

Cultural Discontinuities and Personality Transformation

Margaret Mead

I.

Any contact with the luminous mind of Kurt Lewin meant for one so fortunate such a variety of insights, foresights and hindsight that to select one from the many for discussion in a memorial lecture is very difficult. But those of us who were fortunate enough to work with him over a period of years always found a kind of parallelism between our problem of the moment and what appeared to catch and challenge his interest. This was no doubt partially an illusion, a by-product of his fecund imagination which at any moment contained so many possibilities that the eager expounder of some budding idea was sure to find some corresponding idea, and partially a reflection of the generosity with which his eyes used to gleam from the back row of his own meetings as other people put less well what he would have put better. I know that if Kurt were here in person, I would bring from my latest research the problem which I felt to be most pressing and he would make me feel that that was just what we needed to work on next. The role of one who lives so vividly in the lives of his fellow workers cannot be better perpetuated than as he would have lived it himself until, when we have solved some of the problems that he set and did not live to solve, his stature changes also. We may hope—as Gardiner Murphy (4) suggested in his lecture last year would be possible for mankind in general—that from our scientific efforts, which benefited so much from his brilliance and enthusiasm, we may rear a shelter for men's minds and hearts where he will have a place fairer than any in which his imagination ever dwelt.

This article is reprinted from *JSI* Supplement No. 8, 1954, 3-16.

The years in which I worked most intensively with Kurt were his last years at the University of Iowa, when we worked together on a series of problems of change. The Committee on Food Habits, of the National Research Council, had been requested to study and advise governmental agencies how to change human food habits so that they would embody the findings of the new science of nutrition and also, during the wartime emergency, maintain the nutritional status of the American people, in spite of shortages and necessary shifts in types of food. As anthropologists, we came to the conclusion that our first task was to find out what American food habits were, what was the cultural setting within which different groups of Americans—those of foreign stock, those from different parts of the country—selected, prepared, ate, enjoyed, or endured the food which kept them well or indifferently nourished. However, as a psychologist, Kurt's imagination turned first to experiment. This contrast was one which he and I kept constantly in play: How to fit together anthropological research on the culture and psychological research on the behavior of individuals in the culture; when to study what was given in the historical situation, and when to construct experiments which would test what had been learned from the anthropological studies—although Kurt was never very much interested in the mere testing of good hypotheses, for he worked too near the growing edge of knowledge to be very much concerned with this necessary but pedestrian task—and when to take off from the anthropological studies to learn something new. As I began to work with the problem, from the standpoint of culture, I realized that the question had been asked incorrectly, that we should not ask: *How can we change food habits?* but rather: *How do food habits change?* Any question in which the applied scientist saw his material as passive to his manipulations was a faulty question, stifling rather than releasing, both to the investigator and to those whose problems he claimed to be studying. Kurt, for his part, focussed on the study of how these changes took place, what were the principles underlying man's selection and addiction to one food instead of another, coming out very early with a characteristic formulation: "Men do not eat what they like, they like what they eat"—a first approximation of a psycho-cultural statement about food habits.

So Kurt set up a laboratory to work on problems of change, with food habits as the setting for the research, but his real interest, of course, was far wider even than problems of how the people of the United States, or of the world for which we were also trying to plan, could learn to eat wisely and well. It was a lively group those days in Iowa City who used to meet my plane, and if I come down at any prairie airport at sunset I can still recapture the sense of excitement and freedom, as I was greeted by a group of students whose whole personalities had been freed by the at-

mosphere in which Kurt worked—to argue, dream, plan, experiment, and work, work 24 hours a day and not know it was work at all.

