did not write at all. He then had us read Tylor's *Primitive Culture* and the works of the British anthropologists.

Now, I realize . . . that some of the ideas we secured were antiquated, but the point was that he got us to reading in so-called "primitive" literature. Some of us, like myself, never got very far. We put only one foot into the field of anthropology. But one of my classmates went the whole way—I speak of Earnest Hooton, who was deflected from being a professor of Latin and Greek by that class.

However, for the first time, we realized that there was a continuum, for example, in the making-up of stories; that there was a great deal of resemblance between the stories that we would pick up in the simplest cultures and the stories that we would find in European literature; and that there was no point in which there was a break, and I think the same thing was true in related forms like art and music. We came to realize that we could not work on such a thing as the Egyptian Book of the Dead without knowing something about primitive ceremonials and such things. Without laboring the point, I hope you see that the student of literature, who still makes his living as a professor of literature, is interested in patterns, because he realizes that the patterns found in modern literature and in modern art today have not only resemblances but frequently identities in the patterns of primitive culture; that primitive man had something like what we call stories, novels, even histories, and works of science; and that there are many of these continuing patterns that must be of interest to any student of literature.

LOWIE: I should like to refer to one statement Dr. Meyer Schapiro makes, namely, that Sioux art, by which he means, undoubtedly, Dakota art, follows a definite pattern. The question arises to what extent this pattern is related with such phenomena of Sioux life as, say, family relationships, ceremonial, or what not.

Applying the concept of pattern in the narrower sense to the study of primitive art, I should like to mention the quite extraordinary, although in some respects very simple, style used by the Plains Indians in rawhide decorations, specifically, the type of decoration we find on parfleches. In the earliest studies made by Boas, Kroeber, and others, the tendency was to be purely analytical, to recognize the obvious fact that curvilinear designs were lacking in this type of Plains decoration, and to emphasize the geometrical forms that were found—specifically, of course, triangles, rectangles, and squares, and the combinations of these. We all came to realize, however, that one had gone only a short distance when one recognized that these particular geometrical forms were employed by the Plains Indians, and that everything hinged on the arrangement of these forms into meaningful patterns. The reality of these patterns comes out very forcefully when you make a com-

parative study of different tribes. For example, the Dakota-Sioux are very fond of bisecting the decorative field and then having some pattern, say, a lozenge, in each of the two halves. You will never find that arrangement among the Crow Indians, in spite of the fact that the Crow and the Dakota share many features of culture.

I was impressed at a fairly early time with the fact that the Crow parfleches bear a striking resemblance to the Shoshone parfleches. After I had collected a number of Crow parfleches, Professor Kroeber looked at them, and his immediate reaction was, "Why, they look like Shoshone parfleches." I had occasion to show these same specimens to Professor Boas, and he also said, "Why, this looks like Shoshone."

In other words, you had three independent investigators, who were acquainted with the facts, all of them reacting to this qualitative difference between styles, or patterns, in a group of otherwise quite closely related tribes.

LINTON: This talk about Plains art reminds me of what I think is one of the best examples of the old axiom that the totality is different from the sum of its parts. On one occasion I had the opportunity to work over a considerable amount of Arapaho material: objects, decorative material, and so forth. The Arapaho are one of the Plains Indian tribes most addicted to symbolism. In the interpretations given, one found that the same arrangements of triangles, squares, lines, etc., would be given entirely different interpretations by different informants, not in all-over terms, but if you listed the triangle, it is given the following meanings: A turtle, a mountain, a tipi, a pine tree, etc. This is perfectly accurate, but it gave you no assistance in interpreting a given piece. You had a theme which was then developed in terms of the familiar smaller geometric elements, but it could only be read as a whole.

END-LINKAGE

MEAD: The sort of pattern that Professor Lévi-Strauss was discussing, in part, or the sort of pattern that we see in an art form or when we define a family form purely synchronically is perhaps the quickest way to invest the sort of measurements that Dr. Chapple is talking about with meaning, in the sense that it is usually used for cultural content.

If you use the method of end-linkage developed by Gregory Bateson in 1942 and take your parent-child relationship, which occurs in all cultures, then you have, in all cultures, succorance from the parents and dependence from the child. That is a standard that you have the world over. The child may start catching fish at six and thus feed its mother, but there is an interval when there is a standard situation. You then select a series of relevant analytical categories describing the parent relationship and the responsive relationship on the part of the child. If you are doing British culture, the parent, in the British middle- and upper-class culture particularly, is an exhibitionist and the child a spectator. The parent is dominant, and the child is submissive. When you compare this with the American picture, you find the major distinction is that the child is the exhibitionist and the parent becomes the spectator, often unwilling but still the spectator.

For most of western Europe, you have to make alterations of this sort, whereas when you move toward Asia, you make other systematic alterations. For instance, in Bali, dominance and submission are not useful categories, but elevation and respect are.

By linking this series, it is possible to analyze employee and employer, reviewer and book reviewed, speaker and audience—a very large number of pairs which will show systematic relationships—which gives you the sort of thing that Dr. Chapple wants, which is pre-

dictability. Also, they will give you the positions within which it is worth while to measure quantitatively the sorts of initiation and response, duration and interruption, that Dr. Chapple is talking about, because, if you look at a parent-child relationship, it may be that the baby cries and the mother responds and feeds the baby and the baby stops crying. That is seen as a sequence, or it may be that the mother says, "That child needs feeding; I will go and feed it," the baby eats, and the mother goes away.

You have to break those sequences differently, in terms of the way they are conceived, and we need this sort of an intervening variable if we are going to work out which aspects of family form we want to measure, if we want to measure change over time, in the sense of Dr. Chapple's type of measurement.

PATTERN IN LINGUISTICS

HOIJER: I do not think that there is any real difficulty in defining what linguists mean by "pattern." If they use the term at all, they use a great many synonyms for it. I suppose the most useful is the one employed by Bloomfield, "meaningful arrangements of forms at whatever level." These may be encountered, whether on the level of sound or of morphology or of syntax. Certainly, this is no new idea in language. It has existed for a long time, whether or not the people who wrote about language were aware of the fact. Sometimes they were and sometimes not.

It is almost impossible to encounter or describe a language without using this principle of meaningful arrangment. It impresses itself upon the observer and upon the learner, though the latter may well be unaware and unable to verbalize the particular patterns he is acquiring.

Illustrations of this are commonplace. I recall one which seems to me to illustrate vividly this concept of pattern or meaningful arrangements in language, in reference to borrowed terms taken over by an Apache language from Spanish. It has to do with the two Spanish words, *loco* and *rico*. They were taken over into the Apache languages, possibly because the notion of insanity in *loco* and the notion of richness in *rico* were lacking in Apache culture. But what was done to these words? First, of course, they were adapted to the phonemic system of the language which took them over, which was not too difficult because, except for the initial sound of the second word, these sounds were much the same in the two languages. *Lô·gò* and *gico* in Apache are very much like their models in Spanish.

But they did not stop there. In Apache, as in Navaho, there is no adjective category. Most of the Apache words which convey notions of the type conveyed by adjectives in the Indo-European languages are verbs and are conjugated just as other verbs are conjugated; so from *lô·gò*, which was then interpreted as the third person of the verb, you got a first person, a second person, a third or a fourth person in Apache, and the usual dual and plural persons of the same thing. From *lô·gò*, you got *lô·šgò*, *lóńgò*, and so on. Here we have a stimulus in the form of a particular item, shifting from one language to another and being absorbed, as it were, to a particular set of meaningful arrangements in the host language.

Similarly, the notion of pattern has been of value to the linguist in historical researches. Not the least value of Sapir's articles on the classification of American Indian languages is the method of proceeding by what he calls the "deep-lying patterns" of languages he was studying. I refer particularly to his long articles on the Hokan lan-

guages, where he demonstrated without any difficulty whatsoever that, if one penetrates beneath the superficial, current productive patterns of the languages being compared, one encounters all sorts of connections that could not be perceived otherwise.

In this particular kind of research one can get clues to the classifications of languages which enable the more detailed and intensive work of comparative analysis to go on.

Returning to my own researches in this particular respect, I think the concept of "pattern" as linguists employ it and, I think, as many cultural anthropologists employ it is also useful in the ambiguous field of ethno-linguistics or meta-linguistics or cultural linguistics or whatever you want to call it. It seems to me that Whorf's greatest contribution in the articles we have had reference to lies in the fact that he was able to abstract what he called "fashions of speaking," which stood over and above what the linguist refers to ordinarily as "patterns."

The dichotomy between lexical patterns and syntactic patterns is meaningless in this particular connection. The fashions of speaking that Whorf was talking about were semantic, morphological, and syntactic. They had reference not only to structural features but also to features of meaning, whether those features of meaning had reference to the meanings of specific items in the vocabulary and the classifications thereby implied, or whether those features of meaning had reference to the more abstracted meanings of the broader categories of the language as such.

Whorf was not seeking to understand the cultural implications of a single system, like a gender system, as much as he was seeking to understand the cultural implication of a totality of systems. He did not say, for example, that you could connect the presence of crier chiefs among the Hopi with the absence of tense in the Hopi language, but, rather, he said that these two features of structure, cultural and linguistic, contributed toward a system of thinking wherein these elements were at home, where they were consistent with respect to each other. We have here the possibility of deriving on this higher level of abstraction a key to the culture as a whole, the setting . . . in which the particular elements of culture, whatever aspect they belong to, fit together and make sense. In that sense we are speaking of a logical system of postulates—not using "logical" as a philosophical term but in the more commonplace sense.

JAKOBSON (statement revised after session): About the problem of the pattern concept in linguistics, I agree with what was said by Professor Hoijer. I should only like to add certain specifications.

If you take the development of linguistics during the last hundred years, you see that the basic problem was and is the problem of finding out the pattern of a given language. If you analyze the history of the development of linguistic methodology in finding out the pattern, you come to a very surprising —at least for me—conclusion, that it would be enough to take a book on the history of mathematics—let us say, a book which I can understand, Bell's *Development of Mathematics*—and then simply read certain passages, replacing the names of mathematicians by those of linguists, and the mathematical terms by more familiar linguistic ones; and what is most surprising is that even the years and decades of the development of certain ideas in either science coincide exactly. The problem of invariance, according to Bell and other mathematicians, became particularly important in the 1870's. Simultaneously, Baudouin de Courtenay came to the concept of the phoneme;

uncovering that to operate scientifically with variations, we must set the invariants; and this problem of invariance, developed in the linguistics of the 1870's and 1880's, became one of the pivotal features of modern linguistics. As we read in the history of mathematics, the full import of invariance was perceived only after the emergence of the principle of relativity, after 1916, with the book of Einstein on the general theory of relativity. The same may be said about linguistics, where precisely in 1916 there appeared posthumously the *Cours de linguistique générale* by Ferdinand de Saussure, defining the basic entities of linguistics as relative and oppositive, and presenting the problem of relativity as fundamental for linguistics. Then, from the 1920's on, in linguistics as well as in mathematics, we witnessed an intense development of the whole technique of working with many-leveled systems of invariants and covariants, and the objective methods for detecting such systems have been elaborated both on the phonemic and on the grammatical level.

If topology is defined as the study of those qualitative properties that are invariant under isomorphic transformations, this is exactly what we did in structural linguistics, especially in phonemics, without realizing that, *mutatis mutandis*, we were making topology, like Jourdain, who spoke in prose, yet in complete innocence that it was called prose. Not only the problem of relation between the phoneme and the contextual or stylistic variants, which has for a few decades preoccupied linguists, but also the further pivotal problem of phonemic theory and investigation, what is now called in American linguistics "componential analysis," finds a salient correspondence in modern mathematics and physics. When breaking up phonemes into elementary quanta termed "differential elements" by Saussure and "distinctive features" by Sapir and Bloomfield, the science of language discovers its ultimate units and their various superpositions and sequences. The discreteness of these units permits a strict quantization of phonemic data.

There is a direct help that linguistics is in line to receive at this moment from mathematics, especially from the so-called "information theory" or theory of communication. The fundamental dichotomous notions of linguistics, particularly singled out by F. de Saussure, A. Gardiner, and E. Sapir and called *langue* and *parole* in France, or "linguistic pattern" and "speech" in this country, now receive a much clearer, simpler, logically less ambiguous, and operationally more productive formulation, when matched with the corresponding concepts of the communication theory, namely, with "code" and "message." The attitude of the communication engineer toward this dichotomy differs essentially from the attitude of the inquirer into an unknown language, who essays, through the messages of native informants, to break its code and develops for this purpose a special cryptoanalytic technique. The attitude of the communication engineer coincides with the attitude of the member of a speech community participating in a speech exchange within this community and interpreting signals received from their sender. This receiver is a decoder, and the decoder is not a cryptoanalyst, although the confusion often occurs. The usual addressee of a message is but a decoder, whereas the cryptoanalyst is an unusual, marginal addressee, if not simply an eavesdropper. The decoder interprets the message in terms of a code common to him and to the sender—a set of preconceived, prefabricated representations, as D. M. MacKay puts it. A linguist assimilating an unknown language is compelled to begin as a cryptoanalyst

but aims to become a regular decoder, and the decoding methodology of descriptive linguistics is to be clearly separated from the cryptoanalytic approach of a reconnoitering field worker.

Modern mathematics suggests that "it is not things that matter but the relations between them." In biology also, as Schrödinger professes, the discrete differences of properties are "really the fundamental concept rather than property itself." Since the phonemic entities are purely relative, their correlations within the pattern are the core problem which has engrossed linguists of various countries and bents. The underlying principle of phonemic patterning, the dichotomous scale, is again the most convenient for the operations of the communication engineers, because the dichotomous scale is a series of "Yes" or "No" situations, of two-choice selections, and at the same time it is the real attitude of the receiver of linguistic messages who from each unit has always to pick up the "Yeses" and "Noes." In the *Manhattan Telephone Directory* we find such names as *Bitter, Fitter, Pitter, Titter;* when you first hear one of these names, let us say, *Pitter,* you must make a set of binary decisions: is it Pitter or Titter, Pitter or Bitter, Pitter or Fitter.

On the level of grammatical categories, we detect a similar series of dichotomies, and we face the same problem of finding the invariants in variations. Neither the phonemic nor the grammatical entities can be identified through a merely distributional analysis. We cannot decide, for instance, whether two forms are homonymic or identical if we do not know whether, behind the contextual variation, there is one invariant semantic value or not. It is true that some books on the meaning of meaning have taught that such a semantic invariant is a myth and that there is nothing but contextual meaning. Such statements are adequate but only for one special case of linguistic reality, a certain type of aphasia: the patient is able to recognize the contexts heard beforehand and stored in his memory, but he cannot create new contexts, whereas we others are able to create or at least to understand new contexts when we hear them, and this is proof that the constituents of this context are known to us and possess their invariant semantic value.

The application of the problem of invariants in linguistics is not confined to the mere description of intra-lingual patterns. Such descriptions naturally lead us to a further task, the search for linguistic universals. Kluckhohn's paper raises problems important for all branches of anthropology and for linguistics especially. If one speaks about languages, he herewith admits that there are universals, because he recognizes that another language is also a language and not gibberish, so that it must have certain structural properties of a language. One can, for instance, predict for any language spoken by mankind the existence of such grammatical units as morphemes or sentences, and on the phonemic level one knows well enough that there will be distinctive features—phonemes, sequences—and that two classes of phonemes—vowels and consonants—will be differentiated. Particularly important for the study of universals are laws of implication which state that the presence of an element B implies the presence (or, on the contrary, the absence) of an element A. We do not know, for instance, languages with constrictives but without stops or with affricates and corresponding stops but without constrictives; there are phonemic patterns, however, with stops but without constrictives, or with constrictives but without affricates. It is at times objected that we do not know all the languages of the world, so that exceptions may exist. Neither does the

zoölogist know whether in some virgin forest of Brazil a freak such as a five-legged cat may not one day turn up. Nevertheless, if he says that cats have four legs, this at least carries considerable statistical value; and if we find a language with five legs in Brazil, because in Brazil you can find everything, then we will say, well, there is a hundreth of 1 per cent of an exception; and, even were there a greater number of exceptions, these high-probability statements will still be of importance, since the number of languages registered by the linguists of the world is large enough. There are, moreover, nearly universal laws which govern the patterning of sequences. I know of no exception to two very elementary but, nevertheless, significant laws: (1) any language possesses syllables beginning with consonants; (2) any language possesses syllables ending with a vowel.

Such laws of implication may be checked through the study of language in the process of construction, that is, children's language. If, in the languages of the world, the presence of B implies the presence of A in the same pattern, then children as a rule—and again there may be Brazilian exceptions—cannot acquire B earlier than A. I know a case where a friend of mine, a very distinguished linguist, tried to give his daughter chocolate in order to change the usual order of phonemic acquisitions discussed in my monograph, and did not succeed. Had the infant succumbed to the temptation, then chocolate should be recognized to be an extrinsic factor of inverted order. In aphasia, the phonemic losses invert the order of children's acquisitions: B cannot disappear later than A.

If we can formulate a series of such phonemic (and let us add grammatical) laws of implication, we may subsequently arrive at the typology of phonemic (and grammatical) patterns, a typology fraught with consequences particularly interesting for historical linguistics.

There are no universal laws of linguistic evolution. The development may proceed in different ways, with a single limitation: it cannot lead to a state contradictory to general laws. The general laws, often sought in evolution, govern, as a matter of fact, any synchronic state. Neither is this tenet confined to linguistics. It corresponds impressively to the view of quantum mechanics, as formulated by L. Tisza, its eminent representative in this country: "Quantum mechanics is morphically deterministic, whereas the temporal processes, the transition between stationary states, are governed by statistical laws." In other words, structural linguistics as well as "quantum mechanics gains in morphic determinism what it loses in temporal determinism."

Here we see an essential prerequisite for further studies in the evolution of linguistic patterns. It must help us to overcome the methodological heritage of the neo-grammarian doctrine, the tradition of the late nineteenth century in historical linguistics, even though we accept in this tradition a high number of technical devices. But it was a bias which essayed a history of scattered facts, piecemeal dealings with separate phenomena, without reference to the pattern to which these components belonged, and therefore the genuine evolution of the phonemic or grammatical structure remained overlooked.

In analyzing a change of A into B, we must consistently render account of what the whole pattern was before the change and what it has become after the change; and here, again, we must see what was the sense of the change, its role for the whole pattern. Then we come to very new results, and many events which were considered completely separate appear to be simply fractional and predictable details of one comprehensive mutation which can be

condensed into much simpler formulas.

The gulf between descriptive and historical linguistics so vehemently emphasized as inevitable by Saussure was a temporary gap due primarily to the circumstance that a new methodology was used by him for synchronic linguistics, while in diachronic linguistics the student remained stuck in the neogrammarian rut. This cleavage was, moreover, considered necessary, because there hovered a fallacious identification which endangered, I think, not only linguistics but anthropological research in general, the identification of two dichotomies: (1) synchronic-diachronic and (2) static-dynamic.

A synchronic system is never static. Let me resort to a simple comparison. If you are in a movie and I ask you, "What do you see at this moment?" you do not see static matter. You see gangsters at work and horses running and various other motions. Only in front of the box office, without having paid admission, do you see merely statics pasted on the billboards. Neither is synchrony confined to statics nor statics to synchrony. We may assume a static approach to history and ask in particular what has remained immovable from Latin or even Indo-European until contemporary Italian. On the other hand, there is room for a dynamic approach in synchronic linguistics. A linguistic change is not like a change to Daylight Saving Time, so that on May 1 all members of the speech community would no longer use element A but only element B. There are time-spans when both elements A and B coexist with different stylistic connotation, and speakers may be aware that element A is an archaism and B an innovation, just as in an audience with a high dignitary you will not wear the tie you use on a date. Thus the start and finish of a change originally belong to the same synchronic state. The time factor itself, upon entering into such a symbolic system as language, assumes a symbolic value. The artificial barrier between synchronic and historical linguistics vanishes, since one can analyze changes in terms of a synchronic pattern just as one does with its static constituents.

MURDOCK: Professor Jakobson's statement has certain rather important implications for students of social structure. His emphasis on the change in emphasis in linguistics from a synchronic to a diachronic approach is something we can profit by in the study of social organization. The future studies of social organization that will be most valuable and valid are likely to be those that have the synchronic aspect and see patterns moving through time.

ALTERNATIVES IN PATTERNING

VOEGELIN: I should like to speak briefly on alternatives in patterning rather than patterns. One way of looking at this whole field of patterning is, as Dr. Lowie was saying, you see a pattern, and someone else sees the same pattern; this is by inspection. You look at it, and it is there. If you are doing patterning of the sort that Dr. Hoijer presented so well, of Whorf's material, one of the first difficulties is to follow what Whorf was trying to do. I think Dr. Hoijer has shown that more clearly than Whorf himself—what he was doing.

My point is that there is nothing the matter with patterns being inspectable. That is fine. . . . But suppose I see the pattern one way and Whorf sees it another? These are the alternative difficulties. I have been thinking along the lines of alternative possibilities so much that I never speak of number of phonemes any more; I speak of phonemicizing. Some people ask, "How many English phonemes are there?" One of the great difficulties I have in talking to graduate students is to try to convince

them that the object is not to find the exact number of phonemes of a given language, as though God had doled out just so many phonemes for each language. The object is to work in various ways and phonemicize; depending on how one phonemicizes, one may count more or less phonemes for a given language.

I refer, finally, to Professor Martinet's intriguing paper, in which it seems there are not Europe and America but various Europes and only one America represented in the paper. There are also various Americas in this technical area, I can assure you.

What can we do? Do we really stand apart? I think, perhaps, that we could make some transformation statements, that some differences are isomorphic. Perhaps ultimately the object of the whole thing, of any of these noninspectable patternings that we work on, will be defined to make a series, more than one pattern; that this will be called for and that you have to have transformation statements to connect them, and then, of course, you will have the problem of what is nontrivial, because you could go on trivially after that.

CULTURE IN ITS OWN TERMS

NORTHROP: It seems to me that in cultural anthropology there are two things. One is to get the conception of what you see, as conceived of by the people who made it, and then you have another thing that is happening today and that happened yesterday, that is, conceiving of every culture in the world in terms of concepts that are peculiar to mathematically trained modern minds in the modern West.

This is what is happening when we go to topology or when we take symbolic logic and start applying it across cultures, from one culture to another. I think this needs to be done, but what happens is that the cultures being studied are completely distorted and conceptualized in terms of setting concepts completely foreign to the people in the cultures themselves and peculiar to people in our own culture.

One of the most interesting books that could come out of this conference would be an account of the way people of different nationalities treat the same subject matter. I think there are typical types of nationality that immediately come up when a given topic is discussed. The French here always approach the facts with a certain type of conceptual system, the British in another way, the Americans in a lot of ways. The problem is how to get these separated and see in what sense it is proper to take concepts peculiar to a very sophisticated, mathematical, modern West and apply them to Navaho and to all the cultures in the world.

In a sense this is sound anthropology, but it seems to me to be distinguished from the prior task of getting a culture as it is understood by the people in the culture themselves. Here I would like to refer to a statement of Professor Kluckhohn's after he mentioned the fear of closure that he finds in the Navaho. He said: "We need to deal rigorously with these matters, to find a method."

The question I would like to raise here is whether or not, when you deal rigorously with other cultures—in the sense of mathematical rigor that Professor Chapple and Professor Lévi-Strauss are trying to get—you are not dealing falsely with them. Exactly the same thing occurred in the West, when you got people trained in classical art defending impressionism because they were weak on perspective or they could not draw accurately. You completely miss the point of impressionistic painting or Chinese landscape painting if you judge it from those Western geometrical kinds of mathematical notions.

I have the highest respect for Professor Chapple's work, because I have

paid a lot of attention to Underhill Moore's work in sociological jurisprudence, which is roughly the same technique; but does it not have the error that Dr. Straus pointed out yesterday of treating human nervous systems as if they have only a motor side, just the muscular motor, the overt, behavioristic measurable side? There are the association areas in the cortex. In the association areas of the cortex, you not only have the morphology of the network of the association area, about which we do not know as much as we should like to know, but we have some pretty good theories. Then you have an additional thing. I think it is the great merit of the McCulloch-Pitts psychology of the nervous system that it is able to draw this distinction between the morphology of the networks and the impulses fed into the morphology, that are related in a one-to-one relationship to unique data given through the sensory side of the nervous system. Thus they have a neural psychology that can provide a meaning for symbols, and you get a connection between linguistics and symbolism.

