

American SOCIOLOGICAL Review

February
1950

Volume 15
Number 1

The Official Journal of the American Sociological Society



THE PROSPECTS OF SOCIOLOGICAL THEORY*

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TWO YEARS ago at the annual meeting of this Society it was my privilege to act as chairman of the section on theory and thus to be responsible for a statement of its contemporary position, as part of the general stock-taking of the state of our discipline which was the keynote of that meeting. As that meeting was primarily concerned with taking stock of where we stood, the present one, with the keynote of frontiers of research, is primarily concerned with looking toward the future. It therefore seems appropriate to take advantage of the present occasion to speak of the future prospects of that aspect of sociological science on which more than any other I feel qualified to speak.

The history of science testifies eloquently to the fundamental importance of the state of its theory to any scientific field. Theory is only one of several ingredients which must go into the total brew, but for progress beyond certain levels it is an indispensable one. Social scientists are plagued by the problems of objectivity in the face of tendencies to value-bias to a much higher degree than is true of natural scientists. In addition, we have the problem of selection

among an enormous number of possible variables. For both these reasons, it may be argued that perhaps theory is even more important in our field than in the natural sciences. At any rate, I hope I may presume to suggest that my own election to its presidency by the membership of this society may be interpreted as an act of recognition of this importance of theory, and a vote of confidence in its future development.

Though my primary concern this evening is with the future, perhaps just a word on where we stand at present is in order. Some fifteen years ago two young Americans, who, since they were my own children, I knew quite intimately, and who were aged approximately five and three respectively at the time, developed a little game of yelling at the top of their voices: "The sociology is about to begin, said the man with the loud speaker." However right they may have been about their father's professional achievements up to that time, as delivering a judgment of the state of the field as a whole I think they were a bit on the conservative side. It had already begun, but especially in the theoretical phase that beginning did not lie very far back. The historians of our discipline will have to settle such questions at a future time, but I for

*The Presidential Address read before the annual meeting of the American Sociological Society held in New York, December 28-30, 1949.

one would not hesitate to label *all* the theoretical endeavors before the generation of Durkheim and Max Weber as proto-sociology. With these figures as the outstanding ones, but with several others including a number of Americans like Sumner, Park, Cooley, and Thomas, in a somewhat less prominent role, I feel that the real job of founding was done in the generation from about 1890 to 1920. We belong to the second generation, which already has foundations on which to build. But as for the building itself, a post here and there, and a few courses of bricks at the corners, are all that is yet visible above the ground. After all, two or, more correctly, one and a half generations, in the perspective of the development of a science, is a very short time.

When, roughly a quarter of a century ago, I attained some degree of the knowledge of good and evil in a professional sense, this founding phase was over. The speculative systems were still taken seriously. But the work of such writers as Sumner, Thomas, Simmel, Cooley, Park, and Mead, was beginning to enter into thinking in a much more particularized sense. In fact, a research tradition was already building up, in which a good deal of solid theory was embodied—as in Sumner's basic idea of the relativity of the mores, Thomas' four wishes, and many of Park's insights, as into the nature of competitive processes. This relatively particularized, attention focussing, problem selecting, use of theory in research, so different from the purely illustrative relation between theory and empirical fact in the Spencerian type of system, has continued to develop in the interim. Such fields as that of Industrial Sociology, starting from the Mayo-Roethlisberger work, and carried further at Chicago and Cornell, the study of Ethnic Relations and that of Social Stratification will serve to illustrate. At the same time controversies about total schools, which in my youth centered especially about Behaviorism, have greatly subsided.