In this laboratory group I played a double role. First I had to state what the large-scale political problems were which we must keep in mind, questions like the probability of rationing, whether the rationing plan would be one in which all shared equally the same number of pounds of sugar or butter—in which case we could, on the basis of cultural data, predict one kind of behavior from the American people—or rationing in which there would be choice, inequality, opportunities for initiative, and passive resentment. I had to bring news about the relationship between our special little research budget—which looked justifiably large in Iowa City because so many people were able to do good work within its slender framework—and the whole complexity of the war effort, how the Committee on Food Habits, set up by the National Research Council at the request of Dr. M. L. Wilson, in his role of Coordinator of Nutrition for the Office of Defense Health and Welfare, fitted into wider governmental plans which took the whole problem of cultural change into account. For it was Dr. M. L. Wilson's statesman's vision of what anthropology and psychology could contribute to human affairs which had brought anthropology into the field of nutrition, and had found research money for Kurt Lewin's Iowa Laboratory. And second, I had to state our cultural hypotheses in forms which were intelligible to Kurt and his research group, over-sensitive to individual differences, still skeptical about cultural differences. We wrestled far into the night over such cultural formulations which would be derived from a study of New England and the Middle West: *If you eat enough of what is good for you and not good, you may have a little of what is good and not good for you.* Such cultural formulations were then tested out in versions of the Bavelas test: *What is a good meal for a boy to eat?* and *Who would praise him for it?* From psychological open-ended tests of this sort—psychological tests which were related to carefully constructed cultural hypotheses—we obtained new information about culture, finer differentiations of the maternal and paternal moral roles in the local Iowa version of American culture, concrete details which showed that father presided over meat and butter, mother over green vegetables and fruit juices, while desserts and soft drinks—remembered in the non-moral limbo of a “bad meal”—were wholly delightful and approved by no parent at all.

It was in the course of these studies that group decision developed—strange as it now seems that a procedure which, in various successor forms, now echoes round the world in internationally-sponsored committee meetings and conferences should have developed in a set of experiments on how housewives could learn to eat “variety” meats, the American sugar

coating of the too visceral and so unpleasant term “glandular meats,” and serve them to husbands who, another set of studies revealed, did not, as their wives claimed, impose their preferences on their wives, but instead had to eat what their wives themselves liked to eat. Here, in carefully constructed small groups of housewives or fraternity students, in experiments on using whole-wheat bread, and in one ill-fated experiment in which I was brought in as the prestige expert from Washington to express publicly high approval of turnips—which had no effect at all—we learned how groups of people can do those things which they themselves want to do, how they can decide to reduce the gap between their ideals and their execution. Kurt’s special gift for understanding American ideals of democracy led him to include in these first research plans his clear recognition that you cannot do things *to* people but only *with* them, which already informed his work with students and colleagues, and was now extended to include citizen members of the local nutrition committee and a woman food writer on the local paper. The inclusion of such lay participation was full of troublesome problems even for someone as bountifully dowered with enthusiasm as Kurt. But the experiment laid the basis for principles which are now found in manuals for the conduct of visiting technical assistance experts in Iran or Nigeria.

All through these experiments, which were kept close to the world emergency, close to the realities of the governmental departments which underwrote the work, and close to the real living human beings in the local community, seemingly so different from those laboratories where abstractions called “subjects” perform tasks stripped of relationship to life, Kurt and his students, Bavelas, Festinger, Willerman, Patricia Woodward—Gertrude Lewin participating in all of it and called in for fine qualitative analyses to compensate for IBM results which had been coarsened by speed and pressure—were alert for the theoretical implications of what they did. On some of these theoretical leads, the most formal and beautiful experiments such as those being done by Bavelas (1) on group performance at the Massachusetts Institute of Technology, would some day be built. It was a setting peculiarly congenial to me as an anthropologist, used to jotting down a high-level abstraction plumb in the middle of a crowded scene, filled with smoke and sweat and the sour smell of sago or rotted breadfruit, even as I struggled to see just which paternal aunt had cut the cord of the lustily crying new baby. All of us had our theoretical capacities strained to the utmost by Kurt’s lively march ahead, and we kept close to the ground because we worked in such real settings. There is little doubt that coming to grips with the non-arrival of a shipment of lettuce involved in an experiment was an excellent setting for cooperation between anthropologists and psychologists and for the birth of action research.

II.