If you take this idea and approach cultures and ask this question of them, when you get back underneath language, it seems to go back to perception and to an analysis of perception. Now what this amounts to is this: If we use the McCulloch-Pitts analysis, that different peoples in different cultures are having different trapped universals, trapped in the morphology of their neural nets, this gives different ways of conceiving of the same facts in different cultures. . . . Which goes to show that you understand the facts you find in a given culture objectively only when you find the concepts used by the people in that culture to conceptualize the facts, and that the Navaho embody a given philosophy, and all different cultures embody different philosophies. . . .

KLUCKHOHN: Harry Murray has commented that if you read American psychology you have the impression that human beings have only a motor system.

LÉVI-STRAUSS: As a matter of fact, when we are trying to use the qualitative mathematical approach, we are not doing anything limited to Western civilization but are trying to incorporate into modern Western thought a traditional thought which goes back to the Pythagorean and the agnostic philosopher and the kind of thought which we find among most primitive people. It is only in that way that we become able to speak the same language they do and not an entirely different language.

By using quite refined logical and mathematical methods, I was able to bring a complete backing to a distinction which is being made by most primitive peoples, that is, the distinction between the patrilinear cousin and the matrilinear cross-cousin, which traditional anthropologists of the kind that Professor Northrop wishes to support have stated for years to be absolutely meaningless. I am quite sure that the Navaho specialist beside me would agree that Navaho mythology and color symbolism and all other kinds of symbolism can only be given an interpretation on which the Navahos themselves would agree, by using that kind of treatment.

BREW: We need very definitely some sort of approach in archeology which will be comparable to the way Professor Chapple has tackled personal relationships. I just want to cite one experience I had when it was necessary for me to go through what amounts to the cultural reconstructions which various people have made of the specific archeological assemblage in the Southwest. I had ten of them, and it seemed to me that six of them were completely and definitely in terms of our recently developed western European nationalism, which seems to me could not in any

way apply to these prehistoric southwestern cultures.

WASHBURN: Professor Northrop implied that we had to do one thing or the other. To me the beauty of anthropology is that it combines a great diversity of approaches, and there is no reason why we should not be both generalizing scientists and also people appreciating the culture for what it is.

One of us may decide that he will be concerned only with the demonstration of detailed functional interrelationship, and another may be primarily concerned with the conception of the whole native life as it appears to the people. The strength of anthropology is that we do not adopt one point of view or the other. We feel great freedom in being a humanitarian one day and just enjoying things another day, and another day being a very hard-boiled scientist.

CHAPPLE: I would like to make one further point, that there is a great deal that can be done . . . in the problems of symbolism and so forth, which Professor Northrop is talking about. However, I happen . . . to be interested in the analysis of functional relationships and acting in terms of the procedures of science; so I think in a sense we have to ask, "Well, what are we doing it for?" and leave it at that, rather than to have this either-or dichotomy, which I think nobody wants.

PATTERN IN CULTURAL PHENOMENA

LINTON: It seems to me the one thing that emerges from this discussion is that we all recognize the importance of pattern but none of us has a very clear picture of how to deal with this as something distinct from the elements which are patterned and are organized.

The situation is like that of the physicist in his studies of electricity, in which it is impossible to get electricity without having it in operational combination with a number of other elements and operating in particular ways.

I shall limit myself to the significance of pattern as it manifests itself in cultural phenomena. In studying any culture, you are dealing with a continuum which consequently includes a time dimension, as well as patterns of synchronic organization. Much of the difficulty in connection with the search for laws of culture, I think, has lain in the attempt to reduce these to simple factors of cause and effect.

We can understand what goes on much better if we think of patterns as diachronic. A certain combination of factors at this point in time will lead to the existence of another combination of factors at a later point in time. You can state a number of these configurational generalizations. You may call these purely descriptive rather than dynamic, but this is about as close as we can come to it. For instance, you can say it is a pattern, that an appliance of superior efficiency will replace one of inferior efficiency; then you can go on to elaborate what efficiency means under these circumstances, in terms of the multiple uses and functions which characterize any element of culture.

When you come to the pattern on the synchronic level, you also have to differentiate between two distinct types of organization. I am perhaps incorrectly using "pattern" here as synonymous with "organization." On the one side, you have the interrelations and mutual adjustments between different elements of culture, which operate together to produce a certain result or series of results. For instance, you can speak of the pattern by which a whole series of elements is combined in the institution of agriculture, directed toward the end of producing food for the group. This patterning is of a rather obvious and simple sort.

There is also a different type of pattern, by which various culture elements are related to certain general values or interests which are expressed in various ways—you might say sub-

tended—by concrete patterns of overt behavior.

I feel that it is this second type of patterning which gives cultures the curious continuity which can be recognized in many of them, where, in spite of the fact that a culture may have profoundly changed most of its obvious objective content, you nevertheless get the feel of the culture as being the same. Something has gone on here. I do not know any term that applies to it exactly—ethos, spirit—but it is a particular set of attitudes and generalized values in terms of which new culture elements will either be adopted or rejected and, if adopted, will be modified in order to make them congruous with this pre-existing configuration.

We see this constantly in cases of acculturation. In the diffusion of elements, what is taken over is the objective part of the new element. People copy what they can see or what they can take, then proceed to reinterpret it in terms of their own value system, sometimes with startling results.

Here you have, in a series of psychological attitudes and evaluations, what seems to be the real model upon which new elements of culture, as they are introduced, are going to be organized and integrated. In the same way, the extrusion of old elements will depend very largely on whether the new elements can actually replace them in all aspects of their functions, whether they can really take over, or whether there are certain points in connection with this basic pattern which the new things cannot fill.

PATTERN IN ARCHEOLOGY

CLARK: I want to address my remarks to the topic of the extent to which, in prehistoric archeology, we attempt to give statistical quantitative expression to changes of pattern in the course of time. I am going to take a very basic theme, that of subsistence. Also I am going to consider the prosaic matter of meat-bones, which you find on most prehistoric sites.

Taking any temperate region of the world—northwest Europe or parts of North America—I want you to consider the changes in the pattern of vegetation during the last 5,000 years B.C., as worked out by statistical means by pollen analysts. The pollen, you see, settled on the waters of a lake and became incorporated in the accumulating mucks. So, by taking samples at intervals, you can build up statistically and illustrate the changing pattern in vegetation, and that changed statistical pattern will bear some relationship to the changes of pattern which did, in fact, occur.

I am going to abstract and suggest that, up to a certain cultural stage, you will find that, if you divide the pollen into two categories, in the lower part of your deposit, once the forests have become established during the temperate conditions, you will find that 100 per cent of the pollen, roughly speaking, will be the pollen of forest trees, of forest vegetation.

Then you will reach a stage in which people are beginning to adopt a farming economy, so your economy here will be one of hunting and fishing and gathering. As soon as you get the introduction of farming in any form, that farming has to be carried on at the expense of the forest, so you get a certain proportion of nontree pollen. Farming in its first stages does not involve total clearance, as you get reversion of forest, repollenization, that sort of pattern. Forest will still make up the main proportion.

Then you will reach a stage when farming simply cuts away the forest. You have permanent farming until the forest has either completely disappeared or reached a point where government comes in and you get reforestation.

Here is something which is quite objective, and you need no higher mathematics for it. You mainly have to be able to reduce things to percentages.

Now you are looking at your meat-bones. You have got stacks of meat-bones from archeological sites. If you analyze all the meat-bones that you get from a hunter-fisher site in terms of whether they are wild or domesticated, all will be wild. Furthermore, they will all be from the forest, with few exceptions; I mean, you will have water and so forth.

Now, when you have reached the stage of a rather low form of farming, you are still carrying on a great deal of hunting, and you may, for the sake of argument, say that half your animal bones are wild animals, but half of them, on the other hand, are domesticated animals. Then you can envisage a later stage in which hunting plays a small part, maybe just being carried on by an upper class or something of that sort. There are still some wild animal bones, but the great mass are domestic bones.

If you analyze this in terms of ecology, you will find that in the second stage the great majority of the livestock will be the type of livestock which grazed in the forest—oxen and swine, for example, in Europe—so these will be forest forms; and in the last stage there will be things like sheep and horse, which are more at home in the open habitat.

When the forest is being cleared, you will find a progressive change in the occurrence of your domesticated animals. You will find that sheep and horse will increase at the expense of oxen and pigs. You can work out these spectra, and you are, in fact, getting a change in the pattern of the basis of subsistence which is closely related to the pattern of changing ecology. You can analyze almost any aspect of material culture in this way.

In the study of Paleolithic flint industries, there was a time when people selected certain types as being significant diagnostically. Now people think in terms of proportion, statistical changes, over a period of time.

MOVIUS: In carrying this concept of culture patterns to an abstract level, it occurs to me that we are overlooking the fundamental factors of this ecological-geographical stimulus, in response to which these institutions which we have been discussing have sprung. Professors Kroeber, Steward, Bates, Clark, and others have emphasized the balance that exists between man and nature. This latter factor, through all levels of subsistence, even in the Lower Paleolithic, imposes limitations on the development of material culture, which, in turn, seems to be reflected in social organization and in language also, as well, perhaps, as in the actual physical traits of the human body. Dealing with the prehistoric levels of material culture for the moment, we are all of us trying to determine the extent and the degree of environmental response which we find in material culture.

But if, by diffusion or by culture contact, new material culture traits appear in ever increasing frequency, which have been developed and introduced from higher sources outside, there is immediately a response in social organization, and certainly there is a response in language.

It therefore seems to me that, in approaching the question of pattern, even on an abstract level, we must qualify what we are dealing with in any context in terms of its environmental or ecological setting.

LOWIE: With reference to ecological correlations of culture, we ought not to forget the seasonal aspect in this connection, which was so strongly emphasized by Durkheim and Mauss. Among the Plains Cree, for example, social

structure is completely different during the winter season from what it is during the summer. I think that point ought to be kept in mind.

SUMMARY

LÉVI-STRAUSS: As Professor Linton has pointed out, we all agree that there is such a thing as pattern and that it is important. Of course, we do not all conceive of patterns along the same lines, but there is no real difference in the way in which we define pattern. According to our field of studies and temperamental differences, we are looking for patterns at different levels, but all these patterns do not exclude one another. I am quite confident that any given field of inquiry can give ground for different types of patterning at different levels, and there are ways to pass from one type of pattern to another.

There are two problems which have been preoccupying most of us. The first, emphasized by Professor Northrop, is that there may be some kind of conflict between two aims of anthropological research. One aim is to recreate a culture in its own terms and to make everybody feel that the people themselves experience it. The other one is that kind of barbarous, butcher-like treatment that some of you feared that those among us who are inclined to a mathematical treatment would be ready to employ.

I would like to point out that there is absolutely no opposition between the two approaches. Quite the contrary, one implies the other. Let us take the linguist. The linguist is trying to define the phonemic pattern of a language; but he cannot do it before he has paid careful attention to all the phonetic variations which have no phonemic value, which express differences in status or differences in style or differences in feeling.

Let us take another example. There is probably no problem in the entire field of social organization which has given occasion for so much patterning as the problem of the mother's brother. No such work as has been done on that could have been possible if Junod had not spent thirty years with the Bantu and explained exactly what the mother's brother's position was in Bantu society.

Let us also take another problem, which is very important from the point of view of patterning in social organization; that is, mother-in-law avoidance, which Professor Radcliffe-Brown now seems inclined to rely upon for explaining all evolution in kinship systems. Before we can start working on mother-in-law avoidance, we have first to know what it is. Is it a kind of repressed hostility? Is it a feeling like the one which we have from a state of nakedness? Is it the kind of attitude we have in front of a superior? Is it the kind of attitude we have when we are meeting somebody who owes us some money, but we are not in a position to claim it right now and we prefer not to talk to him? Before we know these facts, we cannot do any patterning on mother-in-law avoidance.

So I submit that, before we start breaking down the facts and trying to find out correlations or invariants, we must know what the facts are, and it is only the most careful kind of analysis, where we try to picture the native life as the natives live it, which can give us possession of the facts.

The second point is, When are we entitled to say that we have a pattern? Of course, there are many different and many wide ways of patterning. It is possible to find patterns everywhere—you merely have to handle the data in a convenient way. This problem of defining objectively what a pattern is and stating the circumstances under which we are entitled to say that a pattern does exist is probably the most impor-

tant problem confronting social and cultural anthropology at the moment.

I would say that there is a pattern when a given configuration appearing on a given level is identical or exhibits the same properties as the configuration appearing on a different level and is mechanically explainable.

When looking at Professor Washburn's photographs, it came to my mind that the black lines were similar to the one we find in the field of electromagnetics, the line of force of electromagnetic particles. If that is the case and if this magnetic line of force can be mechanically explained, as is actually the case, then we might be able to say that there is a pattern on this definite level. But this is not a new kind of rule. It is a rule which the linguist is using widely in defining phonemic patterns. It is nothing else than a rule of commutability, and it is probably in the line of this commutability and research for possible commutation that the solution to the problem raised by Professor Beals can be found.

There is a last point. I was somewhat surprised that no one emphasized that it was quite a memorable event that the problem of pattern was raised not only in the field of the human and social sciences but also in the field of natural sciences at the same time. This is important, because, when the idea of pattern first appeared in Western thought—and . . . for our purpose I think it is mostly connected with the work of Spengler and Burkhardt, that is, historians—it was thought that this discovery that mankind was organized along a patterned line was establishing a very wide distinction between nature and culture. In France the Existentialist School of philosophy is still making a very systematic use of the pattern idea. But . . . we have reached during the past year a second stage in the history of the notion of pattern, where, through an approach of the human and social sciences, something new is being discovered in the field of the natural sciences which may help us bridge the gap which so long has existed between the two fields.

I was told a few months ago by Professor Julian Huxley that the patterns of colors and lines on the bodies of animals can now be classified on an apriori basis, permitting one to say whether these patterns have sexual function or a protective function or anything of the kind. This is the kind of research that is close to what we might be doing in the field of natural sciences.

Of course, in this approximation of nature and culture, linguistics plays a very definite part, because linguistics is precisely the field which makes it possible to pass from one domain to the other, and that in two different ways: first, because it is possible to show that phonemes have a physical basis, because engineers are now in a position to build up machines conceived according to the principle of phonemics, and these machines will reproduce exactly what the linguist has foreseen will be produced by the machine. And the second point, which Professor Jakobson emphasized, is that the theory of communication shows us that there is a direct relationship between what we may know of the structure of the brain and the way in which communication processes operate. And, of course, communication is not only a field for linguistics, but it can be said that society is, by itself and as a whole, a very large machine for establishing communication on many different levels between human beings.

This meeting has contributed not only to clearing up the idea of pattern but also to forecasting the day when natural and exact sciences and human and social sciences will be able to work together.

CHAPTER XVIII

Values

Chairmen: Robert Redfield and Eiichiro Ishida.

Papers discussed: Bidney, "The Concept of Value in Modern Anthropology"; Northrop, "Cultural Values."*

Speakers: Redfield, Ishida, Kluckhohn, Lévi-Strauss, Bidney, Forde, Mead, Tanner, Linton, Steward, Oakley, Northrop, Hallowell, Nadel, Caudill, Beals, Teilhard, Shah, Hoijer, Haas, Koppers.

INTRODUCTION

REDFIELD: On the board, you will find the following brief classification of three areas of interest:

I. Values as a Category of Anthropological Description
II. Values of the Anthropologist
Values of Others in Anthropology
III. Trends of Values in History
Anthropology as a Source of Values

The important distinction occurs between I, on the one hand, and II and III, on the other. I think that the order of these topics is a natural one. . . .

The first heading, "Values as a Category of Anthropological Description," directs us to the values of people whom we as anthropologists are studying. We begin, then, with the characteristic posture of the student of culture or society, who stands outside it in some sense, looks at it, and finds out about it and reports it. We are asking ourselves under this first heading whether there is something there to be reported which is meant by the word "values," and, if so, what it is.

I may cite in this connection the extremely useful review of this aspect of the problem which Professor Kluckhohn published recently in the book edited by Parsons and Shils,[1] and I will now read a definition of values therein given by him, very slightly shortening it.

"A value is a conception, explicit or implicit, distinctive of an individual or characteristic of a group, of the desirable which influences the selection from available modes, means, and ends of action."

Is this distinguishable from other aspects of what we have to describe in the life of that people which we confront as scientists? How much of it is represented by this category, and how is it distinguished from other categories? Related are other questions. What are the kinds of observations which reveal to us the values of other people and lead to propositions as to their values? Are there particular kinds of activity which are relevant to values especially? Another such question is: What classifications of values are possible and useful? And then there are all the questions of the relations of values to other things, in the reality out there which we purport to describe.

[1] Talcott Parsons and Edward A. Shils (eds.), *Toward a General Theory of Action* (Cambridge, Mass.: Harvard University Press, 1951), p. 395.

This first division may be all you want to talk about. It is certainly the more natural thing to talk about as anthropologists because it reasserts our characteristic posture. We are here, and we are looking at some people over there, and our business is to make propositions of fact about those people over there. Among these propositions of fact are propositions as to their values. But—and here we make the transition to the next two divisions—you might also wish to discuss problems of value as they arise out of a consideration of values in the anthropologist himself. In fact, we are in at least two places: we are in a context of behavior in which is also our subject matter, the people being studied; and in the larger context we, too, are part of the scene to be studied.

The values of the anthropologist may have some relevance to the anthropology he does. In this connection I venture to cite a paper by John Bennett,[2] in which he recognizes in the descriptive literature of Pueblo life two contrasting views of Pueblo culture: one in which the integration of Pueblo culture and society is emphasized; in which it is seen that all sectors of Pueblo life are bound together by a consistent, harmonious set of values which pervade categories of world view, ritual, and so forth; and associated with this integrated configuration is an ideal personality type, with the virtues of gentleness, nonagression, modesty, tranquility, and so on. With this he contrasts an alternative view, which represents Pueblo culture and society as marked by considerable covert tension, suspicion, hostility, anxiety, fear, and ambition. . . . Names here are unimportant, but the former view is particularly marked in writings by Laura Thompson, the latter in writings by Esther Goldfrank. In Bennett's paper the argument is made that the difference in interpretation is a difference in emphasis resulting from differences in the values of the two sets of investigators. I quote from Bennett: "To Thompson's way of looking at things, the end is the most important and significant factor; the 'end' in this case being the unique and rather remarkable world view. To her this phenomenon has rare beauty and aesthetic appeal . . . But to Goldfrank it is precisely the means that count. . . ."[3] It might be that a choice between these two could be made on purely scientific grounds, or it may be that the value position of the anthropologist who carries on the investigation is, perhaps, necessarily a part of his subject matter. Related to this is whether the propositions of the anthropologist are purely descriptive or existential, or whether they are also normative. This raises the question whether such concepts as stability, adaptation, survival, and integration tend to become values to the anthropologist; and it runs over into the question as to the separability of normative and existential propositions anywhere. From this one moves to a much broader consideration of the value positions that are open to the anthropologist with regard to his own scientific effort and his position in the larger society. Professor Bidney's paper challenges what he declares to be a common assumption among some of us that we have a choice between ethnocentrism and a cultural relativism as defined by Herskovits and others.

A third group of questions also falls under this second heading and has to do with choice or conflict between the values of the anthropologist and of the community in which he carries on the study. A part of this we have already seen in connection with certain aspects

[2] John Bennett, "The Interpretation of Pueblo Culture: A Question of Values," *Southwestern Journal of Anthropology*, II, No. 4 (winter, 1946), 361–74.

[3] *Ibid.*, pp. 372–73.

of the papers concerned with applied anthropology. . . .

It may be that we have a very complex situation, in that the value positions of anthropologists may differ from country to country, depending upon the extent to which each reflects the values of his national community. All these are matters open to investigation, or at least to discussion here.

Under division III, I had in mind the possibility of examining the transformation of human values considered very generally against a background of all the history of humanity, still maintaining, as I am trying to maintain throughout this entire discussion, the position of description, and asking questions, Have there been any directional trends in the history of values from the earliest beginning to now, and, if so, what have they been? . . . Here, I venture to cite some pages from Professor Kroeber's *Anthropology* in which he reviews the question of progress.[4] It interests me to recall that, writing in 1915 on the subject of progress, Boas flatly came to the conclusion that, except for technological advance, progress was not recognizable in what we knew about human history and, indeed, that ethical changes showed no directional character. Writing a generation later, Professor Kroeber finds a directional character in at least two other important respects: (1) He takes up the way certain experiences that in early or primitive life are given superior worth, as visions and dreams and so on come to be in later, or what he frankly calls "more advanced," societies of less worth, to be regarded even as neurotic or psychotic. . . . (2) He also reports a change with regard to moral judgment, and I quote a sentence from Professor Bidney's paper which might stand as the heading for these paragraphs in Professor Kroeber's book: "There is no reason why there may not be a cumulative increment in our knowledge and achievement of moral ideals. . . ." Reviewing the way in which physiological events of human life involving blood, death, and decay have come to be gradually eliminated, on the whole, looking at all of human history as one and noting that it happens in many parts of the world (China gives up blood sacrifice for symbolic sacrifice, as the West gives up sacrifice for symbolic blood sacrifice), Kroeber concludes that cultures which had once abandoned these practices react with aversion, disgust, revulsion, or shame—that is, there is a change of evaluation to the decent or good—and advanced cultures exhibit concern with humaneness—a confirmation of Professor Bidney's general statement of possibilities.

The last division under III, is "Anthropology as a Source of Values." It might be that out of the propositions of being and becoming, which are the products of our science, values of the general human community are changed.

ISHIDA: Professor Redfield has suggested that we should distinguish the problem of values of people described by the anthropologist from the problem of values in the anthropologist himself. Since values are inseparable from human behavior or human culture, there is no doubt that they should be treated by anthropologists; and, since values have had some effects on anthropological works, even when excluding those of applied anthropology, these effects must also be taken into consideration by anthropologists.

However, we have also to bear this question in mind: To what extent should the problem of values in the anthropologist be a problem of anthropology?

I believe the inventory paper by Dr. Bidney raises a very serious problem. He criticizes the cultural relativism of

[4] A. L. Kroeber, *Anthropology* (rev. ed.; New York: Harcourt, Brace & Co., 1948), pp. 296–304.

Boas, Benedict, Herskovits, and most of the contemporary American anthropologists, who, as expressed in the Statement of Human Rights in 1947, recognize cultural equality and tolerance for co-existing values as a serial ethnocentrism substituted for absolute national ethnocentrism; and he asserts that today anthropology is mature enough to find some transcultural absolute values having objective validity and capable of universal realization. I can understand why Dr. Bidney says that an optimistic cultural laissez faire such as might be concluded from cultural relativism would no longer be adequate to meet the critical conditions of today. But I suppose there may also be a contrary opinion, that anthropologists can and must come to an agreement only as to what man is. What man ought to be is no business of the anthropologist, and how values should be evaluated is a question which lies outside the scope of anthropology.

Such a negative attitude toward what man ought to be would meet strong protest by Dr. Bidney and others who want to find in anthropology a key to solve the contemporary problems of mankind. . . . Here, also, I think we face a fundamental problem concerning values, whether or not you will discuss it. I myself wish, of course, that the day would come when all anthropologists of the world could co-operate for their common normative ideals and goals; but even if it is impossible in the present international situation, an agreement among anthropologists in the objective study of value problems which Dr. Redfield has just proposed . . . would mean a step forward toward a better future for mankind.