Our own generation has seen at least the beginnings of a process of more general pulling together. Even when a good deal of

theory was actually being used in research much of the *teaching* of theory was still in terms of the "systems" of the past, and was organized about names rather than working conceptual schemes. Graduate students frantically memorized the contents of Bogardus or Lichtenberger with little or no effect on their future research operations, and little guidance as to how it might be used. But this has gradually been changing. Theory has at least begun no longer to mean mainly a knowledge of "doctrines," but what matters far more, a set of patterns for habitual thinking. This change has, in my opinion, been considerably promoted by increased interest in more general theory, especially coming from study of the works of Weber and Durkheim and, though not so immediately sociological, of Freud. There has thus been the beginning at least, and to me a very encouraging beginning, of a process of coalescence of these types of more or less explicit theory which were really integrated importantly with research, into a more general theoretical tradition of some sophistication, really *the* tradition of a working professional group.

Compared to the natural sciences the amount of genuine empirical research done in our field is very modest indeed. Even so, it has been fairly substantial. But the most disappointing single thing about it has been the degree to which the results of this work have failed to be cumulative. The limitations of empirical research methods, limitations which are being overcome at a goodly rate, are in part responsible for this fact. But *probably the most crucial factor* has been precisely this lack of an adequate *working* theoretical tradition which is bred into the "bones" of empirical researchers themselves, so that "instinctively" the problems they work on, the hypotheses they frame and test, are such that the results, positive or negative, will have *significance* for a sufficiently generalized and integrated body of knowledge so that the mutual implications of many empirical studies will *play directly into each other*. There are, as I have noted, hopeful signs which point in this direction,

but the responsibility on theory to promote this process is heavy indeed. So important is this point that I should like to have the view of the future role of theory in sociology, which I shall discuss in the remainder of this address, understood very largely in relation to it.

When, then, I turn to the discussion of the prospects of theory in our field I can hardly fail to express my own hope as well as a diagnosis. I hope to combine in my suggestions both a sense of the strategic significance of certain types of development, and a realistic sense of feasibility, if sufficient work by able people is done. I shall also be talking of the relatively near future, since the shape of our science two centuries hence, for instance, cannot, I fear, be realistically foreseen.

Here I should like to discuss five principal types or fields of theoretical development, which are by no means independent of one another; they actually overlap considerably as well as interact. They are:

- 1) General theory, which I interpret primarily as the theory of the social system in its sociologically relevant aspects.
- 2) The theory of motivation of social behavior and its bearing on the dynamic problems of social systems, its bearing both on the conditions of stability of social systems and the factors in their structural change. This of course involves the relations to the psychological level of analysis of personality and motivation.
- 3) The theoretical bases of systematic comparative analysis of social structures on the various levels. This particularly involves the articulation with the anthropological analysis of culture.
- 4) Special theories around particular empirical problem areas, the specific growing points of the field in empirical research. This involves their relations to general theory, and the bases of hypothesis construction in research.
- 5) Last, but in no sense least, the "fitting" of theory to operational procedures of

research and, vice versa, the adaption of the latter to theoretical needs.

The field of general theory presents peculiar difficulties of assessment in sociology. The era of what I have above called "protosociology" was, as I have noted, conspicuous for the prominence of speculative systems, of which that of Spencer is an adequate example. The strong and largely justified reaction against such systems combined with a general climate of opinion favorable to pragmatic empiricism, served to create in many quarters a very general scepticism of theory, particularly anything that called itself general or systematic theory, to say nothing of a *system* of theory. This wave of anti-theoretical empiricism has, I think fortunately, greatly subsided, but there is still marked reluctance to recognize the importance of high levels of generality. The most important recent expression of this latter sentiment, which in no sense should be confused with general opposition to theory, is that of my highly esteemed friend and former student, Robert Merton, first in his discussion paper directed to my own paper on the *Position of Sociological Theory*, two years ago, then repeated and amplified in the Introduction to his recent volume of essays.

The very first point must be the emphatic statement that what I mean by the place of general theory in the prospects of sociology is *not* the revival of speculative systems of the Spencerian type, and I feel that Merton's fears that this will be the result of the emphasis I have in mind are groundless. We have, I think, now progressed to a level of methodological sophistication adequate to protect ourselves against this pitfall.