Out of these years there came a whole variety of insights on different levels—group decision, group productivity, channel theory, possibilities of measuring cultural homogeneity, and a maturing theory of change experiments. Each of these was grounded in the concrete and the immediate, and the set of experiments from which I wish to take off today was perhaps the most concrete and apparently trivial of them all—a study of the conditions under which a new container would be most acceptable, financed by a small grant from the Public Health Committee of the Cup and Container Institute. It was a tiny grant, but the flexible, responsible research organization which Kurt had set up—with the whole masses of hypotheses at half a dozen levels, real lines of cooperation into the community, students alert and enthusiastic—was ready to use it. So Leon Festinger did a series of carefully controlled real-life experiments, combined with laboratory controls, and came out with the conclusion that it was easier to get people to accept a new kind of container—in this case paper cups—if the beverage they drank was new as well.

These results were invested, as everything Kurt touched was invested, with importance, importance for our understanding of the mechanisms of social change, and for our practical attempts to develop a better world from a world torn by war and transition. They raised for me a question which I had wanted to study on my first field trip when I was 23 years old, and my mind was still reverberating with what I had learned in psychology which might fruitfully be combined with anthropological research. I had done my dissertation on the question of the relative stability of different elements of culture, a question of theoretical interest to an anthropology concerned with reconstructing past stages of history by hypothesizing that some one element—the form of material objects, the ways in which they were constructed, the ceremonies which surrounded them, or the social organization within which they were made—was more stable than another. When I had completed this research on a strictly formal cultural level, I wanted to go on to study what happened to those individuals who bore the burden of change within any culture, and here I was influenced by the research my mother was doing on second-generation Italians in Southern New Jersey. If we could ask which elements of culture are most stable in the behavior of a group of people who form a society, then obviously, I argued, there must be a related problem: How were the old and new elements in a changing culture integrated affectively within the personalities of the human beings who comprised that society, whose patterned ranges of individual responses provided us with the behavior which we could abstract into a “patterned way of life”? At my

doctoral examination, Professor Hollingworth asked me, "What relationship does your dissertation have to the normal curve?" I answered, quite truthfully, "None," and he rose and went away, a little wistfully I thought. Professor Woodworth, who had read my dissertation with what was to me then a wholly inexplicable capacity to recognize small discrepancies in the reported measurements of the canoes of various Polynesian islands, brought me back to the psychological problem—when I returned from my first field trip—with a question in which the whole of cultural learning was stated in individual psychological terms, "When does a human child become an *Indian*?"

While I was working on my thesis I had already begun to dream of studying cultural change in the field in psychological terms and with a gleaming new psychological tool, the psycho-galvanometer, which would measure the affective strength of old and new elements of culture. Which would be most affectively toned, those habits which belonged to the past and had been learned in early childhood, or those, just learned, barely learned, to which people might be clinging with a desperate sense of urgency which would be far more affectively toned than the better known, more familiar parts of their lives? But experiments with the psycho-galvanometer were proving disappointing; the measures were too non-discriminating. I had no other tool to suggest that sounded so promising, and I accepted—as part of a vigorous struggle with Professor Boas over whether I would study Indians in safety or risk the alleged dangers of islands seldom visited by steamers—the study of the cultural setting of adolescence instead. (If I make this lecture so autobiographical it is because I think that in a lecture named for Kurt Lewin it is appropriate to make no attempt to fragment the living history of the investigator, to separate problems from people, insights from experience, or theory from practice, but to give to each explicit recognition to the extent that our achievements as scientists are grounded in our place in the universe, linked as we are to every portion of it, past, present and future, in transactions among its creatures and creations.) This field choice did not mean that I lost interest in change or failed to record changes in the cultures of the people I studied, but it did mean a focus upon the problems of how human beings learned to embody homogeneous and more slowly changing cultures. This preoccupation carried with it its own special seduction—for the investigator becomes entranced with the beauty and integration of any interwoven pattern over which many people have worked for generations until all the basting threads have been removed and the whole, seen abstractly, and seen in varying human representations, gives the type of fulfillment which one receives from a great work of art. But contemplation of human culture is specially touched with wonder and hope because a culture is made by all who live within it, and one learns to trust ordinary men as well as to wait for genius.