THE DESCRIPTION OF VALUES

KLUCKHOHN: . . . Any anthropologist who has got deeply beneath the surface of any culture has necessarily talked about the values of that culture, but we have been very shy about calling them values. Rapoport suggests[5] that, while anthropological work has actually created a new kind of phenomenon, which he calls a "culture studying culture," that probably has great consequences for thinking about values all over the world, the anthropologists themselves have tended to accept the folklore of the West from the time of Kant on—that there is a *Naturwissenschaft* and a *Geisteswissenschaft,* and never the twain shall meet. Anthropologists have wanted to consider themselves scientists and have been solemnly instructed that science has nothing to do with values and therefore we must be very careful to avoid the word.

As far as I have discovered, it was the 1948 edition of Professor Kroeber's *Anthropology* where values as such were first included in the definition of culture and freely discussed generally. I think this is symptomatic of a trend, though I hasten to add that, in being preoccupied a good deal with questions of value in anthropology in the last five years, my colleagues and I have sometimes wondered whether there was any use in talking about values, since anything which is cultural, by definition, has some place in the value hierarchy.

I want to comment on one or two things in Professor Northrop's paper. He says that a scientific anthropology must be empirically verified philosophical anthropology. The inner order of a given society is put upon an objectively determinable basis only when anthropological scientists not merely observe in the field as many facts as possible but also discover the philosophy used by these people in the culture in question to conceptualize these facts. There is much that is attractive in this solution of the old dilemma. If values are not given by divine revelation or simply accepted as custom, as mores, in the

[5] Anatol Rapoport, *Science and the Goals of Man* (New York: Harper & Bros., 1950).

literal sense, what criterion have we? Well, says Northrop, we have the criterion of the extent to which the primitive postulates of any value system are congruent with natural science knowledge. . . .

To take one other point in Professor Northrop's paper, he says, in his first paragraph, "Since norms express the ethos of a culture . . ." We know that, except under conditions of extreme physiological stress or of reflective behavior, human beings do not respond to a stimulus except as interpreted and defined in considerable part in terms of our culture. The essence of culture is its selectivity. The selectivity of a culture is reducible, in the last analysis, to the value system of that culture; and, in so far as we are ever going to be able to make a series of parsimonious generalizations about any culture, this will consist primarily in a statement of the primary tacit and explicit premises and categories that are distinctive of this culture.

Therefore, unless we are prepared to remain completely at the level of cataloguers, we must deal with the conceptions of both what is and what ought to be, that tend to be distinctive of one culture or another. . . . If we want to be able to make economical generalized statements, we have to be able to deal with the problem of values.

LÉVI-STRAUSS: We are confronted here with two entirely different problems. The first, to which Professor Kluckhohn referred, is that we cannot describe a culture without taking into account its values. If there has been some reluctance in the social sciences to deal with values, it is because, when we speak of values, we are speaking of emotions, sentiments, affectivity, irrational phenomena; and, since the endeavor of any science is to express the phenomena in rational terms, there seems to be some contradiction between the reduction of irrational data to rational terms without destroying these data. Of course, it is perfectly possible that some irrational phenomena cannot be described in rational terms while there may exist a rational relation between them. Thus there is no adequate mathematical way of defining a tree, but if I have two trees which I can intuitively perceive as identical, one being larger and the other smaller, I do not need to define the tree to say that the rational relation between them is that one is bigger and the other smaller. Likewise, it is not necessary to define value in rational terms to take value into account, to say that in a given culture there is a definite value which intervenes in certain circumstances, that it is possible intuitively to identify this value with another value in a different culture and to state the relationship between them. . . .

The problem comes when we try not only to state that there are values and to place them, if I may say so, on the sociological map, but when we try to define what the values are worth. . . .

We can understand the values of any society only in so far as it is similar to our own; if the society is dissimilar, the values cannot be perceived at all. There are no values, they do not exist; no information can pass from one society to another. From that second point of view, I would adopt an absolutely relativistic position, except that there is the thing which we may call "anthropological experience." This specific experience is a purely intuitive one. It cannot be defined; it cannot be described. It is possible only among anthropologists to know that it has succeeded or that it has not succeeded; so here I would bring back into anthropology this affective, intuitive, aesthetic element which earlier I was trying so earnestly to take out of it.

BIDNEY: Social scientists in modern times have definitely attempted as part of their discipline to give us a science

of values. Particularly prominent in this respect have been the sociologists.

One of the major contributions which anthropologists have conceived of themselves as providing is that they have enabled us to evaluate our own values from a perspective which would otherwise not be available to us. The burden of my paper was simply to indicate that this has been one of the self-assigned tasks of the anthropologists and that the time has come when we should undertake a critical analysis of the conditions of the process of evaluation.

We have tended to take over some basic presuppositions as to the origin of values, regardless of what the culture is, which tend to color our interpretation of our field experiences. To my mind, it is chiefly the sociologists who have attempted to give us a scientific explanation as to the origin of values, particularly, say, in the writings of Karl Marx.

The basic presupposition here is that there is a hierarchy of values in which our moral values in particular are explained as functions of social conditions. The question which we have to face is whether we are to accept our values as given empirically, as the culturologists tend to do, or whether it is possible to provide some kind of scientific explanation of values as somehow conditioned by the society in which they happen to emerge. . . .

FORDE: The affective condition to which Dr. Lévi-Strauss referred, whether it is called "sentiment" [or something else] is related to a complex of behavior or even of natural phenomena. Thus the notion of values is a relational one, and I got the impression that Dr. Kluckhohn was reifying values in a way which makes the problem much more difficult or, rather, unsolvable. This leads to Lévi-Strauss's point that the determination of people's values lies in what relations exist between affective attitudes as manifested by subjects and the phenomena which are temporally and spatially concatenated with them, so that, except in certain pathological conditions, one can assume that one was a stimulus for the other, given the general Gestalt in which the thing took place. This is quite a different question, and not so recondite or mysterious as Professor Kluckhohn suggested.

Dr. Kluckhohn's statement that the determination of the values of a society is the key to understanding the society is not a solution in any ultimate sense. It is a generalized statement with regard to one phase of that culture; it is not a statement which is explanatory with regard to the value system itself.

My second point: empirically, human beings are able to communicate with one another, and it is only in what one can loosely call petty pathological social relations that one human being or one group of human beings does not recognize another group of human beings as such. This is empirical evidence for the fact that a human value system is structured on an initial physiological endowment, which itself is subject to variation, and I should consider very important the neurophysiological branches of anthropology, an area in which the maturation of the individual human organism probably has to be studied in connection with the range of the values scale. . . .

From a heuristic point of view, fieldwork experience is indispensable, but it works only because human beings who go into the field have already got, by their education in Western civilization, a very wide knowledge through the historical depth of their culture into a very wide range of value patterns, and therefore already have a basis for empirical study, both of the relation between sentiments and value objects and also of the anterior problem to which Professor Lévi-Strauss alluded,

namely, What are the determinants of a value system?

MEAD: I do not know whether I misunderstood Professor Forde, but I thought that the place this would lead to was a discussion of recognition of self and nonself, treated neurologically, as Richard Brickner has in his papers on telencephalization, in which he brings up two-headed snakes as the type specimen.[6] In some cases the heads co-operate and in some they compete for the same food or one head swallows the other. They exemplify a sort of end-position of the problem of the self and the nonself. Physical anthropologists might build up our knowledge of how animals at different levels recognize and value the self.

BIDNEY: For any systematic study of values, the logical place to begin is the biological nature of man. In terms of Kluckhohn's definition, all values are human values—preferences of men—and there is much to be said for a study of how and in what sense the biological nature of man as an animal conditions the kinds of values which he seeks. It is an empirical study.

TANNER: We should like to know more about differences, particularly neurological differences and essentially brain differences, between people, which must underlie to some extent their personal value systems and, therefore, underlie the value system of the culture in which they move—which, in fact, they constitute or create. The same holds true for endocrinological arrangements. I can cheaply dramatize this sort of thing by saying that one would not expect the value systems of castrates to be the same as the value systems of people who were not. This is obvious; yet any kind of scientific information on the subject appears to be lacking, and this is indeed a very serious lack in our whole science.

LINTON: You have to take into account a good deal more than personal preferences here. In addition to the biological and psychological needs of individuals in general, which is certainly going to be reflected in a value system, you also have a whole series of what I call "social imperatives," situations that have to be met if the society is going to be able to survive and to function successfully. These, as has been recognized since time immemorial, are frequently in conflict with the, shall we say, full-grown desires of the individual; so compromise has to be made here.

REDFIELD: In terms of the familiar distinction between that which is desired and that which is desirable, you will recall that, in discussing values, we are concerned with that which is desirable.

FORDE: Are we going to include negative valuation in this discussion?—because we should at some point.

STEWARD: Perhaps we come to what Dr. Forde is getting at through Dr. Tanner's comments, which make me wonder where the role of physical differences in values would lead us. If we go into fundamental biological equipment, we might come to some sort of universal values, and we can start there negatively. We could say that all mankind dislikes pain and enjoys the absence of it, with certain exceptions; dislikes hunger and enjoys food; and dislikes death. Thus one might work toward sets of values that are common to mankind and agreed to be universally desirable. But then, if one begins to look at differences, I would like to ask the physical anthropologists how one interprets values in the whole cul-

[6] Richard M. Brickner, "Telencephalization of Survival Characteristics." Reprinted from *The Frontal Lobes* ("Association for Research in Nervous and Mental Disease Research Publications," Vol. XXVII [1947]); see also B. Cunningham, *Axial Bifurcation in Serpents: An Historical Survey of Serpent Monsters Having Part of the Axial Skeleton Duplicated* (Durham, N.C.: Duke University Press, 1938).

ture of which they are a part without finding one's self in a racist position? If the facts will lead us so, all right; but we need this point developed a little further.

LÉVI-STRAUSS: We have a universal tendency to value values, i.e., to consider that the realm of value is really the most refined and elevated in the whole human spiritual life. Now I wonder if a quite opposite view could not be taken and if it is not in animal life that we find the value system realized to the fullest extent—inasmuch as we may figure out what is going on in the animal mind. The animal is constantly oscillating between extreme values, abject terror and anguish, on the one hand, and beatitude, on the other; and, in that light, values would be, in human life and society, rather a remnant of animal reaction than an achievement of mankind. It would be possible to say that value intervened only to the level of culture, and that, if we should succeed in a full culturalization of human life, value would altogether disappear.

REDFIELD: These remarks of Professor Lévi-Strauss suggest some differences of understanding as to what value is. I had supposed that a value was a conception that something present might be continued or discontinued or something not present might be achieved, and that it was proper that one should preserve or end it or move toward it. You might, however, find *yourself* wanting quite the opposite. The value exists in the conception that the thing ought to be or ought to be brought into existence, of itself. Inherently, it is proper that that should be the way one feels, whether or not one feels that way.

MEAD: How can we possibly relate value choices, in the sense that Dr. Redfield has defined them, to biological differences, without taking a racist position, and still incorporate them in the culture? Most people who try end up in some peculiar position, a conspicuous example in this country being Charles Morris' attempt to tie value judgments and aesthetics to somatotypes; and then, before you know it, you have the Children of the Sun roaming around Europe. . . . If one wants to relate constitutional type to a particular culture, one can only say that a given culture appears to have embodied a disproportionate amount of the values that might be hypothesized as appropriate to a particular constitutional type. . . . A considerable case can be made that Balinese culture shows a very high proportion of characteristics which could be identified as schizoid, and you can also make a certain number of constitutional assumptions. Nevertheless, Balinese culture is such that the least schizoid, the least leptosome, individual in it can live within it, be a human being, grow up, marry, and reproduce. If that is kept in mind, then the problem of identification of the value system, as preponderantly of a class in a culture, or of one sex in a culture, or of an occupational group in a culture, having preponderantly more congruence with one hypothesized constitutional type group than with another, does not reduce our recognition that every culture still includes the whole of the human race in potentiality within the possible range of behavior within that society.

OAKLEY: The perfection of manufacture of objects is interesting in connection with the history of values. For example, the technical perfection of the Acheulean hand-ax exceeds mere utility, and one can legitimately impute a sense of joy in creation of objects at a pretty early stage in human development.

NORTHROP: . . . I would like to start with Professor Kluckhohn's comment that the people never respond to a fact but to the way they evaluate the fact

and that this is a function of values. If we stop there, it seems to me we are circular, because what is a value? . . .

A culture has a pattern only when the people in the culture, by communication, have a common set of concepts for conceptualizing the raw data of experience. You never get a value by picking out a certain fact, because it is the relational conceptual system in which the fact is conceptualized that determines whether or not it is evaluated.

In a culture with Western types of logical mathematical physics, data and experience are evaluated with insufficient attention to impressionistic immediance, whereas the Confucian Chinese way of conceptualizing experience is based on knowledge rooted in naive observation alone, not in formally constructed theories. In the latter type of theory, inductive data are just phenomena. Scientific knowledge awaits the abstractly constructed theory which the data only illustrate and verify.

This difference is relevant to the point about the physiological apriori. We all have the same central nervous system, which we bring to experience. But this is not enough.

The Confucian Chinese do not conceptualize experience with the abstract concepts of modern mathematical logic. But we know that individual Chinese are some of the best mathematical logicians in the world. . . . They come to the West and learn how to [conceptualize] in another way, showing that the difference is not physiological but culturally conditioned.

This becomes clear if one works with the McCulloch-Pitts neurological psychology, which distinguishes between the neural nets, with the reverberating-circuit neural nets, and the impulses fed into the nets by the data of experience. The neural nets and cortices must be arranged hierarchically, as evidenced by the fact that in the formal structure of language there are certain basic notions and then ways to permute them—primitive or more elementary concepts and others defined in terms of them.

Now I would like to suggest that the word "value" really means nothing but that hierarchy of concepts that gets trapped as a result of the experiences of people; and you have a pattern in a culture when, by communication, the people of the culture agree on that common set of concepts.

Thus, you do not use "values" to define how a people conceptualize a particular datum. Instead, the way you conceptualize the data, the particular concepts that the top reverberating circuits of your cortex have trapped as the primitive ones, seem to be the ones that you can use to define all the other concepts—to take care of all the facts in your experience.

If this be true, then I think we can get an answer to Redfield's question as to whether the anthropologist himself can objectively describe the value system of a culture other than his own without bringing his own values and putting them in it. The answer is Yes, provided that he will use the method of deductively formulative theory, not of induction. One of the dangers of the inductive method is that, with your attention on the facts, you unconsciously smuggle in extemporized prose to describe them, and these descriptions are likely to become the concepts you use.

Mathematical physics pays as much attention to the concepts used to describe or conceptualize observed facts as to the determination of the facts themselves. This requires that every concept used to conceptualize the subject matter be placed in the open. The anthropologist must separate out, in his thinking, the data he observes and the concepts he uses to describe the data, and then exercise his imagination to put his traditional conceptions behind him and work his way into the men-

tality of his people until finally, by being deductive, he finds their system and is able to say that such-and-such a culture is conceptualizing the raw data of experience with this set of notions.

Dr. Lévi-Strauss is absolutely right when he says that there is no contradiction between the theory of a culture as a way of conceptualizing the raw data of experience and the fact that the raw data are sentiments or emotions or passions or aesthetic feelings and intuitions rather than just behavioristic responses or behavior in the light of laws expressed in mathematical formulas. You can conceptualize anything. Through this deductive method the anthropologist can objectively describe the concepts of any number of foreign cultures and then turn to describe those of his own culture or of his own pet theory, which may not correspond at all to that of his culture, and, in this sense, he can criticize. Now to a question pointed up in Dr. Bidney's paper: once we have separated out for a given people their key concepts for conceptualizing the stimuli that hit them, is there any way of passing judgment on whether one set or another set is the correct one?

There is the suggestion that a position like that of Boas of cultural pluralism permits one to be ethically neutral. I do not think this is true; for, even on the basis of cultural or ideological pluralism, you can say that Russian behavior at the present moment is wrong because it is acting as if there is only one ideology in the world, and that is just factually false. Or suppose Hitler said the justification for his conduct was that the German people were a special race and that they had virtues no others had. But, since value systems are not, in fact, just a function of race, the theory is just factually false [and the justification fails]. The fact that cultural pluralism thus has a good deal of ethical implication will, I think, [help] in the present mood between the United States and Russia—if everybody realizes that the United States and Russia are conceptualizing the raw data of their experience with different conceptual systems, and if we stop judging them from ours and they stopped judging us from theirs. The theory does not leave one unable to say anything ethically.

Is it possible to go beyond this and pass judgment on whether the conceptual system of one culture is, in some cross-cultural sense, more correct than another? If we can make the additional assumption that all the concepts used to conceptualize the raw data of experience refer to the data of nature, then denotatively given nature is common to different cultures, and we have a factor to appeal to outside culture. Then one can ask how the people who formulated it came to believe that conceptual system, which, when accepted, leads them to these evaluations. I believe the answer to that question is that, before there was culture, there were only nature and natural man with his nervous system; so that the initial reference for his concepts was nature. Here the analysis of a myth by Professor Lévi-Strauss, yesterday, interested me. The real meaning of the myth reduced to natural science concepts of direction, space, and temperament. I would like to try thus to establish the point that there is no such thing as unscientific conceptualization in any culture. It only seems so when looked at from the standpoint of the inductive data of another particular culture, with its particular intellectual and cultural history. . . .

If this be true and value is nothing but a conceptual system, then one culture is better than another to the extent that its conceptual system takes care of more raw data which hit the sensory end-organs of the human nervous system.

I believe that there are facts about

natural man and nature taken care of in Buddhism and in Hinduism that have been taken care of in no Western scientific theory. And thus, by analyzing cultures to bring out their conceptual presuppositions, then going behind the presuppositions to the facts and experience that led people to think those were the key concepts that brought order and coherence into the raw data of experience, you will find new facts that even all our scientific investigators have never come upon.

This, I think, supports Professor Kroeber's thesis that, with the accumulation of inductive raw data, men in certain cultures at least are able to release themselves from traditional primitive concepts and pass on to novel ones; and in this sense you get moral progress. What you mean is that you get a set of concepts that have a greater deductive fertility to account for the raw data of experience than the traditional set had. This theory of value, it seems to me, has the merit also of putting it on an empirical and verifiable basis.

Professor Tax, as described by Professor Redfield, said, "There is no acculturation." Now I believe, as a result of this analysis, that there is a fundamental sense in which this is true; that, when new facts come into a culture, what the people in the culture tend to do is not to conceptualize the facts coming into the new culture in the terms in which they were conceptualized in the culture from which they came. Thus these new facts coming in get a different evaluation put on them, because the people conceptualize them with the only concepts they have, and I believe this is very important practically.

If, therefore, you are going, as practical statesmen or anthropologists, to introduce technological ways from one culture into another, you have to take to the culture not only the technological instruments but also the conceptual apparatus necessary to understand them, so that they put the evaluation on those instruments necessary to operate them correctly and to understand them.

LÉVI-STRAUSS (submitted in writing after session): In his reference to my speech, Professor Northrop misunderstood my position. I do not admit that myths are about symbols of nature. It just happened that, dealing with origin myths, I referred to natural elements. But it would have been equally possible to describe these myths by using ritual operations, kinship behavior, etc. The nature of the elements is irrelevant to the structure of the myth; what is important is how certain things are correlated, not what these things are. They may well be anything.

MEAD (submitted in writing after session): Regarding Northrop's comment on giving Asians our methods of thought as well as techniques, the committee of editors who prepared the forthcoming *Culture Patterns and Technical Change*,[7] took the position that we should strip our technical methods to the bone, make them as culture-free as possible, and help create situations within each importing society in which educated members of that society could develop their own characteristic methods of utilizing them.

VALUES OF THE ANTHROPOLOGIST

HALLOWELL: . . . The two main currents which have influenced our discipline both derive from eighteenth-century rationalism and the Romantic movement. A number of the main concepts and terminology evident at this symposium are so derived. [Arthur Lovejoy, in speaking of the Romantic movement, stressed holism and dynamism] and the contrast between di-

[7] M. Mead, L. F. Krank, D. D. Lee, C. Holt, E. D. Chapple, J. Useem, and G. Saslow, *Culture Patterns and Technical Change: A Survey Prepared for Unesco by the World Federation for Mental Health* (1952).

versitarianism and uniformitarianism. Two inferences were drawn: first, the necessity for tolerance and catholicity . . . and, second, the duty to cherish and intensify differences, even idiosyncrasies. . . . It is, of course, no accident that since the key values of the Romantic movement became part of the liberal-democratic tradition, we frequently find ourselves in that camp. As Dr. Bidney says: "In retrospect, it appears that American anthropologists continued to reflect the prevailing attitude of their democratic society. As liberals and democrats, they merely accentuated tendencies inherent in their culture but professed to have derived their 'higher tolerance' from a comparative study of primitive cultures. They assumed uncritically the value of cultural differences and their mutual compatibility." It does seem that in considering our own values, both personal and professional, we should somewhere along the line become aware of ourselves in the cultural-historical framework of western European thought.

Emphasis upon the necessity of tolerance in looking at the values of other cultures, and sometimes even a resistance to making any systematic discrimination between sets of values, whether aesthetic or moralistic or what not, has led to a sometimes ambivalent attitude toward the values of our own culture and, at times, to an overevaluation of the values of the nonliterate peoples with whom we come into direct contact. It has often been implied, if not explicitly stated, that the psychological effects of acculturation are always "bad"; but this viewpoint is often held by those who have deplored the colonization by Europeans of various parts of the world, in so far as it has led to the transformation or extinction of the cultures of aboriginal peoples. As a corollary it is sometimes implied that the native cultures were "good" and the people better off as they were. The implication is that Western culture is not all that it should be, at least in the form offered among non-European people; but it is not always made clear whether it is not the manner or condition under which acculturation took place that was bad rather than acculturation or cultural change as a process. The viewpoint mentioned may also be tinctured with a high aesthetic evaluation of the exotic or different. . . .

Humane as this viewpoint may appear, the simple logic back of it implies that the transformation or extinction of anything that has once come into existence is bad. This is difficult to maintain, since the transformation, disappearance, and replacement of organic forms of life is the essence of evolution, and in the course of this process higher forms of life have emerged from lower ones.

In so far as human cultures are concerned, change, transformation, development, and often the disappearance of cultures must be assumed to have been intrinsic to human history long before the expansion of European peoples in the modern world. But ever since anthropologists eschewed unilinear evolution, the question of lower and higher levels of culture development has been pretty much avoided, whereas in the older discussions, not so far back, of the process of diffusion, it was frequently emphasized that contact between peoples has often been the source of novel and positive cultural developments.

My point is that the whole problem has been discussed too categorically and that the various judgments expressed or implied may be even more culture-bound than we have realized.

In the section of his paper called "Functionalism and the Biological Evaluation of Values," Dr. Bidney points out, for example, that the only absolute value implicit in Malinowski's theory is survival value, all other cultural values being understood as a

means to this end, while for Radcliffe-Brown the ultimate value is the survival of the society, to which other cultural values are instrumentally related. While I fully agree with Dr. Bidney when he says that a critical and comparative study of values is highly desirable and that "the choice is no longer between a romantic cultural pluralism and a fixed evolutionary absolutism," I should like to raise the question whether it is not possible that a psychofunctional criterion may not provide another angle of approach to cultural systems and their central values?

The problem here would be to measure the psychological consequences of one mode of life or system of values as compared to another at the level of psychodynamics, taking as the criterion some form—and I will characterize it here as optimum psychobiological functioning. I am not talking about ideal personality or anything of that sort—it is not so simple. But I do assume that, even in the light of our present knowledge, mental health as an ideal functional concept is far from being vague and superficial, since it is based upon a well-tested body of knowledge concerning the actual mechanisms and dynamic principles of human behavior. Mental health, as I am using the term, is distinguishable from the concept of adjustment.