The basic reason why general theory is so important is that the cumulative development of knowledge in a scientific field is a function of the degree of *generality of implications* by which it is possible to relate findings, interpretations, and hypotheses on different levels and in different specific empirical fields to each other. If there is to be a high degree of such generality there *must* on some level be a common conceptual

scheme which makes the work of different investigators in a specific sub-field and those in different sub-fields commensurable.

The essential difficulty with the speculative systems has been their *premature closure* without the requisite theoretical clarification and integration, operational techniques or empirical evidence. This forced them to use empirical materials in a purely illustrative way without systematic verification of *general* propositions or the possibility of empirical evidence leading to modification of the theory. Put a little differently, they presumed to set up a theoretical system instead of a systematic conceptual scheme.

It seems quite clear, that in the sense of mechanics a *theoretical system* is *not* now or foreseeably possible in the sociological field. The difficulties Pareto's attempt encountered indicate that. But a *conceptual scheme* in a partially articulated form exists now and is for practical purposes in common use; its further refinement and development is imperative for the welfare of our field, and is entirely feasible.

In order to make clear what I mean, I would first like to note that there is a variety of ways in which what I am calling general theory can fruitfully influence research in the direction of making its results more cumulative. The first is what may be called a set of general categories of orientation to observation and problem choice in the field which defines its major problem areas and the directions in which to look for concealed factors and variables in explanation. Thus modern anthropology, by the "cultural point of view," heavily documented with comparative material, has clearly demonstrated the limits of purely biological explanations of human behavior and taught us to look to the processes by which culturally patterned modes are learned, transmitted and created. Similarly in our own field the reorientation particularly associated with the names of Durkheim and Weber showed the inadequacy of the "utilitarian" framework for the understanding of many social phenomena and made us look to "institutional" levels—a reorientation which is indeed the birthright of sociology. Finally, in the field

of motivation, the influence of Freud's perspective has been immense.

Starting from such very broad orientation perspectives there are varying possible degrees of further specification. At any rate in a field like ours it seems impossible to stop there. The very basis on which the utilitarian framework was seen to be *theoretically* as well as empirically inadequate, required a clarification of the structure of systems of social action which went considerably farther than just indicating a new direction of interest or significance. It spelled out certain inherent relationships of the components of such systems which among many other things demonstrated the *need* for a theory of motivation on the psychological level of the general character of what Freud has provided.

This kind of structural "spelling out" narrows the range of theoretical arbitrariness. There are firmly specific points in the system of implications against which empirical results can be measured and evaluated. That is where a well-structured empirical problem is formulated. If the facts then, when properly stated and validated, turn out to be contrary to the theoretical expectation, something must be modified in the theory.

In the early stages these "islands" of theoretical implication may be scattered far apart on the sea of fact and so vaguely and generally seen that only relatively broad empirical statements are directly relevant to them. This is true of the interpretation of economic motivation which I will cite presently. But with refinement of general theoretical analysis, and the accumulation of empirical evidence directly relevant to it, the islands get closer and closer together, and their topography becomes more sharply defined. It becomes more and more difficult and unnecessary to navigate in the uncharted waters of unanalyzed fact without bumping into or at least orienting to several of them.

The development of general theory in this sense is a matter of degree. But *in proportion* as it develops, the generality of implication increases and the "degree of empiri-

cism," to quote a phrase of President Conant's, is reduced. It is precisely the existence of such a general theoretical framework, the more so the further it has developed, which makes the kind of work at the middle theory level which Merton advocates maximally fruitful. For it is by virtue of their connections with these "islands" of general theoretical knowledge once demonstrated that their overlaps and their mutual implications for each other lead to their incorporation into a more general and consistent body of knowledge.

At the *end* of this road of increasing frequency and specificity of the islands of theoretical knowledge lies the ideal state, scientifically speaking, where *most* actual operational hypotheses of empirical research are directly derived from a general system of theory. On any broad front, to my knowledge, only in physics has this state been attained in *any* science. *We* cannot expect to be anywhere nearly in sight of it. But it does not follow that, distant as we are from that goal, steps in that *direction* are futile. Quite the contrary, *any* real step in that direction is an advance. Only at this *end* point do the islands merge into a continental land mass.