On my second field trip, after Professor Woodworth had asked me his question of when a child became an Indian, I worked on the Manus of the Admiralty Islands—the group of primitive people whom I have just revisited after a period of 25 years, explicitly to study cultural change. Here the importance of discontinuities in the educational process were first seen as a problem. Later Ruth Benedict (2) wrote her paper on “Continuities and Discontinuities in Cultural Conditioning.” But this recognition that there was a marked discrepancy between the experiences of the Manus child and the life led by a Manus adult, led me to question the implications for personality—in Manus, or in any society with such discrepancies within the educational process—rather than to raise any new questions about cultural change itself. Even when, in 1930, I did a short study of cultural change in an American Indian tribe (3), not from my own choice but as part of my curatorial duties at the American Museum of Natural History, I did not take up again my earlier interest; the shambles which culture contact had made of a once proud people, their loss of dignity as they lived in a culture which was constantly shrinking and decaying, evoked repugnance and despair rather than any enthusiasm for charting out the personalities of individuals in a dying culture.

But when I began working with Kurt, it was Kurt’s insistence that one could only understand how anything worked by watching it move, that change experiments were essential to any developed social insight, which refocused my interest on the question which had lain dormant so long. During the close cooperation of those war years, the study of change became identified with his optimistic belief in the possibilities for the use of science in human affairs—an optimism which Kurt never lost, even though it was often sorely tried. So it seems appropriate to devote the main body of this lecture to the problem which was foreshadowed experimentally in those years at the University of Iowa.

III.

Stated briefly, the question is: What changes occur in adult individuals when the culture which they embody undergoes abrupt change involving sharp discontinuities between old and new institutions? This is the problem of change within one generation and differs from the question of slower change, or of what happens within personalities when children learn ways of life which their parents did not practice, or when adults must make provision for their children to learn and experience in ways they do not themselves command. In rapid change, as when adults emigrate to a new culture and take on its patterns of behavior, or when there is a sudden

social economic or religious revolution within a society, we find that adults abandon certain ways of behaving and behave quite differently instead. For example, they may shift from obedient, submissive behavior to active, democratic participation, from working with primitive tools on the land to working with machines in factories, from speaking a dialect as non-literates to reading and speaking a more standard version of their language or even a completely different language, from dependence on barter to the use of money, from complete self-sufficiency to dependence on an intricate modern economy. When we study peoples who embody cultures typical of the variations represented in this list, we find very great differences among the human beings in these societies with such contrasting ways of life. A democratically organized society evokes quite different human potentialities from those embodying a hierarchically organized one, as does an individualistic subsistence economy from a modern interdependent economy, etc. I wanted to know if the human beings who embodied one form were able to change sufficiently to embody, at least at many levels, the new form, what happened? And how did these changes relate to the sorts of changes which Kurt Lewin and his students had been able to produce by a change in group atmosphere? Were the individuals who lived for twenty years as adults in a radically new or different version of the culture within which they had been reared, different persons, or would we have to assume that, if the old conditions were reinstated, they would become again just what they would have been if the order of their lives had been undisturbed by change? These were questions which could be explored speculatively, deriving the possibilities from our growing knowledge of character formation. But I had found such speculations far less fertile than field work, even as Kurt had found the prettiest derivations sterile compared with the vitality of experimentation. The field anthropologist finds, as Kurt did, that we learn far more quickly from watching real things happen, and the disciplined testing of hypotheses comes best after the hypotheses have been so derived.

It was furthermore a deeply held belief of those of us who worked together during the war, that our scientific problems must be chosen with explicit relevance to the pressing concerns of our age, that what made any piece of research the next problem must be determined by the state of our knowledge AND the order in which that knowledge was likely to be needed. It seemed clear to me that most of the peoples on the earth's surface would be undergoing, within this century, profound changes in the course of a single generation, and that it was therefore very important for us to know more about what happened. What clues could a study of culture change by an anthropologist give us to such problems as whether adult learning is different, in kind, from first learning, and what were the steps by which

a group atmosphere was able to evoke recognizable related behavior from individuals of divergent cultural background?