One of the major differences between animals and man is that, in the case of the former, adjustment or adaptation is primarily a function of innate biological equipment, and the infra-human animal of any particular species makes his individual life-adjustment within a relatively narrow range of possibilities. In the human species, on the other hand, where life-adjustment is so largely a function of learned behavior in a particular culture and where psychological mechanisms are present which require such a delicately integrated balance of forces, a very considerable range in actual life-adjustment is possible. Thus persistent neurotic, psychotic, delinquent, or criminal behavior patterns may all be considered equally adjustive from the standpoint of a kind of psychological homeostasis, but they are not adjustive on the level of mental health. There may be biological and social survival far below what I am characterizing here as an optimum level of psychological function, and our present knowledge makes it necessary to differentiate a positive higher or optimum level of psychological functioning, call it what you will, from a lower or less positive one. I am referring here to the differentiation made by O. H. Mowrer, a clinical psychologist who has also had contact with anthropology, between what he calls "integrative" and "nonintegrative" behavior. Whereas the former is more psychologically rewarding than punishing, the latter is balanced in the opposite direction, since adjustment is achieved only at the expense of partial psychic self-destruction.

On the assumption that the concept of mental health or its equivalent is scientifically founded and empirically verifiable, it would follow that it is not altogether culture-bound but has universal significance, both as a concept and as a value. If this is so, in addition to recognizing the relativity of different value systems and the inherent difficulty in making any appraisal of them with reference to any absolute ethical or other standard, we should not close our minds to the possibility that, from the standpoint of the psychodynamics of human adjustment, the efficiency of value systems of different societies may vary significantly.

You may ask, What is the role of different value systems with reference to the general level of personality adjustment found among different groups when measured by some standard of optimum functioning? Within this

frame of reference, value systems become one of the most important culturally constituted means whereby human beings make their fundamental and characteristic adjustments to life. They may be viewed in terms of their integrative function in relation to the goals, motivations, and learning processes of individuals in the course of psychological structuralization and the total functioning of personality. Of significance are not only explicitly articulated values in the form of ideals or culturally phrased norms, but also the unformulated implicit values that are expressed in unconscious attitudes lived out in behavior or imbedded in a system of beliefs about the nature of the world.

Two lines of investigation should prove fruitful: (1) studying value systems of different societies from the standpoint of total personality integration and functioning in the light of our knowledge of mental health; (2) studying more closely the psychological aspects of acculturation, particularly the effects of readjustments involved, with reference to the personality structure and value systems.

One question that arises here is how far and under what conditions the value system of one culture is transferable to individuals in the process of acculturation. In my Ojibwa investigation, I experimented with a series of signs of psychosocial adjustment that can be derived from a Rorschach protocol.

Individuals were rated on a 5-point scale. This procedure made it possible to compare the ratings of groups of individuals who differed in level of acculturation and to handle the data quantitatively. I did not know whether I was going to get any significant differences, but significant differences in psychological level of integration were found that correlated with levels of acculturation. I do not hesitate to characterize the psychological consequences of acculturation in the most acculturated group as bad, this not because I believe that any readjustment required by the exigencies of the acculturation process is intrinsically bad or because these Indians have lost most of their aboriginal culture, but because I have some measure, both quantitatively and qualitatively, of what I mean by this evaluative term: the group referred to was the only one of the three compared in which more than 56 per cent of the subjects in the sample fell into the lowest levels of this 5-point scale—in what I call the badly or poorly adjusted category. I can generalize still more broadly by saying that I think it is absolutely bad when more than half of any group of human beings shows demonstrable signs of psychological disorganization, whatever the condition of society, while instances of psychological readjustment on a positive functional level are lacking.

NADEL: For the sake of brevity, I shall take for granted the proposition that the possibility of a value-free social science is an illusion. I do not mean value judgments which express private idiosyncrasies and similar emotional reactions, but those derived from generalized conceptions of an evaluative kind, which, in turn, basically refer to certain decisions which we have to make in calling things good or bad, better or worse. Mostly, these value aspects are concealed in our writings and in our presentations of culture in society. . . .

Having made this assumption, the first question is: Where does the anthropologist find his value criteria or on what sort of arguments does he base his decision to call things bad or good or at least imply some logical evaluation?

Now the value criteria used by the anthropologist are far from being fortuitous or subjective, are entailed in the

conceptual framework of his discipline, and, to that extent, are objective. . . . [By "entailed" I mean] they are not free but lead on from any conception or any principle that is once adopted.

In social inquiry, the two basic concepts which entail our value judgments are: (1) the concept of society and culture—and, for the purpose of this discussion, it is unnecessary to distinguish between the two—and (2) the concept of the psychophysical individual, or, if you prefer, the personality. The first concept compels us to think along the lines of the stability or instability of the societies, the integration or disintegration of cultures, euphoric or dysphoric states, and so forth. The second concept compels us to think along lines of need, satisfaction or frustration, normality or abnormality —Dr. Hallowell's point of mental health, adjustment and maladjustment, and so forth. All these are instances of a plus and minus scale on which we place the phenomena we observe, and, obviously, among the phenomena we observe and place on this scale are the value judgments of the people we study, who themselves place the objects of their environment on a similar scale.

This can be expressed differently. We refer to better or worse solutions of certain existential problems, which we posit as soon as we employ these two basic concepts of society-culture and the individual because we think of a society persisting and remaining stable and individuals persisting, functioning adequately, or surviving. . . .

The two criteria which I have mentioned may offer conflicting evidence. We often speak of a society which maintains its stability at the expense of certain need satisfactions or gratifications. And it is clear, too, that these value criteria belong to a very high level of generality, so high that it oftens ceases to be useful in any more concrete analysis of specific cultural or social situations. If, then, my thesis that social inquiry cannot be value-free still holds, we must admit that, in the area with more particular and concrete phenomena, we are again without the guidance of entailed criteria and seem bound to rely entirely on private and arbitrary ones. I would suggest, further, that even in these two areas our criteria are not entirely arbitrary or subjective. Though not entailed in our conceptual system, they are at least grounded on it. . . . Through our concern with the systematic study of human societies everywhere, we have gained a certain insight and have developed ways of thinking about phenomena and, among them, about human values which are being fed back into our civilization and into our traditional thought. . . .

One example is the idea—the value —of tolerance. We anthropologists, in our own way, have contributed thoughts of value to this development of an idea and have to that extent already helped to reshape, remodel, the tradition of thought which in other ways we have employed or utilized in our own studies. In consequence, when we value tacitly or otherwise any given cultural situation, our judgments are grounded not only on decisions made for us by our civilization but also on the systematic studies which we and others have carried out and which have in some measure shaped these very decisions. You will notice I have not used the term "ethnocentric" or "cultural relativism." These terms are simply too inadequate to do justice to an inquiry like ours, peculiar and unique in many ways, for here we have social beings studying social beings, and we might, I think, rightly say that here we have society becoming aware of itself through the study of society, submitting itself, all its phenomena, to self-scrutiny.

Only one more thing remains to be said. Since these value judgments are always present, we must not conceal them. We must clearly state our own ethical premises. They will not be the same in the case of every person, since, after all, we have not one philosophy in two thousand years of history but several. Whatever we mean by that common word "truth," it only grows out of open scrutiny, out of criticism, and out of controversy. If, as anthropologists, we cease deceiving ourselves about the objectivity and value freedom of our discipline and openly define our value criteria, then I think we shall contribute to that self-scrutiny which is the life-blood of our science.

CAUDILL: I worked under Dr. Hallowell's tutelage with the group of Indians he was referring to. There were a number of anthropologists working with them, one of whom was seriously impressed with the puritanical aspects of the society, whereas I was more impressed with the boisterous, drunken chaos into which this town was turned every Saturday night. . . . One has to take into account the anthropologist in interaction with the subject and report on what goes on in this interactive situation rather than just report on what the person says. This technique is possible to learn and is as much a technological need as new types of tools.

With reference to what Dr. Northrop said and to Dr. Tax's statement that there is no acculturation, there is a group of Mohawk Indians at Caughnawaga Reservation just across the St. Lawrence, who, after three hundred years of being completely beaten down by Western society, very quickly became, by the chance building of a bridge next to the reservation, high steel bridgeworkers. There was an efflorescence of the old native religion; the men left to work on bridges and came back in the wintertime to talk about their exploits. In general, there was a revival of a certain pattern of life, with different content but essentially the same structure as existed in the old society. Why this persistence? Work in Chicago on the Japanese-Americans and their acculturation shows that second-generation children operate explicitly and almost totally on a Japanese system of values. . . .

BEALS: There are two points that I wanted to bring up. One is a question whether we are not actually dealing with two levels of phenomena when we discuss values. We have had discussed those types of values which are rooted in biological phenomena, which are essential to survival. These are of an order somewhat different from those which have strong affective qualities, which are related to world views and things of this sort. I wonder if setting up distinctions or hierarchies of values in this way might not prove that some are much easier to deal with than others, and susceptible to different techniques.

Second, if I have understood some of the undertones correctly, there is a feeling here that we should not only make explicit our values but also proselytize with some of them. This brings up some very difficult problems. Certain observers have, for example, indicated that one of the primary values that has developed in Western civilization is warfare. Dr. Hallowell brought up the value of mental health, and I believe there are few cultures which have a higher incidence of bad mental health than Western civilization. This being so, if this were an action body, I would be tempted to suggest that we set up a committee consisting of our Asian delegates to create some values for Western society.

BIDNEY: I find myself very much in agreement with the remarks of Dr. Hallowell, which should bring to the forefront the concept of dysfunction as

a negative value to which we must give equal consideration in evaluating cultural institutions.

With reference to Professor Northrop, it seems to me that we need not set up any disparity between his theoretical evaluation of values and the practical evaluation of values which is implied in the concept of mental health.

It seems to me, as Dr. Beals has just suggested, we may be dealing with values at different levels, psychobiological and ideational. Our ideational values we may have to evaluate from a theoretical, deductive point of view, as Professor Northrop has suggested; but there is also the empirical criterion of function or dysfunction for which we need not have immediate and direct recourse to theoretical analysis.

TEILHARD (statement revised after session): How much can, or even must, anthropology help us in the task of determining human values? Two main conditions are required of an object or goal in order that it may assume the function and dignity of a human value. The first condition is for this object or reality, whatever it is, to represent, at least in our momentary field of vision, an unchangeable, irreversible advance direction for human activity. The second is possession of a certain character of universality, not so much understood as an abstract or apriori idea, but rather universally understood as a process or pattern, closely connected with the concrete, dynamic structure of the universe.

This being admitted, anthropology, provided that it keeps its natural connections with general biology, is able to detect in ourselves the existence of such a directing *grandeur*, absolute as a parameter of human advance and universal as a physical reality.

Many factors have been considered in the course of this symposium for the purpose of reaching a better understanding of human evolution as a whole. The most conspicuous human characteristic, not mentioned anywhere, is the fantastic and unquestionable property exhibited by mankind of not only continuing to diverge and differentiate as any other animal group but, in addition, to become more and more irresistibly united into a single natural whole. The investigation of ancient cultures should not obscure for us the dominant fact that the specific characteristic of the modern world is no longer divergency but acceleration of a process forcing man to become economically, technically, and mentally one.

Now, if we consider this modern drift of mankind toward material and mental unification, is it not, I ask you, a remarkable fact that this movement in the direction of physical and psychic oneness shows precisely the two conditions expected from any norm of human values, namely, irreversibility and universality? Irreversibility first, since nothing can stop man any more in the process of getting ultra-organized and ultra-reflected on himself under the pressure of various planetary forces; and universality, too, since the process of increasing complexity in which we are caught is one of the most general detected by science in modern life.

From this angle I feel convinced that anthropology, provided that it understands its natural place at the head of a general science of evolution, is perfectly able to supply modern man with an objective basis for a new ethics, which we need so desperately: a basis, I mean, firmly established above any subjective preference on the unquestionable fact of natural human convergency. . . . Since everybody, East and West, is equally converging and equally conscious of this general convergency, it seems that we should find on this ground a basis of understanding. Of course, many conflicting plans

of unification can be developed on this basic assumption. Where to find, how to determine, the best center of human convergency? Still, it would be a very great step in the direction of human achievement if each anthropologist, in addition to his special line of study, should work out for himself and spread around himself this fundamental idea that, after all, man is forced biologically to be one—or to fail.

In the future, no ethics and, I dare say, no religion will be able to grow except by developing such a sense of biological oneness. This is one of the strongest parts of my scientific creed.

MEAD: Père Teilhard surely does not imply that convergnecy toward a larger whole is antithetical to diversity among people. We have a great deal of evidence to show that large complex societies very often give more play to individual differences than highly distinct, small societies that have to coerce the individual more into forms that are beautifully diverse when seen vis-à-vis other societies, but extraordinarily uniform ones when seen from the standpoint of the individual. It would be quite possible for us to commit ourselves to convergency for society and differentiation of culture within societies, using the psychocultural criteria that Professor Hallowell introduced as the optimum psychocultural development of each individual, realizing that those will be extraordinarily different and could in time take in the constitutional differences that Professor Tanner is interested in.

TEILHARD: I agree with Dr. Mead. In nature at large a convergency is always associated with a different position of the elements. I suppose in the world today we are forced to find a real way to unify, and we are going to be unified in such a way that, instead of becoming uniformized, we are going to be differentiated in a directional way.

SHAH: When Dr. Northrop referred to China's being unable to take the benefit of modern values, I want to know whether he referred to modern technilogical advances or to the incapacity to accept democratic ideas and the tendency to run away to communism? . . .

NORTHROP (statement revised after session): I had especially in mind the introduction into China of Western law. There is an essential connection in any culture between the following three things: (1) the ethical and legal method of settling disputes, (2) the symbolism, whether it be pictographic or more strongly syntactical; and (3) the epistemological content of knowledge of individuals and events in nature. . . . The language of the Chinese is pictographic. In pictographic symbolism, grammar and syntax, while present, are at a minimum compared with syntactical symbolism. Thus a pictographic symbolism, such as that of the Chinese, is the ideal language for a nominalistic epistemological theory of nature. The suggestion arises, therefore, that a pictographic symbolism, a nominalistic epistemological and a mediational rather than a code method of settling disputes go together in Chinese society.

[A second example is that of Aryan Hinduism, which contains a dual ethics, nonviolence coupled with legal codes, for example, those of Manu. In Brahmanism we again see] the code method of settling disputes, syntactical language, and the conception of a truly known individual as an instance of a grammatical rule to be essentially connected. [The emphasis within Brahmanism upon formlessness explains the position of Gandhi that legal codes are not proper instruments for resolving the disputes of man.

[A third example is the speed with which Japan mastered Western scientific and technological concepts, as

compared with the Chinese. This is due to a combination of the Shinto religion and the fact that, before Chinese influence, the Japanese language had a syntactical vocal symbolism which it still retains. This permitted the Japanese to think relationally and syntactically to the maximum degree and thus quickly master Western science.

HOIJER (submitted in writing after session): The record should show that linguists do not recognize a verbal pictographic language at any level of human development, oriental or occidental.

HAAS (submitted in writing after session): The characterization of Chinese writing as pictographic is an extreme oversimplification. The proper understanding of the system of writing necessitates an understanding of its development as a long and complicated historical process.

The characterization of Japanese as "syntactic" is virtually meaningless as a device for proving a more sophisticated level of thought-processes. All languages are able to express syntactic relationships—whether this is done by affixes, by separate words, by word order, or by other means. And the means employed by a particular language does not per se make that language superior in expressing thought-processes of a certain type. Such a position is completely rejected by linguists. Those who occasionally revive such a point of view have no real understanding of the languages they deprecate as inadequate.

NADEL: I have an example which seems to contradict Professor Northrop's speculation that if you have a very strong value system and a system of certain categories which organizes your conceptual universe, then a conflicting system of categories must be ineffectual. In the culture of the Empire of Abyssinia there is a conception that in any legal dispute the complainant alone has the right to bring witnesses and fight the case, the idea being that he is the person aggrieved and should have all the latitude. In practice, the complainant usually wins the case, after which the defendant brings another case as complainant, the first complainant brings another case, and so on, often for fifty years or more. Now it fell to my lot, as a political officer attached to the Army during the war, to hear certain disputes. Because of limitations of time, I applied some of my practical value judgments, called the system bad, and introduced a different one. I explained to the people concerned that in the British system you heard both the complainant and the defendant at the same time, you let them argue the case out, the judge made the final decision, and there the matter rested. The resistance at first was enormous, but I exercised the influence which I fortunately possessed, established native courts, and disappeared. When I was in quite a different job in a different part of the country, there was one day a mass immigration from another province of Ethiopia, and people came to me and asked me to hear their case. I said, "I am not a magistrate; you've got your own courts." They said, "Ah, no, but you have introduced a new idea of justice into this country and we wish to be tried by the new justice."

KOPPERS (Lowie translating): I am gratified to hear these problems of value discussed. In Germany they are also discussed, but mainly by philosophers. The difference may be partly that in the United States there are so many anthropologists that a number of them can devote themselves to the discussion of this range of questions.

KLUCKHOHN: Dr. Majumdar very correctly remarked the other day that it is difficult for an outsider, say an American, to deal satisfactorily with the value system of a great cultural tradi-

tion like that of India. I think we would all agree. At the same time, I would like to suggest that, at least so far as the literate societies are concerned, our progress is going to be very limited unless this kind of thing is done reciprocally; that is, we badly need people from India, Japan, and China, to come and study our American values and vice versa. This is an indispensable step. We have to see a value system described from this point, that point, and the other point; and I think we already know, as far as American values in a very broad, not too systematic, sense are concerned, that, in spite of the useful work of Warner and many other anthropologists, we have learned more about American values from de Tocqueville, Bryce, and other outsiders than we have yet learned from "modern anthropological methods" carried out by ourselves.

REDFIELD: That practical suggestion seems an appropriate place to close this meeting, and I am tempted, in doing so, to generalize upon it in the words that were used by E. B. White in the *New Yorker* at one time. I myself have come to feel, especially through the remarks made today, that, while every effort to make one's self objective and self-critical in anthropological endeavor is to be pursued, the quality of our individual humanity is, at the same time, an inseparable part of what efforts we make, even in our anthropological work. The appeal that Clyde Kluckhohn has just made for contributions to the study of values from all parts of the world goes a long way to make our results more worthy of acceptability. E. B. White commented upon efforts of certain well-meaning citizens to produce a balanced textbook for the schools: "That is nonsense. We don't want a balanced textbook. We want a balanced library." In that spirit, we do not want an anthropologist so balanced that his neutrality sterilizes him for observing, but we want a balanced body of world anthropologists.

CHAPTER XIX

Anthropology as a Field of Study

Chairmen: S. L. Washburn and S. Sangvichien.
Paper discussed: Métraux, "Applied Anthropology in Government: United Nations."
Speakers: Washburn, Sangvichien, Ishida, Namba, Majumdar, Mandelbaum, Shah, Held, Senyürek, Ivaničck, Dias, Linton, Lévi-Strauss, Mead, Forde, Lowie, Greenberg, Hallowell, Nadel, Tax, Rowe, Clark, Eggan, Murdock, Métraux, Northrop, Movius, Eiseley.

INTRODUCTION

WASHBURN: Is anthropology a science or a humanity or merely a federation—a loose grouping of people who have common interests and a desire to communicate with one another?

An understanding of other people's cultures was at first a matter of no concern because, obviously, the other fellow's culture probably was not worth understanding. There followed an era of "intellectual imperialism," when the European-trained scholar assumed that he could understand the cultures of other people without, or with a minimum of, their co-operation. Now we know that we must have the active co-operation of people if we want to understand their ways of life. . . .

SANGVICHIEN: . . . Anthropology has advanced tremendously in the United States. Its direction has differed from that of its European counterpart. It is far beyond the understanding of some of us in the Far East. Since different nations have different problems and pursue different lines of research, I would like to turn first to representatives of other countries so that we may hear what lines they would like anthropology to take in their countries—this as a step toward better understanding and genuine co-operation in our discipline.

ANTHROPOLOGY AROUND THE WORLD[1]

ISHIDA: Japanese anthropology began late and is still in its infancy. Its contemporary status can be summarized as lacking in balance and in integration of its fields. No Japanese university yet has an independent department of anthropology, and archeology, linguistics, etc., are offered as isolated courses in some host department. Archeologists rarely have ethnological training, linguists and physical anthropologists, pursue their separate interests, and many ethnologists are, in turn, uninterested in the general findings of the other fields.

There are three positive gains which the last war has brought to Japanese anthropology, despite the loss of Japan's colonial territories for field research: (1) the collapse of the militaristic regime has freed Japanese anthropology of the long-imposed taboo on speaking any truth which might contradict the conception of the divine origin of the imperial family. Japanese children are now being taught anthro-

[1] Similar matters were discussed in the thirteenth session (see chap. xiii, pp. 218–23).

pologically oriented conceptions of the place of the Japanese people among mankind. Archeology has become a favorite topic, even among junior high school boys and girls. Folk ethnology has gained weight in social studies newly introduced into school education as a way for the Japanese to examine themselves. (2) The ethnocentric concern hitherto basic in Japanese anthropology, confined as it was to studying the ethnic origins and the native customs of the Japanese, is turning into a broader interest in mankind as a whole. Whereas other parts of East Asia were considered primarily to throw light on Japanese background . . . anthropology now offers a firm footing from which the Japanese can launch out toward becoming citizens of the world. In anthropology is the humanism in science they so vainly sought in the past. (3) The need for integrated cooperation among the anthropological sciences has been keenly felt since the war. The widespread recognition of this need is evidenced by the formation in 1948 of a "League for the Study of Human Affairs."[2]

In summary, these postwar phenomena are signs of a thriving Japanese anthropology. Interest in anthropology, as a comprehensive study of man, capable of being applied to practical human relations, is becoming more intense. Our urgent need is for the establishment, at least in some large universities, of independent departments of anthropology with a balanced representation of fields, in order to train teachers and researchers in anthropology. . . .

NAMBA (submitted in writing after session): Sociological and anthropological research on Japanese societies and cultures has been carried out by many American scholars, but unfortunately most of it is fragmental and not integrated. Ruth Benedict's *The Chrysanthemum and Sword* is an excellent work, considering her lack of field work in Japan. As a whole, it is all right; but detailed explanations of Japanese culture seem to be pretty far from our standard. Gorer's study of the national character of the Japanese is too one-sided and also lacks accurate interpretation. Though they have a very good reputation, Embree's classic Japanese studies, *Suye Mura* and *The Japanese Nation,* already seem to belong [to the past]. Much notable research work in postwar Japan has been done by the Natural Resources Section and the Public Opinion and Sociological Research Division, CIE, GHQ, SCAP (for instance, *The Japanese Village in Transition* [1950], but has been done mainly to meet the specific needs of occupation forces and is not accessible to the public. Intensive studies of small villages and their cultures by a research group from the University of Michigan are still continuing but are restricted to one region.

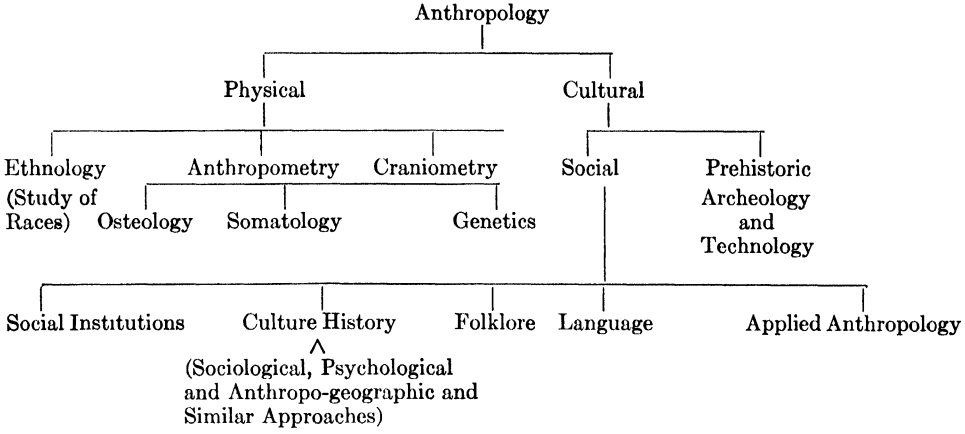
Much valuable field work has been conducted by Japanese scholars, during and after the war, but their researches tend to be individualistic in method and erratic in problem selection.

What we need now is a project to seek out fundamental patterns of culture, on a comparative basis, with integrated field methods. It is very important to grasp the dynamics of acculturation in postwar Japan as well as the values in Japanese subcultures, in order to have a sound basis for policy-making.