I selected for this exploratory study the people of Manus, because they were reported to have gone through very rapid and cataclysmic social changes and to be demanding even more changes, and because—as I had studied small children there in 1928-29—this would provide me with a community of adults whom I had known as children, in a society where I had a record of what the adult culture had been, or, stated another way, of how those same children would have behaved, if no change had come about. I would be able to work with the same small community that I had studied 25 years before. Here again there was a line of comparability, as Kurt and I both preferred to work with multiple measurements on small groups, rather than with fewer measurements on larger groups or series of subjects. (This is an aspect of anthropological work which is often not recognized by psychologists who work with long series of individuals who have no ties with each other, not even that of a few hours' membership in an experimental group. When an anthropologist works intensively with a small village of 300 or 400 people, he is charting out a field of enormous complexity, in which each item of information about A is also information about every other member of the community, as A's brothers, cousins, brothers-in-law, rivals, neighbors, fishing partners, etc.) From the behavior of the children 25 years before I had predicted their future roles, had the culture remained relatively unchanged, and these could be compared with their roles under the new conditions.

It is not practicable in the course of this short lecture to deal substantively with what I found. I want to summarize the overt changes quite briefly and then devote the rest of the lecture to a special problem which my results raise—the problem of the significance of the pace, completeness and scope of change.

I found a community which had undergone a profound social revolution, whose people had, under their own native initiative and leadership, overhauled their old institutions, discarded those which they felt were incongruent with their desire to become a modern western type society, preserving those which they felt were congruent, and designing a new form of social-economic-religious-political life for themselves. Stated in other terms, they had moved from the stone age to the 20th century, from a society in which men insulted each other in a phallic war dance, as they exchanged thousands of dog's teeth in the name of a bride who would never be allowed to pronounce her husband's name, to a society which was willing to devote many hours of earnest debate to such questions as the relative responsibility of teacher and parents for the children's out of school hours, or the relationship between desirable adult character and type of child-rearing practices.

These changes had occurred without any dislocation in the power relations of individuals such as takes place when a different class or age group or religious group takes power in a revolutionary situation. The same men were leaders in the new society who would have been leaders in the old. If there was any difference, it lay in the slightly enhanced capacity of the mentally unstable to introduce confusion and distraction. Nor were the personalities of those individuals whom I had known well as children in any way unintelligible to me. It had been clear when he was fourteen that Pomat would be some kind of stuffed shirt; as a judge, he claimed and possessed all the virtues of impartiality which he urged in the village court room. Banyalo, despite his selection for a higher education, had lacked the spark of enthusiasm and trust which was necessary, and later, when his education was in demand in the new setting, this same lack of enthusiasm and trust showed through. In their temperamental qualities, the men were definitely sons of their own past selves; their relations to each other in the adult community were a predictable extrapolation from their childhood personalities, in terms of relative intelligence, curiosity, reliability, initiative, etc.

And yet almost every act, every word, and every expressed thought was different. People laughed and jested, sang and smiled at one another, who would have been sulky, persistently hostile before. Children, who would have been rebuked for the slightest damage to one leaf of a thatched wall, were indulgently permitted to kick holes straight through the walls of the new houses which had been built, in imitation of an American town, with such enthusiastic labor. Girls who would have been traded, irrespective of their own wishes, into marriages which promised the highest financial gains, were allowed to marry for love. Issues which would have been decided by shows of strength, ghostly pronouncements, and open threats of black magic, were decided by long communal discussion meetings, and the silent were urged to participate. An ethic which 25 years ago was concerned only with words and acts, was now concerned only with motives and thoughts—anger and lust were spiritual matters cutting one's mind off from God, while fighting and adultery were matters for the court. People who had had no calendar, no knowledge of geography, no sense of the political structure of the world, dealt with dates, records, maps, and their place in the British Commonwealth. A world unbounded in time or space had become bounded, and their conception of their own personalities—25 years ago a bundle of loosely related affects and cognitions—was now a more unified conception of a mind-soul localized in the head, and so related to the body that faulty thought processes could render one vulnerable to disease or non-responsive to modern drugs. They were capable of understanding why I was there, why I wanted to study them, of relating

themselves to me and my world, whereas 25 years ago they had no possibility of understanding altruism, research, dedication to long-term community aims or any other of the moral and intellectual complexities of modern social science.

In a sense this is a very preliminary report, for the large body of detailed projective tests on individuals has yet to be analyzed by my two associates, Theodore and Lenora Schwartz, but while their results will add to our comprehension of Manus personality today and possibly develop new hypotheses which can be explored further in my old material, the immediately essential materials are my own records of this group of people at two periods 25 years apart.

It is first necessary to consider under what conditions such a sweeping discontinuity in culture could have been introduced. Here the essential conditions seem to me to be that the change was one which the people wanted, initiated and carried through themselves, not something done to them or for them but by them, that the *whole* pattern of life—houses, dishes, furniture, clothing, political organization, beliefs about illness and death, marriage, child rearing, kinship, sense of their own identity—was changed at the same time, and that the whole change was carried through very quickly.

Because the change was initiated and executed by the people themselves, even though it was modelled on their understanding of Christian teaching, British law, and American standards of democracy—the importance of human beings, the relative unimportance of property, and the style of personal relationships—it bore the stamp of their own culture. However radical the changes seemed to be, nevertheless the choice of emphases, the points in the culture which they decided were crucial, were selected in terms of existing cultural perceptions. The new culture was a Manus version of the old Manus culture, preserving beneath the apparent extreme change, a recognizable pattern in which such matters as inability to deal with sex, extreme emphasis on masculine values and father-child ties, a strong sense that conduct and disease were inter-related, a preoccupation with anger and autonomy, were all still there. In a “PTA” meeting, evoked by the school boys running away in order to create an incident which would make the community face troublesome issues as to the relative powers of teacher and parents, the parents challenged their children and the children answered—a modern, “democratic” version of the old style of interchange between parents and children. The culture had in fact been transformed into another culture, rather than having suffered from extreme changes in some parts, no changes or partial changes in others, of the type which accompany slower and less conscious social change. The new social forms, the new political forms bore a recognizable relationship to the old forms when considered as patterns, just as the new language,

Neo-Melanesian, based on Melanesian grammar and imported, mainly English vocabulary, bore a strict structural relationship to the Manus language, so that people could shift from one to the other without anyone noticing it, or could speak with any mixture—although it was in Neo-Melanesian that the new concepts could be expressed with such words as *thought, idea, meaning, government, council*, etc.

Because the change had been so complete and so fast, there had been less time for people to develop resistances or adaptive rejections of the new. In terms of Leon Festinger's old experiment, no one was given a chance to get to hate paper cups by being asked to drink some old familiar drink in them, the new kind of cup and the new kind of drink were presented together, mutually reinforcing a new cup-drink relationship, which was however recognizably a substitution for an earlier container-liquid relationship.

And the change had not only been self-initiated, fast and total, but the group of people who had made the change had made it all together, three generations moving in step, grandparents as faithful attendants at the new political ceremonies as grandchildren. The familiar pattern of human relationships, in families, in the village, was still there. Everybody had their own grandmothers to treat in the new style, not strange, unknown old women. Men kept their sons even while the style of fatherhood was altered so that a man, who would once have kept his son a sulky servitor in the back of his house, now surrendered his house to his son when the son married; but it was the same father and the same son—face and figure, voice and gesture, remained as the sustaining stuff of human life.

IV.

The results lead in many directions, but today I wish to go only in one. The fate of this little group of people—only a bare five thousand—who have so boldly attempted to remodel their lives, is one with the fate of their society. Their new personalities are dependent upon their new sense of ethnic identity and their continuance as a group. This in turn depends on world events, the price of wool, the price of copra, the continued independence of Indonesia from Communist domination. What happens to the Territory of New Guinea and the hundreds of small native groups struggling to make new adjustments will occur in terms of world struggles far beyond their control.