MAJUMDAR: Time limitation prevents a complete account of the progress of anthropology in India, and I shall simply indicate the trends and activities pertinent to this field. They will be

[2] See *International Directory of Anthropological Institutions,* ed. William L. Thomas, Jr., and Anna M. Pikelis (New York: Wenner-Gren Foundation for Anthropological Research, Inc., 1953).

treated under two broad heads—physical and cultural, as follows:



An account of archeology is included in my paper for the *Handbook of World Resources for Research and Education in Anthropology* and is thus omitted here. . . .

The scope of our work in craniometry and osteometry has been limited, as our finds of bones, fossil, or other have been few and systematic search in this direction has not yielded many dividends. . . .

Work in anthropometry began as early as 1891, when Sir Herbert Risley initiated a racial survey of India. [Inaccurate results due to absence of standardized measurement techniques and of attention to personal equations of the investigators, plus unwarranted selection of subjects] led to eventual abandonment of this classification. Yet, in the absence of a more competent study, it found a place in all books on ethnology, history, and geography, and today appears in most anthropology textbooks. In 1931, the census department initiated a second racial survey, in which a new classification was adopted . . . and interest aroused by this inquiry led to a rapid development of anthropometric research, resulting in completion of three more racial surveys during the last ten years. It has been possible to map out the frequency distribution of stature, cephalic indices, and other anthropometric traits of the Indian population . . . and great advance has been made in perfecting statistical methods for anthropological data. . . .

In recent years, serological research has been much emphasized and has enabled us to map out the distribution of isogenes and to correlate serological data with anthropometric, thus aiding the study of intra- and intergroup differences. Twin studies, inheritance of diseases, and growth studies figure in anthropological programs, as well as investigations on the nutritional status of social groups.

The first significant impetus to cultural anthropology can be traced back to 1774, when Sir William Jones, in his inaugural address to the Asiatic Society of Bengal, defined the scope of its inquiries as the entire field of studies concerned with man and nature. There followed a period of feverish activity on the part of administrative officers of the then Indian Civil Service to record the ethnography of the country, taking the form of several detailed volumes of ethnographic literature on customs of the tribes and castes of India.

In 1912 the first monograph on an Indian tribe was published by the pio-

neer anthropologist of India, the late S. C. Roy, and was followed by several more monographs by him and his students and by the personnel of the Indian Civil Service. Many of these monographs are known for meticulous and objective evaluation of Indian tribal life.

Since 1921, when anthropology was first introduced into the curriculum of Calcutta University, it has enjoyed state encouragement. From the very beginning Indian anthropologists saw the need of orienting their studies to the requirements of day-to-day life, and anthropology slowly acquired an applied role.

Since it is believed that anthropological knowledge must be applied to the creation of wider scope and opportunity [for individuals and groups], certain anthropological problems are receiving priority. Some of the spheres of interest of cultural anthropology in India are studies of caste mobility; cultural dynamics; transition in tribal life; the processes of acculturation, assimilation, and the rehabilitation of refugees; causes and consequences of the infamous Bengal famine; problems of contact and clash of cultures in the northeast borders of India; the nature and cure of labor unrest; conflict and social distance between castes and communities; studies of tension between social groups; and rural and community studies.

The first anthropological study of the pattern of Indian city life has been undertaken in Lucknow, financed by the municipal board of Lucknow, and sociologists, psychologists, and statisticians are working with anthropologists on this project. The need for such collaboration has been fully recognized in India.

In our study of tribal transition, we are surprised to note the speed of the acculturative process in backward areas. Tribal labor migrants to plantations and liminal towns, to the steel and textile industries, and the degree of adjustment effected by these people to technology do not support Professor Northrop's illuminating conclusions. . . .

MANDELBAUM: . . . There is, in various countries of South Asia today, a fine opportunity for advancing anthropology . . . by encouraging anthropologically minded government workers, for example, to take some training in anthropology. In Pakistan, Burma, Indonesia, Ceylon, and so on, there has been considerable efflorescence in sociology but relatively little as yet in anthropology, although conditions are ready for it. . . .

SHAH: In recent times, even the primitive tribes of India are becoming aware of their status. Of course, dynamic changes are taking place in India that are of great importance. The old feudal arrangements are all practically destroyed, and the constitution provides for both equality of opportunity and treatment even among the very primitive peoples. We should not treat our knowledge as garbage but rather as something important for the purpose of bringing out the originality of these people, in the light of modern culture. Let us not treat each civilization, each culture, from a particular standard of values but study it as an independent object, without assuming the superiority or inferiority of anybody. With this viewpoint we shall be able to understand the so-called "primitive" peoples better and be able to get on better with the work at hand. . . .

HELD: From these comments about India and Japan, it seems that the attitude [of distrust of anthropology] that we have in Indonesia is not present. [Since the distrust involves only] a certain field of anthropology, the question of what anthropology is, is again raised. I believe that anthropology should be a science which always takes

into consideration personality, values, and group organization. Only as a definite discipline [with these ends] can anthropology be introduced into countries like Indonesia, where most scientific work is done by Europeans.

In Indonesia we have had some experience in relation to customary or adat law. As a discipline it was the creation of a noble man, but he did not take into consideration the three aspects of culture—group, personality, and values. He had, of course, to introduce some values, and he introduced the value of personal freedom as a criterion in adat law, and that led him so far that he left out of the picture the Indonesian kingdoms and political structures. The Indonesian people are afraid of this [sort of thing], of the anthropologist introducing his value concepts without taking into consideration the group, the values, and the personality of the particular culture. . . .

SANGVICHIEN: Anthropology has no firm footing in Thailand. We have some ethnographic studies of primitive people, especially of the hill types, by people in the Thailand Research Society. We have had some measurements of Siamese soldiers by Professor Edgar D. Congdon and his pupils, but the material so far has not been published, and now Professor Congdon is dead. . . . Work in historic archeology has been satisfactory, but prehistoric study has lagged. . . . My government has been engrossed in other problems, and I do not think it could support this kind of work. Support of any kind has to come from the United States. No systematic work has been done on the physical characteristics of the Thai people and those people in the northern and southern part of my country. . . . Sociology is nearly unknown in my country. There are some courses, including linguistics, given at Chulalongkorn University, but they are of little significance. Dr. Andrews' work on the economic condition of the farmers was not continued and tested for its accuracy and value. The work of Dr. Sharp, so far as I know, has not yet been published. Work of this kind ought to be done by the natives themselves, or supervised only by well-trained foreigners.

Thailand needs fellowships for training abroad. When a foreign student comes to be trained in another country, it is difficult for him to get oriented . . . and what I, for one, learned from a university here was from the lectures and the laboratory work assigned to me, which gave me no idea of American culture. This results in loss of profit to all. Students want to know the country, they want to know American life, but they see only the poor side of it. If there are any Thai students who come here, take more time with them. Give them an idea of what is going on in this country and, if possible, take them into the family to study the family life of the American people so that they can know exactly the culture of the American people.

SENYÜREK: In Turkey, anthropology is represented at the university level by a department of anthropology in the faculty of languages, history, and geography in the University of Ankara. . . . Of necessity, most of our work is confined to Turkey, but the department is interested in other countries as well.

To complete requirements, a student must take a certificate in each of the fields of physical anthropology, human paleontology and prehistory, and ethnology, plus a certificate from the department of geography, history, archeology, anatomy, or geology.[3] There is

[3] Dr. Senyürek's discussion of university organization and of laboratories and other anthropological institutions in Turkey is duplicated, in essence, in the *International Directory of Anthropological Institutions* (see n. 2 above). This information is, accordingly, omitted here.

close co-operation between the various phases of the discipline in the university.

Most of the work done is in the field of physical anthropology and prehistory, and work on ethnology is just beginning. This is just a question of personalities. We are naturally in close contact with other disciplines in the faculty of letters, such as archeology, since some of our work is naturally oriented toward the solution of problems in archeology and history.

An example of this scholarly interchange is in work on the prehistoric population of Turkey. The archeologists were interested in finding out whether the people of one period continue from a former period or whether new peoples came in. . . . The prehistoric Copper Age population of Turkey, the Hittites, is, on the whole, a continuation of the Chalcolithic people, who were longheaded. But one site in Anatolia reveals Copper Age peoples with a high percentage of roundheaded elements. It was suggested, on an anthropological basis, that this roundheaded population of Anatolia was probably intrusive and represented the ruling class. Archeologists, when studying the burial customs of the Anatolian Chalcolithic population, found the graves in one site to be box graves, while in other places they were earth and jar burials. Thus this line of evidence also supported the idea that the population was intrusive. . . .

[An example in which philology as well as archeology combines with anthropology is illustrated in my recent article on the fluctuation of the cranial index in Anatolia from 400 B.C. to 400 B.C.]

We hope that it will some day be possible to apply anthropology in Turkey; this has been done so far only on a preliminary basis. . . .

IVANIČEK (statement revised after session): In Yugoslavia "anthropology" is understood to mean physical anthropology. The branches of cultural anthropology, such as ethnology, ethnography, sociology, linguistics, prehistory, archeology, etc., are considered distinct. A similar situation is found in all European states except Great Britain, where conditions are very similar to those in the United States.

. . . In Yugoslavia, the subdivisions of cultural anthropology are well advanced, but physical anthropology is barely beginning. This is explained by the fact that Yugoslavia, before the second World War, had no institutions emphasizing physical anthropology. This is in contrast to the fact that branches of cultural anthropology were taught in all universities, and there are, in addition, other institutions for the advancement of cultural anthropology. Some of these institutes are under the national academies of sciences and arts in Zagreb, Belgrade, and Ljubljana and the national museums in each of the federal units of Yugoslavia. Some of these institutions include papers on different aspects of cultural anthropology in their regular publication series.

The first work in physical anthropology in the countries now comprising Yugoslavia was done by the Croatian paleo-anthropologist, Dragutin Gorjanovič-Kramberger, who discovered Krapina Paleolithic man fifty years ago. But the first attempt at organizing an anthropological society was not until 1926, when a physical-anthropological section was established within the sociological society in Zagreb. This anthropological section survived only six years, because the sociological society in Zagreb was closed by the government along with other Croatian national societies of that time. After the second World War, conditions became better for advancing physical anthropology. In 1943 the first steps toward the foundation of the anthropological institute in Zagreb were taken. The

government of the National Republic of Croatia extended all assistance, so that there was founded the first anthropological institute in Yugoslavia under the medical faculty at the University of Zagreb. There was later founded an anthropological institute by the government of the National Republic of Slovenia, under the faculty for natural sciences and mathematics at the University in Ljubljana. In the near future it is planned to organize an anthropological institute in the National Republic of Serbia at the University of Belgrade.

Thus we now have in Yugoslavia two anthropological institutes. The first publications of these institutes promise further development of physical anthropology in Yugoslavia.

DIAS: I do not think Portuguese anthropology has contributed very much toward the advancement of science, with the exception of physical anthropology and some archeology. Cultural anthropology does not exist at all in Portugal, although under the name of "ethnology" we are trying to do what you do under the name of "cultural anthropology." As the Portuguese were the first European people that had much contact with many peoples of the world, we have very interesting materials in Portugal that can contribute to comparative studies of modern ethnology from other parts of the world. We have good descriptions of the ways in which people lived at the time of exploration, many of which contributed toward the development of European interest in the life of other peoples in the world.

We are carrying out community studies in Portugal and Spain, studying the people in the same way as we do primitive peoples; i.e., we study a community without prejudice, utilizing all aspects and approaches that we have in our science. This idea originated out of helping the Brazilians, who were very keen to know about Portuguese culture, and also because we feel we are primitive enough to be studied by anthropologists. I think that if the primitive would come and study us, we could learn something more.

LINTON: Most of those present are already familiar with the status of anthropology in the United States. We have all been astonished by the number of institutions now teaching anthropology and by the variety of individuals teaching it. All that seems to be required is a good text and perhaps a little instruction in teachers colleges. However, this is a healthy sign of the developing interest in the science. Another healthy sign is the gradual coalescence of various schools of anthropological study existent in recent years. It is characteristic of any science that during its early period you have distinct points of view which are held with almost religious enthusiasm. I can remember when there were in the United States the diffusionists and the confusionists. The former sought the origins of all cultural phenomena in borrowing from somewhere else. This school was progressively weakened as we came to know more and more about the out-of-the-way parts of the world, and consequently did not have so much room in which to place potential sources. The other school held the doctrine that wherever you saw some sort of order emerging from the welter of ethnological and archeological data, you must show that there has been a mistake. Both these attitudes have largely passed. As far as diffusion goes, [the extreme views have passed, but] interest in historical construction is still vigorous and quite effective. Even the functionalists appreciate that in the case of cultural phenomena you have something of a reversal of that which you get in biological evolution, that is, that frequently it is not function. In other words, a thing is borrowed and

then integrated into the culture along the lines of the potentialities which its forms and its initial associations suggest.

The confusionist idea that laws were something immutable, the sort of ultimate truth which should be unveiled by proper research, has, I believe, gone the way of a number of other philosophical concepts. We now feel sure that in anthropology, as in many other sciences, it is possible to make generalizations with regard to dynamic processes which will at least give a high probability. If you can consistently do 10 per cent better than chance, you can make an excellent living on the races. Similarly in anthropology, with respect to process, predictions which are definitely worth having are possible in a number of situations.

There is an order emerging, and at the present time in America a common corpus of theory is now ready to be directed at more and more problems. Also, American anthropologists are rapidly finding themselves out of a job, as far as "primitive" peoples go. I am reminded of a comment by one of my friends at the University of New Mexico who had a student who was writing his Ph.D. on one of the ceremonies of one of the Rio Grande Pueblos. A point came up in the seminar, and the student said, "I'm not sure about this, but when I go home at Thanksgiving, I'll ask Aunt Mary, and she can clear it up for me." He was a Tewa Indian who was taking his Ph.D. in anthropology.

However, we are also beginning to realize that the same techniques and theoretical framework which can be applied successfully to primitives can also be applied to all sorts of other situations. Apart from applied anthropology, we can find, even among the various communities of old Americans, very marked differences which provide us with something of an experimental laboratory. We also have the opportunity to study culture change in process all over the world, where you have, for the first time under observation, contacts not only between various non-European and European groups but also between different non-European groups—where you can see how various techniques of social control have assisted acculturation and so on, based on initial premises different from our own.

ANTHROPOLOGY IN HISTORIC PERSPECTIVE

LÉVI-STRAUSS: I shall try to state what anthropology means to at least some of us in European countries.

Over and over again during our meetings, it has come out that anthropology is a two-sided affair, and this is extremely important. How did anthropology come into being? It has made itself out of all kinds of refuse and leftovers from other fields. . . . At the end of the Middle Ages whatever was left out of the classical fields was food for the anthropologists, and thus observations from India, China, Africa, etc., were anthropological data. Some time later it was understood that such matters as Chinese thought or Indian thought could be organized into an autonomous field, and what remained, that is, Africa, South America, Oceania, was left as the field for anthropology. Even in the names of our schools it is possible to trace this evolution. In the very near future the field of African study may be considered worth organizing into an autonomous field and taken out of anthropology. . . .

Trying to do whatever we can with the "garbage" of other disciplines is still the task of anthropology, and it is extremely important that all of us should be trained as ragpickers. This is the main and first thought of our work and should never be forgotten. Although this garbage is not worthy to be consumed by other disciplines, anthropologists have always been con-

vinced that the most important clues to the understanding of human nature were located precisely there, and the reason for this has not always been made clear. It is precisely because these things are left over that they are extremely important. Why? Because they belong to cultures or things which are extremely remote from what we are used to dealing with and are extremely diversified and offer quite different aspects. It may happen that things which do not fit and which, for that reason, are not preserved by disciplines which are extremely eager to systematize their data may fit with one another much better than they could fit with what they were separated from. . . .

In order to understand the results of this, we have to go back in history. When the first step toward a closer integration of cultural data was taken, e.g., the integration of Greek and Roman thought through Arabian thought into the modern mind, this was for the first time a truly anthropological approach. It was an attempt to see human conditions through different points of view, borrowed from different societies.

This remained limited both because the societies called on for this co-operative work were very few—those belonging to the Mediterranean classical world—and because what was considered representative of any given culture was the literary and scientific work of some very refined and distinguished minds. Anthropology was limited in both the geographical sense and the human sense.

What we are doing now and what we may expect from anthropology in the future is truly a new renaissance, a new humanistic revolution, which will at once extend itself to all the cultures of the world and also consider everything cultural as representative of the culture. That is a democratic humanism in contradistinction to the aristocratic humanism of the Renaissance period.

Finally, I expect that another change will take place at the same time. Thinkers of the classical period, say Roman or Greek, considered the products of human culture exclusively on an aesthetic level. A poet was a great poet, and that was all. The first anthropological revolution did not destroy this aesthetic approach but added the historical approach. To understand what a work of art was, it was necessary to see not only why it was beautiful but how it came into being and its relationship with other works of art and previous stages of art development. The second anthropological revolution, the one in which we are taking our modest share, adds a third approach, which will be the scientific approach: Not only are we concerned with aesthetic value and historical processes, but we are also in a position to perceive correlations, some general principles, which really have scientific value. It is like somebody in a very limited spot of the earth perceiving only the beauty of the sunset but, when able to travel a little, getting some geological understanding of the landscape and, if able to travel in an interplanetary way, understanding that the earth is just a planet like other planets and that there are laws of gravitation which are holding them all together.

At the end of the eighteenth century, Kant considered that the greatest human achievement of his time was to have reduced the human mystery of the universe to two terms: the "starry sky" above our heads and the "moral law" inside us, and he was referring to the Newtonian revolution, on the one side, and to his own work, on the other.

The second anthropological revolution will perhaps put us in a position to overcome this duality and to understand, according to the same principles,

the "starry sky" and the "moral law." Linguists have already told us that inside our mind there are phonemes and morphemes revolving, one around the other, in more or less the same way as the planets do around the solar system; and it is in the expectation that this unification may take place that I feel anthropology may really have a meaningful and very important function not only in the development of modern socicty but also in the development of science at large.

MEAD: First I take up Dr. Linton's point that when a science gets mature it does not have "schools." I think we have to recognize that in any growing science there will always be violent controversy and schools. There will be new areas within which anthropology will reach out for new forms of integration, new insights; and there will be controversies of a severe order. These will not prove that we are not a science.

Second, I suggest that we keep clear that we are a science that uses a great many levels of analysis. Sociologists assume that all the psychology they need will be provided by others called psychologists and that the psychologists will assume that all the biology they need will be produced by biologists [and so on] . . . and that they will never deal in any way directly with the material—an approach that assumes that each science will keep the other in a box and not deal with it. On the other hand, we have historically been a science that has been willing to use all levels for the understanding of man. Anthropologists have been exceedingly successful in interdisciplinary research because they were willing to work through different levels; and very often one finds violent battles between an anthropologist and a sociologist in an interdisciplinary context for this reason.

Third, I am reminded by Professor Lévi-Strauss's use of the term "garbage" that [we must avoid value-laden terms] like "native," "vernacular," and "primitive," [which antagonize non-Europeans]. We have to watch at every turn that we do not compromise our capacity to work in the international sense in which each people makes a contribution to the science of anthropology, and recognize that only after each major culture area of the world has made its contribution can we have man commenting on man in terms of man's whole history of thought.

As for the relations between anthropology and the humanities, on the one hand, and science, on the other, I think that what has kept us from contact with the humanists has been not only that the humanists were interested in the aesthetically beautiful but that we were busy pointing out that people had to eat, have shelter, and clothes. We have often given a disproportionate emphasis to what our material can show about man's potentialities and have made small attempts to give a biological basis to the higher aspects. We have contributed most to a statement of limits below which man cannot fall and be man. Now we have the task of giving some material on human aspirations, potentialities, and on the kind of culture that we might create for the world—a set of data for stating man's possibilities as well as his limitations.

LÉVI-STRAUSS: I meant by "garbage" that, at each stage in the development of scientific thought, science met with things which did not fit, sometimes because they belonged to cultures remote from our own, sometimes because they were thought to be too insignificant or meaningless to be taken into account. What is in the garbage can consists not only of things referring to people quite different from us but also of a great many other things. The garbage metaphor fits perfectly well in reference to the kind of anthropological work we are doing in our own society. We are picking up odds and ends and finding

out that they are much more important for the understanding of our own society than what was thought of as being really important. But this has nothing to do with the distinction of culture.

FORDE: . . . If, from the odds-and-ends field, assuming it to be finite, a continuous series of newly organized fields have emerged which have been taken away from anthropology, then there would be, from year to year, fewer and fewer odds and ends, and anthropology would disappear. Since that is not the case, there must be something wrong with the analysis.

In the nineteenth century, far from being something which dealt with odds and ends, the anthropological outlook grew up in the biological field; and one has only to think of the work of the nineteenth-century comparative anatomists and of the whole development of thought with regard to human paleontology to see that it was not odds and ends outside a field but a bridge between traditional morphology and the evolutionary thought which Darwin started. The same is true to a very large extent in the cultural field; both Tylor and Spencer were fertilizing established areas of knowledge. I would also say that the classical studies in Oxford have been more and more informed by anthropological thought. . . . One has only to mention figures like Jane Harrison or call attention to the degree to which Minoan archeology is integrated with classical studies—as well as contributing to the culture history of man—to feel that the odds-and-ends metaphor only deals with a tiny fraction of the relation of anthropological data to the broad field of scholarship. . . .

LOWIE: The objections, which are sometimes voiced against the collaboration of the several sciences that we in America usually embrace under the head of anthropology, are of no particular cogency when we consider that we start with specific problems . . . and our function is to solve these problems with the aid not only of other divisions of what is known as anthropology but also of all science that is available.

GREENBERG: I want to comment on Margaret Mead's statement that with the end of schools there will perhaps be the end of controversy. I trust there will not be an end to controversy, but I greet with considerable joy the end of schools.

A school in anthropology is a number of people who hold a set of dogma, not just with regard to one item of controversy, but a number. If you belong as a full-fledged member, you believe that these sets of beliefs coherently hang together; but, of course, they do not always do that. The quasi-religious nature of this group was alluded to by Professor Linton. Progress in anthropology or any science is simply progress from group combat to individual combat; the individual has an eclectic series of points of view and may fight about this or that particular issue. As long as there is a science, there will be fights about individual points.

HALLOWELL: I was very glad to hear Dr. Mead use the phrase "human potentialities." I think this is directly related to our data in an empirical form and to the kind of discussion we had about values. If you look at matters in terms of human potentialities, it is probable that in no one culture can full human potentialities be realized.

There may be differences at different levels with reference to the realization of human potentialities. This, it seems to me, is directly related to the kind of thing that Professor Northrop discussed. It also seems that we can say that, in terms of the realization of human potentialities, biological survival is not enough, nor is mere persistence of a society as such. Here is where what I referred to as an optimum level of human development comes in.

NADEL: I want to refer briefly to a phrase which Professor Lévi-Strauss used in his recent remarks, when he spoke about things that "did not fit." Certain things just do not fit when approached from a particular point of view or in a particular conceptual framework, and one looks around for different conceptual frameworks to make them fit. This discovery that things do not fit is not entirely a discovery of the anthropologist. Historically, it started in all the other social sciences—political science, law and so forth. . . . A tradition has grown up in other disciplines that anthropology is a very good information service for the sort of things they want in order to complete and round out their own systems. . . . They are rather upset when the anthropologist provides not only information but also some theory and some statements which are intended to correct their frame of reference.

There is a danger in that piratical attitude where we claim to know and to be able to tell the other sciences where they go wrong, where their gaps are; namely, that if we are going to tell other sciences where they go wrong, we ought to know something about the other sciences. We ought to know at least a few of the trades for which we are going to be jacks of all trades, and, as regards that, there are certain technical limitations. . . .

LINTON: By the time we came on the scene, as a distinct science, a large number of what were regarded as the richer claims had already been staked. We had to take up the interstitial areas, and this is one reason why anthropology has spread so broadly in all directions and also one of the reasons we are able to contribute to other sciences. We have worked somewhat beyond the borders which they initially set for themselves and to which they are now beginning to realize their problems carry over.