But the application to problems of education and research of what we can learn from their effort—whether that effort is fated to succeed or fail—is something which does lie within our powers as a group of students

of human behavior in relation to social issues. It is possible for us so to educate children that in their expectations, in their capacities, they will be able to move as the Manus have moved, capable of developing what might appear to be a culture as sharply contrasting with the culture of the present day, as is Manus culture in 1953 in contrast with Manus culture of 1928. The Manus of 1928 displayed two characteristics which seem significant. First, their children developed potential preferences for values which the culture did not implement, so generations of children grew up valuing friendship, sharing, group effort, only to be turned by a system which could not use such behaviors into grasping, exploitive, materialistic adults. Second, the Manus of 1928 knew that a culture is a way in which an identified group of people conducted their lives, they knew that different groups had different cultures, they knew that cultures could be changed. They understood this as very few members of our own society yet understand it, and this gave them part of their ability to change without hurt and, in that change, to use the potentiality of their childhood education and to install the values which previously had been violated in adult life as the ruling ethic of their new community. Can we develop a type of education within which children will be given precursors of possible changes—in such a way that the discontinuity is not so great that some vision of a more felicitous way of life does not make them strangers within their own culture—and which will give them the ability to seize upon or develop new forms of culture when they become possible?

We lack, at present, any theoretical structure which shows us how to make such moves. Anthropological research can provide suggestions for psychologists, can produce some of the hypotheses necessary for a field theory of life, but only part. We can suggest, for example, that it may be very useful for psychologists to take language as a model, and to see each natural language as a system which matches the existing diversities of human temperament and endowment so that, because every natural language can be learned by every normally endowed child, every other natural language is accessible to every normally endowed adult who has already learned one language. Like language, all the rest of a human culture must be so constructed that every normally endowed human being can become a human being within it. All human cultures are, in this sense, interchangeable, and a man who has learned one as a child should be able to learn another, or more than one, as an adult. The second learning should be different in kind, just as the learning of a second language is radically different from learning to speak, and as learning to read and write a second language are very different from learning to read and write. From this point of view there is no reason why the essential core of a human being should alter as he changes culture, any more than a musical theme should be basically altered when it is played in a different key. As civilization

has progressed, our conceptions of music have become ever more complicated but no matter how complex and lovely it becomes, the music of every people must be grounded in the capacity of the human ear to hear, of the human brain to imagine, and of the human hand and voice to execute. Although the gap between the music of a savage tribe and the music of 19th-century Germany is such that the savage could not, without a transforming education, appreciate the complexity of Beethoven, nor could the German, without a transforming education, appreciate as does an Eskimo an Eskimo song, yet, because both kinds of music have been developed by human beings, both are essentially graspable by human beings, provided the formula of transformation is present.

The study of the way in which people learn new languages again gives us a model. The first essential is to believe that all languages are alike *in kind*, that there is no basic difference between one's own language and others, that one's own language is one of many comparable ones, and that oneself, having learned one language, now knows how to learn languages. The Manus had believed, before they encountered European culture, that culture was something that varied, and that people could borrow the customs of others or alter their own. From Christianity they learned that all men could be regarded as brothers, and that therefore what white men did, they also could do. This, on a wider cultural scale, is equivalent to learning that a new language is a dialect of a language already spoken and so quickly intelligible, rather than a completely strange language and so—to those who have learned only one language—initially completely unintelligible. We need to learn to see all the ways of life that human beings have developed, are living within, or may develop, as learnable by human beings, and that without loss of continuity of personal identity. This is the major premise out of which we could begin to create the learning situation within which each generation of children would learn to be transformable.

V.

These are very broad outlines that I have been sketching in, hypotheses drawn from a single carefully observed and very unusual historical experiment, placed in the context of what we know of culture, of cultural character, of temperament, of personality as a whole. Each detail needs experimental exploration. What are the implications, when translated into terms which can be handled in the laboratory, of making a rearrangement of a system oneself, rather than accepting a rearrangement second-hand? Can an individual, or a group, so place the stamp of their previous

learning on the new material, as to make it immediately assimilable? What are the differences between making small alterations, one at a time or in different parts of a system, and making a complete change from one system to a completely new system? Is the ease of transition, the completeness with which the new learning is mastered, a function of the way the first learning occurred, the premises on which the first learning was based, the completeness of the first learning? If, when one starts to learn how to drive, one learns about the first car as one among many, will one learn to drive other cars more quickly? And what are the relationships in the speed and completeness of forgetting or retaining in dead storage of one or more of several comparable systems? These are all problems for the experimental social psychologist. Each needs careful laboratory exploration and checking, each may be expected to change under the scrutiny of a gifted experimentalist, an experimentalist who, in the tradition of Kurt Lewin, can design experiments which are hypothesis-producing as well as hypothesis-testing.

There are other lines of research which need to be followed up. When I say that every natural language can be spoken by every normal human being, provided he is born to it—that is, is born among the social expectancies that he will of course learn—I include in that statement a hypothesis that every natural language makes provision for a variety of human gifts, and so is in a sense redundant from the standpoint of any single human being, that meaning is embodied at several levels, at the phonemic, the morphemic, the syntactical, the textlet and the discourse level, that declension and conjugation and word order repeat, in varying ways, the same clues for the variously gifted, for the more or less concrete-minded, for those with better and poorer memories, for those who depend more upon auditory images and those who depend more upon kinaesthesia. One of the questions which Kurt was asking insistently in the years just before his death was: How can we make group products exceed the individual capacities of any member of the group? No one individual has ever designed consciously a language which compares with a natural language in its ability to satisfy different kinds of human beings in intercommunication with one another, just because a natural language does exceed the creative abilities of any single person in its redundancies—for it is just at this point of redundancy that the single creativeness fails. Explorations of group creativity must include explorations of the variety of human gifts which are orchestrated in any such group operation—whether the consciously planned small experimental group, or the human society which has, over time, designed houses in which man can live together, a diet of patterned proportions from a mass of raw food materials, a style of life within which children can become adults, no matter how much they

differ in temperament, in intelligence, in physique, from their natural parents. We may fairly assume that no society has done this perfectly, that probably no society ever will, but we can gain increased knowledge of men's innate diversity, of the way in which these diversities are orchestrated to produce cultures which can be embodied in all temperaments, and from which, out of the slight stress and strain on those individuals where the fit is not perfect, new patterns – from their private worlds – will emerge, which must again be reworked, as one single new lyric theme can be taken and orchestrated for many instruments.

At present we know very little about these diversities, especially do we know very little about qualitative and discontinuous diversities, of what constitutes the differences between those people with concrete, time-space-oriented, detailed, reproductive memories and those who reduce all experience to abstraction and forget both names and faces and have to be told whether it is May or November. We note, but have not explored, the differences between those who are alert and attentive on waking and those whose faculties come into play only after many hours of partial wakefulness, between those who can understand others only by complete identification – as persons – or by sympathetic reverberation to the same emotion, or by empathy without identification or sympathy. Is the difference in reliance on different sensory modalities an inherited difference or a matter of early experience, dependent at least in part on the emphases of the culture? And how much is the pattern of such capacities related to somatotype, so that individuals can use clues from physique as clues to compatibilities and incompatibilities, so that selection of a culturally ideal physique may become a molding device for the way in which many other human abilities are patterned?

Working on cultural materials, on a carefully recorded kinship system of 25 years ago and a recorded kinship system now, or with the ethical ideas of 25 years ago and the ethical ideas of the present, the anthropologist, re-studying a society like Manus, can match one pattern with the other, and can suggest that a knowledge of how one pattern is related to the other in terms of human capacities would be illuminating. The next steps have to be taken in the laboratory, especially in the type of laboratory which Kurt used, in which groups – small, chosen and controlled because adequately and fully observed, controlled in fact through knowledge – work on problems which mean something to them. In such research, whether conducted by anthropologist or social psychologist, the trust of the investigator in the productivity of properly observed reality is matched by the yield of new insights which can be so developed.

REFERENCES

1. Bavelas, Alex. "Communication Patterns in Task-Oriented Groups." In *The Policy Sciences*, eds. Daniel Lerner and Harold D. Lasswell. Stanford: University Press, 1951, pp. 193-202.
2. Benedict, Ruth. "Continuities and Discontinuities in Cultural Conditioning," *Psychiatry*, 1938, 1(2), 161-167.
3. Mead, Margaret. *The Changing Culture of an Indian Tribe*. New York: Columbia University Press, 1932.
4. Murphy, Gardner. *Human Potentialities*. *Journal of Social Issues Supplement Series*, 1953, No. 7 (Kurt Lewin Memorial Award Issue), pp. 1-19.