TAX: Although we range from biologists to linguists and we pick different pieces of the whole, and although some of us, very analytically inclined, limit ourselves to small problems, yet all of us are anthropologists united by a common feeling that we must know man biologically, culturally, socially, from all points of view and in all time and in all space. A common value which restricts our wish or ability to be analytic is that we should not draw limits on what we must take into account in order to understand a particular problem. This view of anthropology is aptly summarized in a limerick which Kenneth Boulding, the economist, produced recently at a meeting where it was described:

> The whole vast perspective of man
> Is what anthropologists scan,
> So the net that they take
> Is as big as the lake;
> Let the fish get away if they can.

ROWE: . . . Is there not some anachronism in the suggestions that have been made on this floor? Looking back at nineteenth-century writings in anthropology, you will find that the field was strongly integrated by the concept of evolution at a time when no strong distinction was made by many writers between biological and cultural evolution. The fields which were brought together under anthropology, in this country particularly, were ones which contributed something to the history of man as due to evolutionary theory. When evolutionary theory lost its firm grip, the field began to look like a grab bag because there was no longer a strong integrating principle. Some have been fumbling somewhat since, trying to think up a new integrating principle. Others have been satisfied simply to let it develop along the grab-bag lines.

CLARK: In archeology we look, on the one hand, to natural sciences and, on the other, to the historical disci-

plines. In the earlier days of archeology, some of us tried to extend history backward. Others were working out from geology and biology. Archeology was very much in the position of a receiver, contributing very little to start with. In the last hundred years, particularly during the last twenty-five or thirty years, the position has changed considerably. We still seek a great deal, but we are in a position to give, and it is interesting to notice the growing interest from outside the field. Our historians used to think that society began in 1066. They are now saying that the society which is noted in the Domesday Book is almost a society in disintegration. They cannot understand it from clearly historical sources, so they want to go digging. One has almost to restrain them.

Pollen analysis illustrates this rather well. Every little kink in the diagrams that these people produced was supposed to indicate some great natural change in the environment, to which early man must have bowed in some way or another. Now students have taken account of the findings of prehistoric archeology and say that these things in relation to one another are, in fact, a function of human economic activities.

EGGAN: I would just like to add a footnote to the grab-bag theory of the origin of the science of leftovers. It seems to me, in historical perspective, these leftovers have assumed a central position. I was recently at a meeting of a group of humanists who attributed the low state of their discipline mainly to the fact that the anthropologists had stolen all their most important problems, and they decided that the only way to renovate themselves was to get the problems back again and to begin working on them themselves.

MURDOCK: . . . I remember that in my earliest days as an anthropologist it was considered perfectly proper and admirable to follow any piece of garbage wherever it was carried. But to examine the elements of garbage assembled in one can, or to compare what was found in various cans, was inappropriate. I found myself, then, supporting the functionalists very strongly as to the legitimacy of looking at the garbage in one can. The functionalists became quite well established and integrated with the historical approach in the United States, and new interests in psychology, represented by people as diverse as Margaret Mead, Ralph Linton, and others, began to become interesting to some. There was a controversy as to the legitimacy of this, so I found myself in the position of defending it. When the social anthropologists seek to exclude various other kinds of interest and to say, "We must study one garbage can at a time only," I find myself reversing my position; hence I am becoming increasingly historical as the functional position gets more and more adherents and as I see younger graduate students so interested in culture and personality that they insist on doing only Rorschachs and T.A.T.'s when they go into the field, to the exclusion of life-histories and the ethnographic work of the life-line. I find myself again supporting traditional anthropology. I feel that the kind of experience we have had in the United States, where these various approaches have been laboriously and after struggle brought together and are considered by most of us legitimate and valuable, is, for countries where anthropology is younger, an indication of a series of mistakes which we have overcome and which might be a model of what to avoid.

MÉTRAUX: In the United Nations we have modestly tried to build up an international society. There is a great deal about anthropology in many of our programs, but this recognition of the importance of anthropology is mostly in the form of lip service. We

have heard that anthropology is a science that studies culture and is able to interpret one culture to another culture. It is felt that anthropological surveys should precede any developmental projects or that anthropologists should be sent to evaluate the results. But, in practice, very little attention is paid to the recommendations of the anthropologists. Nevertheless, I think that the fact that anthropology is recognized internationally is important, because it is making people culture-conscious. . . . In Europe we are not really culture-conscious in the anthropological sense. This is important, since the nineteenth-century ideologies are still very strongly prevailing within the frame of the United Nations. There is a strong belief, for instance, that if people would know other people, they would love other people. . . . It is very naïve, and I do not think that the excellent work which has been done, practically, is going to bring any result except to satisfy a few social scientists or anthropologists. Nevertheless, our consciousness of culture is important for the members of these different secretariats, because many proceed on the assumption that Western civilization is something that should be spread all over the world—it is excellent, it is good—and do not pay much attention to differences. . . . Our major task should be to popularize, to make people aware that there is something called culture.

MEAD: There is no real difference in this question of potentials between what Dr. Métraux said and what Père Teilhard said. . . . When we talk about the potential in the world, we are not talking about the potential of preserving technological or public health behavior or the aesthetic interpretations, even, of a tribe in the middle of New Guinea, but are saying that they have, as members of a human culture, a potentiality to share in wider human cultures; that the simple people may not be ready for a world conference yet, but can very soon be ready for a conference on an island—some of them will go into a regional conference; and the aim of the United Nations and especially the specialized agencies is to find ways of bringing them rapidly into this widening world society of which Père Teilhard talks.

SYNTHESIS

NORTHROP: Synthesis is a conceptual problem, and more is involved than getting together people expert in the different sciences bearing on man. More is involved than merely attacking a given problem at different levels, because you never have a science except as you have the facts of its particular mode of analysis expressed in a certain set of concepts; and the question then is, Are the concepts used by different experts commensurable? How are you going to put them together? The matter of synthesizing the findings of different experts is a matter of finding a language for translating from the concepts of one mode of analysis to the concepts of a different mode of analysis. . . . At bottom our problem comes back to the perception problem. Just as different cultures may be thought of as different ways of conceptualizing the raw data of experience, so the different sciences arrive at their particular set of concepts by different methods of abstracting from the raw data of immediacy. We find the way to bring the concepts of the different sciences together into a meaningful relationship is by analyzing out the methods they use as they proceed from raw immediacy to their particular concepts.

MOVIUS: At this meeting, we have only half-filled the equation which you could make if you conceive of the pole of archeology that is allied with the natural sciences as turning to ethnology for enlightenment in the solution of its problems. In the historical fields of ar-

cheology, anthropologists have not tried to proselytize nearly to the limit of the potentialities. If we are going to study culture change, the dynamics involved in the rise of high civilizations, the factors involved in bringing a given civilization to efflorescence and then decline, anthropologists are going to have to overlap in that field as well.

HELD: I see in anthropology two extremes. On one side there is the tendency to integrate into anthropology every science related to man. That is a difficult concept to work with. On the other hand, I see a tendency to consider anthropology as a Weltanschauung. If it is considered as a Weltanschauung, I am afraid this Weltanschauung is indeed restricted to one particular culture and difficult of acceptance by other cultures.

EISELEY: A biologist remarked to me not long ago that he regarded the discovery of culture and the sort of self-examination that it brings into our consciousness as the greatest scientific discovery of our period. In the long run it matters very little whether we are able here to achieve all the kinds of syntheses we would like or to create some kind of nice tied package as to what anthropology is. The real contribution here lies in the kind of illumination that passes among an assemblage of distinguished minds, the kind of new thoughts that they get, the new contacts, the new stimulations which result in a great deal more new work being done. I am sure this is going to result from this conference.

WASHBURN: We probably all agree with Dr. Majumdar that the major aim of anthropology is to understand man. We are also probably agreed that, as Drs. Métraux, Mead, Eiseley, and others have suggested, the most important and distinctive part of this aim is the question of making men everywhere culture-conscious. This is the crucial issue in the world today; and, if we can achieve an understanding at a citizenship level, this will be one of the major understandings at a scientific level.

Although this is the main point of view, there are other aspects to the problem of man understanding man. To understand man, the aims of long-term history and of biological understanding need to be included in the science which many of us enjoy and have fun working in—anthropology.

CHAPTER XX

Concluding Review

By A. L. KROEBER

RESERVATIONS

WHAT I am supposed to attempt today has been listed as a summary; "synthesis" would be too presumptuous a word. Obviously, even a summary will have to be not only partial but also personally tinctured.

I shall begin by mentioning those topics which I think it seems fairest to omit outright and frankly.

First of all, I shall have nothing to say about any of the applications of anthropology. This is not because I think they are unimportant nor because I am adverse to them, but because all through my career I have consistently not participated in practical movements or considerations. I have deliberately kept myself out of them as far as I could. This began as a matter of temperament and finally crystallized into a principle. I decided that as long as normally I was not responsible for action decisions, my role had best be that of a person who is completely uninvolved in active issues or causes, no matter how worthy, and who would therefore be in position, when called on, to give impartial testimony as to fact or the run of prevalent opinion.

The result is that, having never systematically given thought to practical questions, I am therefore really incompetent to speak about them.

In the second place, I shall omit everything in the nature of direct reference to "culture-and-personality." Again this is not because I oppose it but because it is evident to all of you who know my work—as well as having become increasingly evident to myself in the last decade—that my approach is prevalently macroscopic, long range, historical, purely on the cultural level, impersonal, and nonphysiological. This approach, which was congenial to me from the beginning and which I consistently deepened and channeled, no doubt relates to the fact that I came into anthropology through an interest in language. Just as in language studies we do not ordinarily or seriously inject consideration of persons or individual personality, so with my approaches to culture.

There have been some hasty findings made in the field of culture-and-personality, but there can be no objection of principle to the approach. Indeed, I see all cultural phenomena as occurring enveloped in a psychological atmosphere and suffused by psychology. One who refuses to take cognizance of the psychological or personality element in culture misses much of the essence of it. I have not hesitated to live up to that belief. But describing cultural patterns, forms, or relations with a psychological suffusion is a quite different thing from directly and consciously embarking on inquiries into the interrelations of culture with personality, into the personality-and-culture field as such. Having abstained from that, I do

not consider myself its competent judge.

Finally, another idiosyncrasy seems to have made me from the very beginning suspicious of specific causal explanations of cultural phenomena, especially of patterns. I have even at times suggested that causes were impossible to find. That is going too far, I now realize. But I do think causes remain difficult to pin down. Well, with that point of view, it seems only just to disqualify myself from appraising the attempts of others. I shall confine myself, later on, to some remarks on principles of causality in culture.

RELATIONS TO NATURAL SCIENCE

I want to begin this morning's topic by commenting on the place of anthropology in science. This symposium has strengthened my belief—in fact, several statements made have strongly reinforced it—that there exists basically only one kind of fundamental science. All genuine science aims at the comprehension of reality, and it uses both theory and evidence, in combination, to achieve this comprehension.

The model for that, of course, is the already developed natural sciences. But I believe that our modern philosophers, in inquiring how science operates, have made a mistake in the almost exclusive degree in which they fall back on physics as the only model of science. It is easy to see why they do that. But in the case of human behavior we are obviously far away from the phenomena both of mechanics and of atomic structure; and there is an infinity of other phenomena which are as wholly "natural" as the phenomena of physics, but very much nearer to the phenomena which we anthropologists deal with. We ought, in precedent and comparisons, to look primarily to these nearer fields, to biology rather than to outright physics.

Further, I submit that man as a set of phenomena, including his culture in all its aspects—along with values—not only is *in* nature but is *wholly part of* nature. It is evidently going to be somebody's business to deal scientifically with these human phenomena: to work at a more than aesthetic comprehension of them. Such a comprehension would be intellectual, aiming at an intelligible concord with reality, resting both on specific evidence and on broad coherent theory.

RELATIONS TO THE HUMANITIES

In the past, except for largely unsupported attempts by anthropologists, the phenomena of man's culture, his products and values, or at least certain of them, have, by general consent, been left to—the humanities. Now humanists unquestionably operate evidentially. They not only cite evidence; it was they who invented the footnote. They are also empirical in procedure in starting from a given, although this may be mainly what others have written before them and though they are also idealistic in trend. But the humanities, as we were told this morning, also occupy a special position. Their first Renaissance proponents felt that they represented the breaking-away of illumination, of true enlightenment, from medieval darkness. It was indeed a widening of horizons; and, as Lévi-Strauss pointed out, the classics were really rediscovered as something worth while in themselves, as against the narrower provincial view of medieval Europe. As a result of this active, significant rediscovery by the Renaissance of the previous great culture of antiquity, there occurred the exaltation of what was ancient. After all, it is only two hundred and fifty years ago since a few iconoclasts first dared to pronounce the moderns and their books as great as those of the ancients; up to then, under the spell of the Renaissance humanists, it had been taken for granted that the

classical antiquity of Greece and Rome represented something superior.

That, in turn, meant that the humanist, as the representative of antiquity, claimed a position of privilege, a particular and high status; and it began to be accorded to him by the world, even while natural science was celebrating its first great triumphs.

Along with status for their subject matter, humanists claimed for it an exemption: the exemption from nature. What they dealt with was the human spirit: humanity as something outside the laws of nature; and from that there stemmed the long contrasting in German of the *Geisteswissenschaften* with the *Naturwissenschaften.*

That, of course, is a basic hangover from the Middle Ages. *Geist,* like our English equivalent "ghost," first of all means "soul." Only secondarily did it mean "psyche" or "mind," and then "essence" or "spirit" in the wider sense of humanity; just as another extension of it meant and still means "wit." But the claim of the humanists, that their field was exempt from nature's law, came to be accepted and was long honored by the scientists of nature also. Perhaps the scientists themselves had not progressed by 1750 far enough beyond mechanics, mathematics, astronomy, and anatomy to venture on a wider jurisdiction—let alone a universally phenomenal one.

The humanist aesthetic rediscovery and reliving of classical antiquity, and then of their own contemporary arts, inevitably dealt with value judgments. From appreciation they passed on to judgment of values. And from this there followed, as Lévi-Strauss also pointed out, the rediscovery of meaningful history.

What, however, the humanities failed in was systematic conceptualization. It is obviously hard to take a series of aesthetic judgments and weld them together into an intellectually satisfying coherence. That the humanists failed to do. It is probably impossible to do. An adequate intellectual theory system has evidently got to conform to the extended reality of nature also. That humanists did not feel the need of—in fact, claimed exemption from. This perhaps remains their greatest defect today. As scholars, they exact evidence; they do not exact its broadest possible intellectual organization.

SOCIAL SCIENCE

The "social sciences" developed definitely later than the humanities. Adam Smith's *Wealth of Nations* dates from the year in which this country declared its independence—1776. Sociology under Comte goes back to about 1830. Political science as something separate from law and history is still later.

Not only were the social sciences late. From the first they were practical, concerned with the welfare, rather than the values, of humanity. They aimed at applied science from the beginning. They were not really fundamental at all. Whatever theory they developed was really secondary and quite special, or came in on a logico-philosophical level, as with Comte. All the founders of social science had practical, helpful aims. They were utilitarians. Many were ameliorationists. When not themselves reformers, they believed in the utility of their knowledge.

What did they do for theory? Essentially, they developed a dialectic. It may be a theory like Comte's. I must leave it to philosophers to judge how much intrinsic philosophic originality and value Comte's theory has or how far it is tendentious dialectic. The economists developed "economic theory." It is a formidable body of thought; but it seems to me essentially a body of dialectic. While it keeps going back to economic phenomena, it remains a closed system. The phenomena and the theory are a pretty well shut-off little

universe. We may finally ask: What real connection is there between economic theory and the organized biologic theory of today—or psychologic, or cultural, let alone physical, theory? It is hard to see how a theory evolved in such isolation can ever become really allied to fundamental scientific theory. Economic theory seems not only to be unintegrated to general science but not in the way of becoming integrated.

It might also be said about economics and the other social sciences that they are really dealing mainly with phenomena of culture without recognizing culture. They select certain aspects of culture—economic, political, societal, populational—and then deal with them in isolation. That works out well on the practical level, but it obviously does not lead to integration with the rest of the body of science.

In a previous meeting Lévi-Strauss spoke of how the social sciences do not work in depth, but of how the humanities do agree with the natural sciences in operating in depth. The question was asked, "Just what does 'depth' mean in this connection?" I should say that "depth" means intensity of search. In proportion as inquiry is ready to go far and long, to go deep, to take into consideration everything relevant instead of quickly setting off some enclave, we have depth. Depth is a quality that scholars in the humanities have generally sought for to the best of their abilities, as physicists and biologists have also sought for it. But I agree with Lévi-Strauss's challenge of depth in the social sciences. How can there be depth when fundamentals are not seriously inquired into?

COMMUNITY STUDIES

I would also agree with Linton's comment in accepting the verdict of depth or lack of it, but adding, if I understood him correctly, that, in anthropology, depth holds only as far as we deal with the *Gemeinschaft* or face-to-face group, the community of the size and sort that the anthropologist has been used to dealing with.

In the main, that is certainly true. It is perhaps the reason why anthropologists have been so reluctant to abandon their community studies. It is plain that the present-day community study of a more or less isolated group within a larger society, and its particularities of culture, is merely a substitute for the old-fashioned holistic tribal study on which ethnography cut its teeth. Primitive persistences just got to be fewer and fewer and became so remote that where formerly for a few hundred dollars one could study a tribe for a summer, now one has to go off for most of a couple of years and it is a matter of thousands of dollars to do field work—and many native cultures are wholly gone, even as memories. So anthropologists, faced with a shrinking material, as Linton called it this morning, have turned to the modern type of community study, which you can sometimes make by paying a dime and taking the subway somewhere downtown. In these community studies one can still use the traditional face-to-face methods, the familiar personal-relation-with-informants methods, which have been so distinctive of anthropologists as compared with most sociologists and all economists.

But it must be admitted that many community studies have been done somewhat in the blind, with a prevailing hope that somehow something useful and larger would come out of them. Generally, the inquirer becomes eagerly interested in the members of his group and their subsegment of culture. But many such studies have been undertaken, especially by our students, because they sensed that the specific thing that an anthropologist did, and the thing that they wanted training in, was to have some of this experience of

dealing personally with a community and then analyzing and describing it. What meaning came out of it was for their elders to decide, rather than for themselves, who were only on the firing line.

This going it blind was less true of ethnography. A study of the Cheyenne obviously contributed to knowledge of the Plains, and this to an understanding of native American culture, which, in turn, was necessary for the interpretation, whether in sequential or descriptive terms, of human culture as a whole. It is less clear whether or how a thousand studies of East Side Gypsies, Boston Armenians, Amish groups, and settlements of Greek sponge fishermen in Florida will add up to an understanding of even the culture of the United States. Such community groups are somewhat like organs in a large complex body; but tribes are much like organisms, separate and different indeed, but classifiable into historically grounded orders and phyla of life. Systematic descriptive ethnology is obviously a sort of natural history. This quality is less evident in studies of community segments of a civilized nation, which, on the other hand, invite applications, betterments, planning, and controls toward conformity.

THE AIMS OF ANTHROPOLOGY

Well, after these detours you will be surmising my thesis on the humanities. It is that anthropology largely represents an unconscious effort of total natural science to extend itself over the area traditionally held by the humanities. Almost sixty years ago Rickert had already proclaimed that *Geisteswissenschaft* was not really science of the soul or spirituality but was *Kulturwissenschaft,* the understanding of culture as a varied series of phenomena, intelligible by methods somewhat different from those of established natural science. This point of view I believe to have been increasingly vindicated since.

It is in this extension of scientific method to the field and material of the humanities that I see the future of anthropology largely lying—especially that part of our functioning that will transcend present activities. I believe that is where Tylor was tending; where Boas was tending, in spite of his impatience with classification and all his negativistic cautions; and where we are tending. In substantiation: linguistics has long ago shown us the road. Scientific linguistics grew out of philology—in other words, out of a humanity interested in certain endless particulars of letters considered valuable in themselves and recognizing a hierarchy of languages, of forms, and of better and worse values. From this, linguistics gradually disengaged itself as a genuinely pure science and has had its extraordinary development, which no natural scientist who realized what the development had been would ever deny as true science.

I believe that in the same way cultural anthropology will ultimately receive heavy reinforcement from the humanities themselves for transformation into the kind of science it is to be. If not, it will be our responsibility to achieve the transformation ourselves by gradually acquiring the necessary knowledge and methods to take over the fields of the humanities and generalize them. I suspect that, on the whole, it will be a peaceful *rapprochement.* At any rate, the naturalistic point of view must be established in the field of the humanities, and part of the job of anthropology is to help do that.

THE BASIS OF ANTHROPOLOGY

Another suggestion has longer personal associations for me, but I feel doubly sure of it since Lévi-Strauss this morning talked about the new humanities. This proposition is that the central

field of anthropology is culture. I do not mean in any way to belittle physical anthropology. Culture presupposes human organisms, and their interrelation constitutes a basic problem. Nor do I mean to read language out of culture. Speech has relatively high autonomy; but culture could probably not originate without language, and this would be nearly empty of content without culture. I mean merely that our most distinctive field, the one around which we can best organize our data, is culture; and, as has been said several times in our assembly here, anthropology has certainly had a large hand in the recognition of culture. I do not think we can claim to be the original discoverers of it; but we have assiduously cultivated the discovery, exploited it, stayed with it, and begun to utilize it as our first and biggest organizing principle.

And that, if true, seems inevitably to involve a principle that I have long advocated: the doctrine of orders or levels and of culture as a distinctive one. I shall not repeat the arguments; suffice it that one way of putting it is to recognize culture as an emergent level. We can in the same way recognize society as an emergent level distinct from the individual organism, just as life also represents a level emergent from suborganic conditions.

Many years ago there seemed to be little recognition of this principle. Feeling a bit like David before Goliath, I went forth and slung my stone for it. But today, except for verbal differences of emphasis, all anthropologists appear to agree on the distinctiveness of culture and on its serving as a principle around which to organize our thinking. In 1952, it is "reductionism" that has come to be on the defensive.

Recognition of a separate culture level does not mean, of course, complete isolation or total ultimate self-sufficiency. There is certainly need in principle for culture-and-personality as a field of study in which the interactions of phenomena on the psychological and on the cultural level are examined. And similarly for generic basal "human nature," and for environment.

APPROACHES OF ANTHROPOLOGY

In the second place in this listing of basic principles of method in our science I would put a distinction of which I have also long been an advocate—though I tried to acknowledge my debt to Redfield the other day—the distinction between what I used to call the "historical" and the "processual" sciences, and sometimes simply "history" and "science." It is not so much a matter of different sciences as of approaches; and "phenomenological" is a less ambiguous term than "historical." It is this approach which "preserves" its phenomena, tries to integrate them as such, and therefore depends much on the building-up of context. In contrast, when primary interest is in process, phenomena are "resolved" by analytical procedures, and there is more concern with cause.

I retract certain former intransigences on this point. The various sciences can hardly differ ineradicably in their objectives. It is probably much more largely a matter of their stage or sequence of development. Any fully developed science will presumably be completely analytical, will be concerned at all times with process, and will therefore deal with causes, so far as it is able. But, until a science becomes fairly developed, it cannot escape the prior task of observing and recording its phenomena, ordering or classifying them—humble as that may seem as an intellectual exercise—and therewith seeing them whole in integration and context.

In other words, process is what is extricated later. Arrangement, classification, organization of phenomena as

phenomena, can and should be done at an earlier stage. My former attitude was right to the extent of recognizing that anthropological science had not yet got to the point where it could analyze out process and cause as successfully as, for instance, physics. To have thought that, would have been an illusion; and it still would be so.

Process, in short, is harder to isolate and operate with successfully in culture than it is for inorganic matter and energy. It is harder, probably, than for organic phenomena. We have had physiology for a hundred and fifty years, and some important beginnings of it much longer than that, whereas we are still in the beginnings of understanding processes in culture.

It is also well to remember that, while biology began as natural history with Aristotle and again with Linnaeus, and that we now have physiology, biochemistry, and genetics as well-established process sciences, nevertheless the natural history aspect, the classificatory and sequential organization of the phenomena of biology, still remains alive and active. It remains active in paleontology, in evolutionary study, in what Marston Bates renames "ecology." The same no doubt holds for anthropology. The historical or contextual or integrative approach will presumably continue to be pursued; but I expect it will be increasingly superadded to by progress made by the processual and analytical approach.

A third distinction I touch on briefly. It is that subfields vary according to the data. Some kinds of data carry certain advantages in them; others, different ones. For instance, as regards archeological versus ethnographic data, it is plain that the ethnographic data can have a larger total significance because we are dealing with the whole culture, with the living people still in it, whereas in archeology we are dealing with only part of the physical remains of a culture, plus some conditions and relations we can infer from them. Yet, when it comes to such matters as time sequence, ethnographic data are a shaky reed and have often proved a false one, whereas archeological data can yield an objectivity and certainty which the ethnographer can only envy. Both, in other words, are needed to supplement each other.

THE ORGANIZATION OF ANTHROPOLOGY

I was much interested in what was said in our discussions about the organization of anthropology, both the other day and this morning. On the Continent of Europe it is clear that what we take for granted in this country as being the unitary science of anthropology is a series of fragmented sciences. Perhaps even "fragmented" is not quite accurate, since they have never been brought together except superficially by a sort of federal union on top. Their particular roots of interest were quite diverse, though they usually agreed in a certain degree of nationalistic weighting. Prehistory was first gone into as an extension of national history: that certainly was its prime motivation. Ethnology went in for the exotic abroad, but at home for peasant dances, regional customs and costumes, superstitions and folklore of the provinces. Even physical anthropology, though practiced in a laboratory on rows of lined-up skulls and directed at an accounting for all the races of man, tended to get involved with Merovingian and pre-Roman ancestors.

All these activities then had some national roots to a degree which we here in the United States find it hard to imagine, since Europe is unbelievably tradition-bound, as Ackerknecht told us, from the American point of view. Europeans mostly do not fully realize the difference, because, until they have lived here long enough really to enter

actively into American life as participants, they are unaware of how little tradition we have and how little we want to have. We possess only a brief history, and as a people we tend to be proud of not having much. It must sometimes be a shocking discovery for Europeans to make.

It might even be said without undue unfairness that one typical American attitude is explicitly antihistorical. In a scientific sense that attitude is, of course, mere shallowness, since there is little question that the bulk of the culture of any people comes from the past, and to ignore that fact is a shortsighted intellectual limitation. On the other hand, once one considers why this attitude exists, it is easy to see how it originated. Most of the people who came to this country to populate it, early or late, were really wanting to get rid of much of their past. If there was to be a history for them, they were going to start it anew. Once we as a nation had entered that path, we tended to continue on it, since there is no question that those unfettered by tradition, those not bound by their history, can more readily achieve a certain kind of mobilized effectiveness. Intellectually, it may be a misfortune. It would be wise for Americans to respect the people of Europe and Asia for being steeped in their history and bound by tradition, whereas we cannot even muster much of either when we occasionally want to.

On the contrary, when it comes to the assembly line, the producing of things by the million and turning them out accurately, when it comes to developing an economy and converting this at need into military power, there is no question that our nonhistorical attitude, our lack of tradition, can be a great help. We do not first have to make a thousand adjustments and sweep a lot of established things aside.

Obviously, our interest in this symposium is to look upon the difference as an existing fact and not to assume that one attitude is the better. Each approach has its real virtues.

In returning now to the more limited field of anthropology, it is easy to see why anthropology is organized as a general science here, why it is taught in a hundred or more universities, why all of us are in essential agreement as to what constitutes anthropology, whereas in Continental Europe it is still somewhat difficult to bring the several sciences together and to make them really co-operate. That is one reason why congresses such as the one to be held in Vienna this summer or, for that matter, the Americanist Congress in Cambridge are so important in Europe. To us, such a session is, well, a week's Donnybrook Fair. But to Europeans these congresses mean very much more, because ordinarily their disciplines are so separately rooted and so little integrated.

The British hold a somewhat intermediate position. As Anglo-Saxons, they have some of our attitudes; but they are also tradition-bound; and they like it so. The Latin-American organization of anthropology has, directly or indirectly, perhaps been influenced prevalently by American and Mexican example. In Japan, Ishida told us how influence has varied from time to time. India has listened both to Britain and to the United States and—wisely—to herself.

Nevertheless, let me remind you that there is precedent outside the United States for seeing anthropology as a larger whole, as an integral unit. In 1865, Tylor published *Researches into the Early History of Mankind.* Primarily, we would now say, the book dealt with prehistory. In 1871, in *Primitive Culture,* he gave the first formal or explicit definition of culture. The concept was known previously as "civilization" in England and France, as *Kultur*

in Germany. But Tylor first put the word in the title of a book; and he set its definition in the very forefront of his book. A decade later, in 1881, he published another book called *Anthropology;* and in this he tried to encompass substantially the whole of what we here call anthropology today. There is, then, this illustrious precedent from well back in the nineteenth century and from Europe, for seeing anthropology as a larger unit.

SOCIETY AND CULTURE

In comparison with this matter of range, it would seem that the interesting discussion we had as to "social and/or cultural" anthropology is of somewhat more limited importance. I was struck by Ben Paul's statement that, in the United States, social anthropology tends to be construed as an enclave set apart within cultural anthropology, much as we carve the night out as something special within the twenty-four-hour day. Our wider term is certainly "cultural anthropology." When an American speaks of social anthropology, it is because—like, for example, Lloyd Warner—he is focusing on the interrelations of social classes and bringing in cultural criteria principally as a means of defining the social structure. In Britain, of course, the situation is reversed and "social anthropology" is the generic term, either because of Durkheim–Radcliffe-Brown influence or perhaps because British ethnographic studies have been so largely devoted to the living cultures of Africa and Melanesia, where the culture carriers still function as active interagents with meaningful social roles, instead of cultures being essentially vehicles of memory of dead or decaying cultural forms, as among most North American tribes.

I am less sure about Paul's opinion that social anthropology seeks timeless generalizations, whereas cultural anthropology looks for histories in particular times and places. The distinction sounds as if it stemmed from Radcliffe-Brown—though I would be inclined to see a more solid and substantive contribution in Radcliffe-Brown's own formulations of time-and-place-bound patterns, as for Australia, than in his timeless generalizations or laws.

What is basic in the society-culture problem is the fact that social relations constitute a widespread and ancient order of biological phenomena, occurring more or less all through the animal kingdom. In contrast, culture, resting on speech and symbolism, whose origin we do not know, is almost exclusively peculiar to the human species and therefore cannot be more than a few hundred thousand years old at most. In the history of organic life, culture thus is a highly specialized and unique kind of phenomenon, society a broad and recurrent one. This contrast seems to deserve precedence over a dichotomy like that of timeless generalization as against time-and-place-bound history.

Incidentally, there seems no more reason, except for our having got tangled up with names, why there should be any dispute of principle about what are social and what are cultural phenomena. The tangling, of course, is due to the words "social" and "society" coming into general usage long before "cultural" and "culture." So sometimes "social" still means "social" (societal), sometimes "sociocultural," and sometimes "cultural"; and, once there is addiction to particular words, the tendency to stick to them is strong just because they are ambiguous. Still, with only recent men having culture but with societies extending far back through zoölogical phyla, there seems no longer any real excuse for ambiguity.

One reason why ambiguity nevertheless persists in spots is the fact that cul-

ture as a distinctive set of phenomena has proliferated so immensely in its relatively short span of existence. Some of you may have heard, a year or two ago, in this very room, Julian Huxley's lecture in which he argued that man with his culture has come definitely to dominate all subhuman species and that the supremacy of the future is with him as against the rest of the animal kingdom. Culture has become such an enormous aggregate, with all its ramifications, that it overshadows and rather engulfs human society. While it is logically possible to consider human social phenomena as such, there seems to be something arbitrary in the limiting procedure. That is apparently what Murdock meant when he spoke of culture including social structure in man.

That position seems still to be challenged by Radcliffe-Brown—who unfortunately could not be with us—and who sees society as of primary importance and culture as somehow secondary or derivative. That is also in essence the position of Talcott Parsons the sociologist.

The question is what is meant by "primacy." Is society more significant? Or is it more real, culture being something intangible and elusive and hard to define? I do not believe that Radcliffe-Brown and Parsons base their claim on the fact that societies existed on this earth long before cultures. While that is true, it does not seem to be what they rest their autonomous primacy of social relations on.

Most American anthropologists and probably most European ones would agree with Murdock that it is wholly legitimate and often profitable to see culture as including human social structure. On the contrary, it seems very difficult to define society at once in a strict sense with specifically social meaning, and also so broadly as to include culture. The Radcliffe-Brown and Parsons position appears to come down to this: that society is basic and culture is somehow a kind of extension of it. But just in what sense culture, with all its vast specializations, can be a mere extension or secondary derivation from society as such has never been made really clear. In this point sociologists like MacIver and Sorokin are much nearer the prevalent anthropological position.

These are points we cannot settle here, but they are matters that will have to be settled.

ETHNOLOGY AND CULTURE HISTORY

Now, how about ethnology? I am about ready to abandon this baby to the wolves—to a premature fate or to a senescent death as one may see it. However, I shall not do so without making some new claims as to its succession. After all, I am myself more ethnologist than anything else.

Ethnology appears to be vanishing for a variety of reasons. One is the increasing difficulty of finding unspoiled primitives and the increasing ease of making community studies on non-primitives, as already discussed. Another is that much of ethnology, both of primitive and of folk cultures, has remained a cataloguing of items. And too many ethnologists, falling in love with their particular people and chosen culture, have a way of becoming absorbed in its concrete detail and ending up by hating to turn away to view its larger relations. Often they will not even make comparisons. An extreme example was Malinowski. He understood and loved to portray Trobriand culture, and he was skilful at it; but near comparisons tended to intellectualize and blur the apperceptive focus of his picture; so he kept adding finishing touches until he was ready to institute a telling contrast with our own ways, or to let loose a common-knowledge hunch generalization. When Malinowski did really compare, as in

the sketches of other New Guinea tribes, in the introduction to *The Argonauts,* he showed the same skill as in his Trobriand work. But he, and many others, built up systematically comparative pictures only too rarely.

Another way of putting it is that the larger interpretations of ethnographic data have long tended to skip the laborious task of step-by-step analytic comparison and classification and to leap directly into speculation. The speculation sometimes concerned evolutionary stages and sometimes Protean, ill-definable concepts like magic, animism, sacrifice, and the like. Bachhofen, Maine, Morgan, and McLennan marked the first generation of this speculative burst; Frazer and Hartland the second.

That the earlier interpretations should have been speculative was almost inevitable, since the data came from travelers and missionaries and were more often miscellaneous than holistic. Moreover, ethnographic data are interpretable chiefly by speculation until they have been subjected to an empirical classification with consideration of which cultures are more similar and related to one another and which less so.

Biologists long ago began the classification of animals and plants. The earlier classifications were inclined to be either artificial or external. One counted the number of stamens, pistils, and petals and made cross-secting classes, or divided into crawling, swimming, running, or sessile groups of animals. But gradually these crude classifications were replaced by what we call the "natural" classification, which aimed to take cognizance of all structural features of the organisms being compared—of their total pattern structure. And that classification, when evolution suddenly and, as it were, overnight was accepted and a time element was thereby injected, of course proved to be remarkably sound. Without knowing it, the old-fashioned "taxonomists," as they are sometimes called in a sort of derision, had really worked out in their classification much of the essential history of life; so that, when the doctrine of evolution hove into consciousness, it prevailed so rapidly and decisively precisely because the preliminary work of classification in biology had been done so well: it fitted and substantiated the new theory remarkably in the main.

We have not done anything corresponding with our ethnological data. A very gallant attempt was made by Father Schmidt and his associates. But I think it was preliminary—too pre-Linnaean, too much concerned with enumeration of items, too little with types or basic patterns as they occurred. It was therefore cheering to hear Father Koppers say that the *Kulturkreis* scheme was in need of drastic revision. Indeed, all ethnographic classifications need revision and constant revision. Every sound "natural" classification of culture is also inevitably a sound genetic history of culture, as in biology it yields a history of life. We even possess in archeology a factual partial check on our inferences from cultural pattern to culture history—a sort of life-line—a counterpart of the substantiation which paleontology gives to classificatory and evolutionary biology. With that, ethnology is not so much jettisoned as merged right into culture history.

Then, when we once have enough sound classification and history of cultures, we should be able to take the next step and, with some genuine solidity, to extricate the processes at work, to generalize the story of culture into its causal factors.

This is not the only way to define processes. They can also be derived, of course, in the "microscopic" way, by bringing personalities into considera-

tion, by close-up psychological dissection of phenomena under watch and perhaps even under control. This will be the counterpart and balance of other generalizations that can be inferred only from long-range mastery of historical material, much as the paleontologist and ecologist get certain results and physiologists and geneticists supplementary or corroborative ones.

The difficulty of moving precipitately into process is shown by Boas, whom I used to think hostile toward anything like historical interpretation, though Ackerknecht is probably right in considering it more of a "bashfulness." I suspect Boas rather looked down on anything classificatory. You do not outrightly classify phenomena in physics very much; so "real" science seems to begin only after classification. In time, Boas proclaimed that he stood for dynamic interpretation, which means that the important thing to seek is process. I agree that it is, when one can isolate it. However, Boas' work fell in a period when, though he was able to show the existence of a multiplicity of processes in culture, he was not yet able—it was impossible in his day and perhaps still is—to formulate these into a systematic theory.

LANGUAGE

Linguistics is regarded as the most "scientific" of all social and humanistic studies. It is rigorous, it is precise, it defines its terms. Probably it owes these virtues to its material, which constitutes one of the most nearly self-sufficient and autonomous fields in nature.

Linguistics, which is comparative and general, grew out of the analysis of particular languages studied as ends worth while in themselves. Linguistics is a genuine natural science dealing with intangible phenomena. That its development was out of a humanity whose sanction was religious, patriotic, aesthetic, socially polite, or otherwise culture-bound augurs well for the prospects of an analogous development of the study of culture.

One of the relations of linguistics with the rest of culture is in ethnolinguistics, the sort of problems that Greenberg talked about, the matter of closer and remoter historic classification of languages. To the linguist himself it usually matters very little whether or not two languages are related: it is a mere fact of detail which does not seriously touch any principle or, usually, relation of form. If we succeeded in adding a thirteenth branch to Indo-European, it would slightly complicate the job of Indo-Europeanists and would necessitate revision of many of their particular findings; but presumably it would not be at all a revolutionizing discovery linguistically.

However, to culture-historical anthropologists such discoveries can be all-important. For instance, let us assume the discovery anywhere in America of an indubitably Indo-European language accepted as such by all Indo-Europeanists, spoken by natives at the time of discovery, and of a degree of divergence indicating that it had branched off from the stock four or five thousand years ago. Imagine the flood of interpretations this would open to our thinking about the history of native America. The immigrant people must have brought along a culture more or less like that which other Indo-Europeans enjoyed in Eurasia at the time. Did that leave any traces? If not, why was it entirely wiped out but the language preserved? If the culture was not altogether lost, is it this surviving element or that which has often puzzled us, that is due to this very Indo-European migration? Just take this as an imaginary example. The discovery would be of the utmost importance to us for its involvements and implications. That is why, whether we are

ethnologists or archeologists, we are constantly looking toward and trying to use these linguistic classifications of relationship.

Now, if the linguists are working on higher problems and do not want to be bothered with this rather elementary sort of classification, that is fair enough. But let them then at least occasionally allow their assistants to assemble the data for a competent classification, or aid an ethnologist with basic linguistic grounding to go ahead with it, since it is so important to students of culture. Really, this last would perhaps be the most practical procedure, as long as linguists with a professional reputation to lose are likely to insist on using their precision tools for what is actually more nearly a pick-and-shovel job, if they undertake it at all.

Let me remind you of the tremendous utility and considerable influence on American Indian sociocultural studies which Powell's classification of 1891 has had. Not that it was a very good classification or went nearly far enough. It was made for Powell, who was a geologist and an army major, by Henshaw the ornithologist when Powell found that he would never get his philologist-linguists like Gatschet, Hewitt, and Pilling to come through with the commitment of a classification.

As to meta-linguistics—the Whorf sort of Weltanschauung premises—I agree with Lounsbury that some cultural anthropologists show a strong "emotional dedication" to this point of view. It certainly seems to promise us all sorts of deeper understandings about behavior and culture. But I also agree with Lounsbury that the evidence or illustrations so far cited, in spite of what is often assumed, relate essentially not to grammar or linguistic structure but to semantics, the meanings of words.

It is clear that, in the degree that linguists grew away from philology, they progressively lost interest in everything lexical. Most of them still intend to supplement their grammars with dictionaries, but few of them ever get around to it. The emphasis is increasingly on form; since Bloomfield, the *avant-garde* attitude is that meaning is an impurity, a little of which, unfortunately, has to be admitted. Since the culture "and/or" language controversy has come up, there is even some overt inclination to say that meaning *is* culture and to hand it over to cultural anthropologists or whoever will have it. At any rate, so far as Whorf and Lee are sound, their findings relate to a borderline field, the transitional, "more nearly cultural" part of language.

These alleged correlations between patterns of culture and speech seem in some ways like the pattern correlations of Spengler. They come out of perceptive intuition, which Whorf undoubtedly possessed to a high degree, as did Spengler. Whorf's intuitive faculty did not prevent his being an extraordinarily able linguist; just as Spengler could probably have been a top-flight historian if he had condescended. Both men obviously had brilliant insights, which are not lightly to be dismissed but which are difficult to prove; and, so far as they may prove true, they are surely important.

Take, for instance, Spengler's finding that there is an inherent connection, a similarity of deeper, covert pattern, between occidental counterpoint in music, double-entry bookkeeping, calculus as a form of mathematics, and the complicated system of stresses and balances in a Gothic cathedral. You can enunciate this correlation or subsuming of patterns with a slightly cynical or derisory tone, and the idea sounds like poppycock. But again, you can say it with a dedicated and reverent air, and it is extremely impressive. Honestly, I do not know and would

hate to have to decide positively about the reality of this correlation so fervently proclaimed by Spengler. It may be true or just a fabric woven out of words. The similarities may be merely analogical, with no actual historical connection. The master-pattern may have existed only in Spengler's mind. At any rate, it impressed him, and it has impressed some others since. But the interpretation has not been proved; and I think the same holds for Whorf's meta-linguistics. Hoijer, who evidently leans favorably to it, virtually admits the lack of proof.

CAUSES

Some remarks on cause. I began the study of culture with a predisposition—perhaps an aesthetically grounded one—to be skeptical of causal explanations. To some, such explanations seem easy. With an idea about the cause of a situation, one can almost always find some evidence in support. But, really, causes are quite difficult to determine with certainty. For one thing, there are so many of them, and they are so interactive. However, a few general principles as regards cause in anthropology seem fairly clear by now.

One principle is that immanent causes as such must be excluded. I suspect that they will all prove to be crypto-teleological. Just as Marston Bates, Simpson, and others have made clear that biological orthogenesis is usually some sort of teleology in disguise, or at least due to a hankering after something that resembles teleology, so I believe immanent causes will have to be generally discarded in our explanations of the phenomena of culture. I would make only one exception, in certain fairly short-range phenomena, where it will probably turn out that a factor of "momentum" is at work. That is, when a cultural movement starts in one direction, it is likely to acquire enough momentum, especially if values attach to it, so that it does not suddenly turn over or get reversed or go off in a perfectly random direction. But that need not mean that there is any long-range immanence, a larger inherent design or objective toward which the movement must be tending. The short-range phenomena of which I am speaking are especially those within styles. Styles possess a beginning; then the possibilities of that particular start tend to be increasingly exploited; finally they are exhausted; and then the style ceases to grow and becomes repetitive, or it widens its pattern for a renewed start or is displaced by a different style.

Now if one wants to attribute a sort of immanence to such cases or to assert that immanence is implied in the use of terms like "exploiting a pattern" or "saturation and exhaustion of a pattern," that can perhaps not be gainsaid. But I do not believe that these are serious lapses into crypto-teleological immanence.

Even in regard to art styles, an important value of Meyer Schapiro's paper was his showing, on the one hand, that though an exhausted style may freeze, on the other hand, there are cultures in which several conflicting currents of style may coexist, as in our own plastic arts at the present time. He instanced enough such variabilities of style behavior to make it clear that any theory which affirmed that style always followed a predetermined course, that it possessed an inherently necessary configuration of profile, could be shot full of holes.

Another point of principle concerns effective or efficient causes. Bidney first brought to my attention some loose thinking on my part there. He put it on the ground of my being an idealist along with Plato and Hegel, which I must say, with all due respect to Plato, shocked me somewhat. At any rate, it led me to thinking; and I am ready to

admit now that efficient causes, in the Aristotelian sense of fourfold causes, must be in individuals. The immediately effective causes of cultural phenomena must reside in human beings. And yet every man is always influenced by the previously existent culture that impinges on him. That, in turn, was created by other men, but they again were influenced by the culture preceding them. So in a problem covering the relation of personalities to culture, it is precisely the interaction of the two that is in focus. But in other situations involving the relations of forms of culture—and this is usual both in linguistics and in culture history—the immediately efficient personalities move out of focus, are abstracted from, and one operates telescopically directly from one form of culture to another. On this purely cultural level, the personalities who are the immediate causes are not denied, but they are omitted from the operation.

Now, if I may once more lapse into the personal, this last possibility I evidently sensed and lived up to early in my anthropological career without being fully aware of it; but it also led me into going too far and tending to assume or look for immanent causality independent of the human carriers of culture. Many of our errors in anthropology, as in other sciences, are due to our tendency when we find a new idea to make it into an over-all or universal one. It is a human frailty to like all-or-none attitudes, whereas most actual situations are more or less mixed. So I now and then inferred from the fact that in certain intellectual operations—such as much of linguistics—human beings could be taken for granted and left out, that the relations of the abstracted cultural forms actually took place in a sort of vacuum and could be dealt with as a separate, self-sufficient, and self-fulfilling universe. Obviously, however, cultural interactions do not take place in a vacuum or wholly autonomous subuniverse but in a real and very mixed world, on this planet that we call earth. Cultural relations and influences are indeed involved in culture, but so are men with their human nature and their individual personalities; so are social factors, such as the mass and cohesiveness of groups; and so are limitations, pressures, and catastrophes of the total nonhuman environment, together with random and varying combinations of these. It is a causally complex world in which culture occurs—so complicated that it is a testimonial to the strength of the basic cultural impulses that any cultural patterning at all is recognizable among the welter of unco-ordinated forces.

Another point in this connection is a suggestion recently made by Rushton Coulborn. Believe it or not, he is a historian, but he thinks about culture. And he thinks about causes, too. In an article in the first number of the *Anthropologist* this year, he makes the point that most of any culture at a given moment always comes from the past—to which we might add that most of it probably comes from outside, too. If so, one can speak of culture being largely the author of other culture, and hence that culture causes culture, provided that one does not exclude the agency of men also, other than provisionally and operationally.

Coulborn goes on to point out that what professional historians mainly deal with are events which are irregular, novel, and extraordinary and in which human wills—plus environment—are the dominant causes; but that anthropologists deal with culture, which is continuous, repetitive, canalized—in a word, patterned—and whose determination is largely by previous culture, with infringements from personal wills and nonhuman environment producing chiefly innovations, disturb-

ances, irregularities—producing what from the angle of culture pattern is extrinsic and "accidental."

This view seems fair, balanced, and theoretically important.

Speaking of accident, I would pass once more from culture to biology. So-called "genetic drift" due to the formation of isolates evidently operates as a matter of accident disturbing the even flow of the patterns of adaptive evolution under natural selection. It is factors of distributional segregation, due to elements extrinsic to progressive adaptation, that cause the isolations and bring about the "drift."

There is considerable dispute on among geneticists and other biologists as to how far nonadaptive factors are at work in evolution. I think the sound disposition is to admit the probability of a sizable proportion of nonadaptive changes and persistences. On the other hand, adaptive selection undoubtedly is extremely frequent, and it is a positive principle. Therewith, however, it becomes a temptation to hold that all selection is or must be adaptive, ruling out disturbing accident and making a neat, closed monistic system. Open-ended, limited interpretations surely are less satisfying emotionally, but they carry us farthest toward true understanding.

CREATIVITY

"Creativity" is a word we are beginning to hear more about after a long silence. Toynbee brings up his creative minority, along with God and morality. Bidney has several times specified creativity as an essential factor in human culture. I do not know whether Father Koppers in occasionally mentioning *Geisteswissenschaft* is thinking in terms of creativity as such, or whether the usage is mainly a terminological hangover of still referring to culture as *geistig*.

I suspect I have played perilously near the idea at one time or another in construing the climax and then exhaustion of a pattern in terms of optimum and then subsidence of realization of genius. In *Configurations* I seem to have evaded the outright concept of creativity, and the term is not in the Index. However, I have freely operated with the concept of intense or high values and with genius construed as a measure of pattern development, and these may be more or less translatable into creativity, if one will.

If we recognize creativity and if it proves operationally definable, it may help us to explain better the endless variability and irregularity which overlie pattern in the history of culture. It may also be a link in recognizing more clearly the relation between personality and culture—though, so far, creativity is a term which personality psychologists have shied away from. As for style, creativity may well be a better means to explain what style is and how it happens than, for instance, the old basic physiological needs or their alleged psychological derivatives. Or, conversely, personality psychology may help to define creativity. In any event, however, a usable concept of creativity must get itself dissociated from spirit as something outside nature. It cannot be a *deus ex machina* which we inject when we are stuck for natural explanations. If we are going to use the concept, it must obviously be defined in natural science terms; and we may look forward to philosophers like Northrop and Bidney, with perhaps help from personality psychologists, to aid us in finding more of a definition than we have.

VALUES

I am now to speak about values. I hope you will agree with some of my propositions about them. First of all, I would maintain that values are principles of organization of culture and therefore influence culture. If culture is an order of phenomena of nature,

values are therefore phenomena of nature, even though subjective ones. Existing within the cosmos, they are a legitimate and necessary subject of natural science investigation. Others have said this before, and so have I; but it was before an audience mainly of non-anthropologists, and I am glad to have the chance to reaffirm it before this international group of anthropologists.

Values evidently are intimately associated with the most basic and implicit patterning of the phenomena of culture. This may sound dogmatic, but I suspect that most of you will agree, in principle, even if you hesitate to embark on the pursuit.

Next, it looks as if values must be found, got at, or determined by empathy. That means not by some mystic or supernatural means, but by experiencing them in some degree. I think one has to live himself into values in order to know what they really are. Not that one has necessarily to become a complete member of another culture, to go native or foreign. Perhaps one can get values by observing, listening, asking. You can certainly reach some of the values of Indian philosophy or Chinese literature by reading; but even that is an experiencing. Values have got to be felt, at least vicariously: I think that has to be recognized.

There is a fear of feelings. It is said, "Feelings are one thing, but science another." Yet if feelings are important phenomena, we will have to operate with them; and if the only way to get hold of them is by experiencing them —well, we shall just have to experience them, or go in for a wholly different subject of study than culture.

If that is true, it follows that perhaps one reason why cultural anthropologists have gone farther in dealing successfully with values than, say, sociologists or economists is that they operate less with questionnaires and census data and averages but more with the actual individuals in a community. It is the *Gemeinschaft* approach, its personal, face-to-face contact, which makes the appreciation of the other fellow's values so infinitely easier than seeing them described in chill-blooded scientific jargon or statistics.

It seems also to follow that what Linton has called the "ideal culture" is as important for the study of values as the "actual culture"—or perhaps more important. The ideal culture is in a sense the value system of a culture. It is probable that this ideal culture with its values is, in large measure, the *de facto* basis of our apprehension of the real culture. Without it, one has a thousand disparate items which add up to little. With the ideal value system in hand, the culture begins to make sense. It gathers into a whole and fits into patterns.

Similarly, the essential thing that a linguist gets in determining the phonemes and structure of a language is its ideal pattern, its value system of forms. Only as against this pattern of pronunciation and grammar do the infinite individual and momentary variations of speech, the slurs and slovenings, the colloquialisms and lapses, make sense. Before that they are only confusing: we do not even know what is pattern and what is nonsignificant deviation.

A great many of us in dealing with natives or downtown Gypsies or immigrant Poles or Italians have come to like our informants, to appreciate their ways, and, in a sense, to be propagandists, or at least apologists, for their cultures. Is such an attitude a value judgment—perhaps a biased value judgment? I think it is. But, even if so, let us look charitably upon it as a natural spilling-over of empathy, a bit of which is perhaps necessary for most of us if we are to secure the values of the culture studied.

I also believe Nadel was right when he said here that value judgments are always made by ethnographers—as by-

products of study, of course, not as its aim. We must also admit value judgments as distinct from values. Value judgments are decisions for action; they take sides as acts of will. Values are things one feels in order to apprehend and describe as objectively as possible.

Lowie told us of the very different reactions he experienced in his contacts with his Plains culture tribes, Basin tribes, and Pueblo cultures. He called these value judgments or preferences "irrational," though "nonrational" would perhaps have been a more exact term for such emotional participations. And he went on to tell us how conscientiously he thought he had left out of his monograph on Hopi kinship the feelings he had about the Hopi, whom he felt as insignificant, timorous beings with a code of morals and behavior much inferior to that of the upstanding, impressive Plains tribesmen. I think he did stifle this value judgment in his monograph. But I think Lowie did not leave values out of his book on the Crow, and I think it is a better book for it. This time he let the values penetrate and re-emerge. As far as possible, he presented the values in his Crows' own statements; but he put them in.

In addition to the natives' own values as they recognize them, we shall also need ultimately the deeper, implicit values of cultures. In ascertaining these, I do not believe we need too much to fear involvement in our own emotional reactions, if only we recognize them as such and try to keep them distinct from our intellectual findings.

SUBHUMAN VALUES

There is a hobby that I have been playing with for several years without having put it into print or having spoken about it at a meeting, but which I am going to touch on now. I do not recall that the subject came up directly in our discussions, but, as these went on, something awakened the buried notion in me. I would like to raise the question, problematically, whether it is not possible to project something of the quality of human values downward below culture into the subhuman field. At any rate, we seem to have on the straight organic level things that are interestingly similar to values, though expressed there in organic structure and function. These qualitative differential weightings of species and classes of organisms obviously do not reside in beliefs and attitudes as human values do. We do not even know whether animals have attitudes; they certainly do not formulate them. It would be interesting if perchance some of our trained philosophers here would say whether this parallel which I glimpse is a mere empty analogy or whether it has in it something more substantial. Biologists do recognize and deal with the qualitative patternings or subhuman values that I am about to mention; but they do not call them values, for it has long been part of the basic code of scientists that values are outside science.

As an instance I take birds with their power of flight. Birds possess certain interrelated features or qualities which other animals do not have. They are winged and they are warm-blooded—almost hot-blooded. They need the heat because of the loss of temperature in rapid motion through the air. That is also why they have feathers. The feathers, again, lie close to the body, and at the same time spread out in the wings. They therefore both preserve body heat and help flight. To aid the latter, the bones are hollow, the lower vertebrae rigid. Birds are streamlined in head, body shape, the fit of the feathers, the retraction of the feet in flight—almost as much as the retraction of landing gear in an airplane.

Birds have an unusually rapid growth. An ordinary-sized bird reaches

adult size in a few weeks after hatching, and very soon thereafter it is usually able to fly. This rapid maturation seems again to be required with flight as the dominant mode of locomotion. A helpless infancy for months prolonged would tend against survival. That feature, in turn, involves parental feeding and is the evident reason for birds generally being more actively domestic, with constructed homes, and even more addicted to their young than mammals. The fact of flying obviously means that they can have no hands, no forelimbs efficient for grasping or holding. It is a question whether that correlates with the fact that they have a solid, structurally simple beak instead of a mouth with teeth. They certainly cannot usefully co-ordinate their forelimbs with their mouths, as a squirrel or a dog or a bear can; and, if they had teeth, these might entangle; their characteristic pecking is untypical of other vertebrates.

All these features and many others characteristic of the class of birds are obviously interrelated. They serve and emphasize certain sharply patterned qualities. Perhaps the legitimate parallel to culture ends with the qualitative patterning. But I raise the question whether we do not have in such organic patternings the congenital, hereditary, structuro-functional, presymbolic prototypes of what on the symbolic, nongenetic level of transmission we are wont to call the values of human culture.

UNIVERSAL VALUES

Finally, there is the question of permanent or universal values. For a year and a half Clyde Kluckhohn and I have wrestled about that as co-authors, never very far apart, but still confronting each other over a little barrier that we could not quite cross. Finally, only yesterday, I was asked a simple question by a nonanthropologist which somehow jelled the problem, and I would like leave to put that reaction before you.

Acceptance of relativity of morals and values invests our conceptualization of each culture with a certain degree of autonomy. It is a cardinal point in anthropological doctrine. We all admit cultures to differ, in place, time, content, forms, and values. It is possible to imagine how our culture might come to be quite different in a thousand years through some mutation in the genes of the human species. Even more, we would all admit that the values of our own descendants a thousand years from now will almost certainly differ from our values, merely through the seemingly inevitable processes of culture change. Who, for that matter, would want to freeze our values today —at least what anthropologist would want to have them all exactly retained unaltered in 2952? To me, that would seem a most dreary future to look forward to.

On the other hand, we hear it said, "Accept relativism all the way, and what have you ahead but a philosophy of cynicism, with immorality sanctioned along with morality? There must be some permanent core, a residue to cling to, in all this fluctuating relativism—at least a common denominator of universal features."

Well, the fact that all men have an essentially common anatomy and physiology, differing very little if at all, makes a pervasive generic similarity of cultural values expectable. On the other hand, what we know about culture and its historic behavior, its plasticity, its susceptibility to influences of all sorts, makes variability also expectable. The situation then boils down to this, that we can expect both a generic similarity of values and a considerable degree of variability among cultures. The problem is neither: Is there some absolute uniformity? nor: Is relativistic

differentiation unlimited? The proper form of the problem is: How much similarity and how much dissimilarity are there? What the quarrel has really been about is that we tended to see it as an all-or-none distinction: either relativism was true, or it was false. Whereas it is really a question of how far similarity and dissimilarity extend.

That is a matter to be worked out piecemeal and progressively. But in its customary form the question is in the area of unanswerable and therefore nonscientific problems. The pair of questions, Are values relative? Are there universal values? is unanswerable because both are true. An answer really amounts to a contention that we would like them to be relative, or we would dislike them to be such. It seems a situation like that regarding free will and determinism, which also cannot be answered scientifically but which life, sooner or later, does ask of us. Theoretically, we have to act, we must make decisions, whether we accept responsibility or not. If we are determinists, we still have the illusion of choice, whereas we may believe in free will and yet feel compelled by fatalistic necessity. Either seemingly theoretical position is really a preferential choice between conflicting evidences.

So for values. We can act as if our culturally acquired but preferentially held values were absolute; and, in general, that is what we do when we have to act, even if we consider ourselves relativists. We do not review the hundreds of other value systems and paralyze our decision, but we use our own value system, act according to it, and abide by the consequences. As human beings, that is all we can ordinarily do. But the problem of how far values are relative and variable or universal and permanent is wholly separate from living. It is a fundamental scientific question which will not be answered soon and will not be answered in terms of zero or a hundred per cent. The answer will presumably be somewhere in between and will begin to be worked out as the question is reformulated. Values, then, are both particular and relativistic and universal and permanent.

APPENDIX I

List of Participants

DR. ERWIN H. ACKERKNECHT, Professor and Chairman, Department of the History of Medicine, University of Wisconsin Medical School.

DR. MARSTON BATES, Professor of Zoölogy, University of Michigan.

DR. RALPH L. BEALS, Professor of Anthropology, University of California, Los Angeles.

DR. WENDELL C. BENNETT, Professor and Chairman, Department of Anthropology, Yale University.

DR. DAVID BIDNEY, Associate Professor of Anthropology and Philosophy, Indiana University.

JUNIUS B. BIRD, Associate Curator of Archaeology, American Museum of Natural History, New York.

DR. KAJ BIRKET-SMITH, Head Curator, Ethnographical Department, National Museum, Copenhagen, Denmark.

DR. WILLIAM C. BOYD, Professor of Immunochemistry, Boston University.

DR. J. O. BREW, Director, Peabody Museum of Archaeology and Ethnology, Harvard University.

DR. SALVADOR CANALS FRAU, Nazarre 3455, Buenos Aires, Argentina.

DR. ALFONSO CASO Y ANDRADE, Director, Instituto Nacional Indigenista, Mexico, D.F.

DR. WILLIAM A. CAUDILL, Instructor in Social Anthropology, Department of Social Relations, Harvard University.

DR. ELIOT D. CHAPPLE, President, The E. D. Chapple Company, Inc., New York.

DR. JOHN GRAHAME D. CLARK, Disney Professor of Prehistoric Archaeology, Cambridge University, England.

DR. CARLETON S. COON, Curator of General Ethnology, The University Museum, University of Pennsylvania.

JOSÉ M. CRUXENT, Director, Museo de Ciencias Naturales, Apartado Correos 3893, Caracas, Venezuela.

DR. ANTONIO JORGE DIAS, Director, Centro de Estudos de Etnologia Peninsular, University of Oporto, Oporto, Portugal.

DR. FRED EGGAN, Professor of Anthropology, University of Chicago.

DR. LOREN C. EISELEY, Professor and Chairman, Department of Anthropology, University of Pennsylvania.

DR. PAUL FEJOS, Director of Research, Wenner-Gren Foundation for Anthropological Research, Inc., New York.

DR. DARYLL FORDE, Director, International African Institute, London, England.

DR. JOSEPH H. GREENBERG, Associate Professor of Anthropology, Columbia University.

DR. MARY R. HAAS, Associate Professor of Siamese and Linguistics, Department of Oriental Languages, University of California, Berkeley.

DR. A. IRVING HALLOWELL, Professor of Anthropology, University of Pennsylvania.

DR. ROBERT F. HEIZER, Assistant Professor of Anthropology, University of California, Berkeley.

DR. GERRIT JAN HELD, Professor, Ethnological Department, University of Indonesia.

DR. JULES HENRY, Associate Professor of Anthropology, Washington University.

DR. HARRY HOIJER, Professor of Anthropology, University of California, Los Angeles.

DR. EARNEST A. HOOTON, Professor and Chairman, Department of Anthropology, Harvard University.

EIICHIRO ISHIDA, Professor of Cultural Anthropology, Tokyo University, Tokyo, Japan.

DR. FRANJO IVANIČEK, Director, Anthropological Institute, Medical Faculty, University of Zagreb, Zagreb, Yugoslavia.

DR. ROMAN JAKOBSON, Professor of Slavic Languages and Literatures, Harvard University.

DR. EDWARD A. KENNARD, Director of Social Science Research, Foreign Service Institute, United States Department of State, Washington, D.C.

DR. ALFRED V. KIDDER, Director Emeritus, Division of Historical Research, Carnegie Institution of Washington.

DR. CLYDE KLUCKHOHN, Professor of Anthropology, Harvard University.

REV. DR. WILHELM KOPPERS, Professor and President, Institut für Volkerkunde, Universität Wien, Vienna, Austria.

ALEX D. KRIEGER, Research Archeologist, Department of Anthropology, University of Texas.

DR. ALFRED L. KROEBER, Visiting Professor of Anthropology, Columbia University, 1948–52. (Emeritus, University of California, Berkeley).

DR. CLAUDE LÉVI-STRAUSS, Director of Studies, École pratique des hautes Études, Paris, France.

DR. OSCAR LEWIS, Associate Professor, Anthropology, University of Illinois.

DR. GERHARD LINDBLOM, Director, Statens Etnografiska Museum, Stockholm, Sweden.

DR. RALPH LINTON, Sterling Professor of Anthropology, Yale University.

DR. FLOYD G. LOUNSBURY, Assistant Professor of Anthropology, Yale University.

DR. ROBERT H. LOWIE, Professor Emeritus of Anthropology, University of California, Berkeley.

DR. GORDON MACGREGOR, Technical Cooperation Administration, United States Department of State, Washington, D.C.

DR. DHIRENDRA NATH MAJUMDAR, Professor and Head, Department of Anthropology, Lucknow University, Lucknow, India.

DR. DAVID G. MANDELBAUM, Professor of Anthropology, University of California, Los Angeles.

DR. ANDRÉ MARTINET, Professor of Linguistics, Columbia University.

DR. THEODORE D. MCCOWN, Professor and Chairman, Department of Anthropology, University of California, Berkeley.

DR. MARGARET MEAD, Associate Curator of Ethnology, American Museum of Natural History, New York.

DR. ALFRED MÉTRAUX, Department of Social Sciences, United Nations Educational, Scientific, and Cultural Organization (UNESCO), Paris, France.

DR. CARLOS MONGE, Director, Institute of Andean Biology, University of San Marcos, Lima, Peru.

DR. HALLAM L. MOVIUS, JR., Curator of Palaeolithic Archaeology, Peabody Museum of Archaeology and Ethnology, Harvard University.

DR. GEORGE P. MURDOCK, Professor of Anthropology, Yale University.

DR. S. F. NADEL, Professor of Anthropology, Australia National University, Canberra, Australia.

DR. MONKICHI NAMBA, Professor of Sociology and Cultural Anthropology, Doshisha University, Kyoto, Japan.

DR. RUSSELL W. NEWMAN, Quartermaster Climatic Research Laboratory, Lawrence, Massachusetts.

DR. F. S. C. NORTHROP, Sterling Professor of Philosophy and Law, Yale University.

DR. KENNETH PAGE OAKLEY, Principal Science Officer, Department of Geology, British Museum of Natural History, London, England.

DR. BENJAMIN D. PAUL, Lecturer in Social Anthropology, Harvard University.

DR. ROBERT REDFIELD, Professor of Anthropology, University of Chicago.

DR. IRVING ROUSE, Associate Curator of Anthropology, Peabody Museum, Yale University.

DR. JOHN H. ROWE, Associate Professor of Anthropology, University of California, Berkeley.

DR. S. SANGVICHIEN, Professor and Head, Department of Anatomy, Faculty of Medicine and Siriraj Hospital, University of Medical Sciences, Bangkok, Thailand.

DR. MEYER SCHAPIRO, Professor of Fine Arts and Archaeology, Columbia University.

DR. MUZAFFER SENYÜREK, Professor of Anthropology, Antropoloji Enstitusu, Dil Ve Tarih-cuğrafyā Fakültesi, Ankara Üniversitesi, Ankara, Turkey.

MR. P. G. SHAH, President, Anthropological Society of Bombay, Bombay, India.

DR. ALEXANDER SPOEHR, Curator of Oceanic Ethnology, Chicago Natural History Museum.

DR. JULIAN H. STEWARD, Graduate Research Professor of Anthropology, University of Illinois.

DR. WILLIAM L. STRAUS, JR., Professor of Physical Anthropology, School of Medicine, Johns Hopkins University.

DR. JAMES M. TANNER, Lecturer in Physiology, Sherrington School of Physiology, St. Thomas' Hospital, London, England.

DR. SOL TAX, Professor of Anthropology and Associate Dean, Division of Social Sciences, University of Chicago.

PÉRE PIERRE TEILHARD DE CHARDIN, Research Associate, Wenner-Gren Foundation for Anthropological Research, Inc., New York.

DR. FRANZ TERMER, Director, Museum für Volkerkunde, Hamburg, Germany.

DR. STITH THOMPSON, Professor of English and Folklore, University of Indiana.

DR. HENRI V. VALLOIS, Director, Musée de l'Homme, Paris, France.

DR. C. F. VOEGELIN, Professor and Chairman, Department of Anthropology, University of Indiana.

DR. S. L. WASHBURN, Associate Professor and Chairman, Department of Anthropology, University of Chicago.

DR. HANS WEINERT, Director, Anthropologisches Institut, University of Kiel, Kiel, Germany.

DR. LESLIE A. WHITE, Professor and Chairman, Department of Anthropology, University of Michigan.

DR. GORDON R. WILLEY, Bowditch Professor of Central American and Mexican Archeology and Ethnology, Harvard University.

APPENDIX II

List of Inventory Papers Discussed*

ACKERKNECHT, ERWIN H. "Paleopathology."
BATES, MARSTON. "Human Ecology."
BEALS, RALPH. "Acculturation."
BENNETT, WENDELL C. "New World Culture History: South America."
BIDNEY, DAVID. "The Concept of Value in Modern Anthropology."
BOYD, WILLIAM C. "The Contributions of Genetics to Anthropology."
CARTER, G. S. "The Theory of Evolution and the Evolution of Man."
CASO, ALFONSO. "New World Culture History: Middle America."
CAUDILL, WILLIAM. "Applied Anthropology in Medicine."
CHAPPLE, ELIOT D. "Applied Anthropology in Industry."
CHILDE, V. GORDON. "Old World Prehistory: Neolithic."
CLARK, J. GRAHAME D. "Archeological Theories and Interpretation: Old World."
FORDE, DARYLL. "Applied Anthropology in Government: British Africa."
GREENBERG, JOSEPH H. "Historical Linguistics and Unwritten Languages."
HAAS, MARY R. "The Application of Linguistics to Language Teaching."
HALLOWELL, A. IRVING. "Culture, Personality, and Society."
HEIZER, ROBERT F. "Long-Range Dating in Archeology."
HELD, G. JAN. "Applied Anthropology in Government: The Netherlands."
HENRY, JULES, and SPIRO, MELFORD E. "Psychological Techniques: Projective Tests in Field Work."
HOIJER, HARRY. "The Relation of Language to Culture."
KENNARD, EDWARD A., and MACGREGOR, GORDON. "Applied Anthropology in Government: United States."
KLUCKHOHN, CLYDE. "Universal Categories of Culture."
KRIEGER, ALEX D. "New World Culture History: Anglo-America."
LÉVI-STRAUSS, CLAUDE. "Social Structure."
LEWIS, OSCAR. "Controls and Experiments in Field Work."
LOUNSBURY, FLOYD G. "Field Methods and Techniques in Linguistics."
MARTINET, ANDRÉ. "Structural Linguistics."
MEAD, MARGARET. "National Character."
MÉTRAUX, ALFRED. "Applied Anthropology in Government: United Nations."
MONGE, CARLOS. "Biological Basis of Human Behavior."
MOVIUS, HALLAM L., JR. "Old World Prehistory: Paleolithic."
MURDOCK, GEORGE P. "The Processing of Anthropological Materials."
NEWMAN, RUSSELL W. "Applied Anthropometry."
NORTHROP, F. S. C. "Cultural Values."
OAKLEY, KENNETH P. "Dating Fossil Human Remains."
PAUL, BENJAMIN D. "Interview Techniques and Field Relationships."
REDFIELD, ROBERT. "Relations of Anthropology to the Social Sciences and to the Humanities."
ROUSE, IRVING. "The Strategy of Culture History."
ROWE, JOHN HOWLAND. "Technical Aids in Anthropology: A Historical Survey."
SCHAPIRO, MEYER. "Style."
STEWARD, JULIAN H. "Evolution and Process."
STRAUS, WILLIAM L., JR. "Primates."
STRONG, WM. DUNCAN. "Historical Approach in Anthropology."
TANNER, J. M. "Growth and Constitution."

* These papers have been published as *Anthropology Today: An Encyclopedic Inventory* (Chicago: University of Chicago Press, 1953).

TEILHARD DE CHARDIN, PIERRE. "The Idea of Fossil Man."
THOMPSON, STITH. "Advances in Folklore Studies."
VALLOIS, HENRI V. "Race."
WASHBURN, S. L. "The Strategy of Physical Anthropology."
WEINERT, HANS. "Der fossile Mensch."
WILLEY, GORDON R. "Archeological Theories and Interpretation: New World."

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