At the very least, then, general theory can provide a broadly orienting framework. It can also help to provide a common language to facilitate communication between workers in different branches of the field. It can serve to codify, interrelate and make available a vast amount of existing empirical knowledge. It also serves to call attention to gaps in our knowledge, and to provide canons for the criticism of theories and empirical generalizations. Finally, even if they cannot be systematically derived, it is indispensable to the systematic clarification of problems and the fruitful formulation of hypotheses. It is this organizing power of generalized theory even on its present levels which has made it possible for even a student like myself, who has done only a little actual empirical research, to illuminate a good many empirical problems and formulate suggestive hypotheses in several fields.

Though it is not possible to take time to

discuss them adequately for those not already familiar with the fields, I should like to cite two examples from my own experience. The first is the reorientation of thinking about the field of the motivation of economic activity. The heritage of the classical economics and the utilitarian frame of reference, integrated with the central ideology of our society, had put the problem of the "incentives" involved in the "profit system" in a very particular way which had become the object of much controversy. Application of the emerging general theory of the institutionalization of motivation, specifically pointed up by the analysis of the contrast between the orientation of the professional groups and that of the business world, made it possible to work out a very fruitful reorientation to this range of problems. This new view eliminates the alleged absoluteness of the orientation to "self-interest" held to be inherent in "human nature." It emphasizes the crucial role of institutional definitions of the situation and the ways in which they channel many different components of a total motivation system into the path of conformity with institutionalized expectations. Without the general theoretical reorientation stemming mainly from Durkheim and Weber, this restructuring of the problem of economic motivation would not have been possible.

The second example illustrates the procedure by which it has become possible to make use of psychological knowledge in analyzing social phenomena without resort to certain kinds of "psychological interpretations" of the type which most sociologists have quite correctly repudiated. Such a phenomenon is the American "youth culture" with its rebellion against adult standards and control, its compulsive conformity within the peer group, its romanticism and its irresponsibility. Structural analysis of the American family system as the primary field of socialization of the child provides the primary setting. This in turn must be seen both in the perspective of the comparative variability of kinship structures and of the articulation of the family with other elements of our own social structure,

notably the occupational role of the father. Only when this structural setting has been carefully analyzed in sociological terms does it become safe to bring in analysis of the operation of psychological mechanisms in terms derived particularly from psychoanalytic theory, and to make such statements as that the "revolt of youth" contains typically an element of reaction-formation against dependency needs with certain types of consequences. Again this type of analysis would not have been possible without the general reorientation of thinking about the relations between social structure and the psychological aspects of behavior which has resulted from the developments in general theory in the last generation or more; including explicit use of the contributions of Freud.

Perhaps I may pause in midpassage to apologize for inflicting on you on such an occasion, when your well-filled stomachs predispose you to relaxation rather than close attention, such an abstruse theoretical discourse. I feel the apology is necessary since what I am about to inflict on you is even more abstruse than what has gone before. Since I am emphasizing the integration of theory with empirical research, I might suggest that someone among you might want to undertake a little research project to determine the impact on a well-fed group of sociologists of such a discourse. I might suggest the following four categories for his classification.

- 1) Those who have understood what I have said, whether they approve of it or not.
- 2) Those who *think* they have understood it.
- 3) Those who do not think they have but *wish* they had, and
- 4) Those who didn't understand, know it and are glad of it.

I can only hope that the overwhelming majority will not be found to fall in the fourth category.

With relatively little alteration, everything I have said up to this point had been written, and has deliberately been left stand-

ing, when I underwent an important personal experience which produced what I hope will prove to be a significant theoretical advance precisely in the field of general theory. With the very able collaboration of several of my own Harvard colleagues and of Professors Tolman of California and Shils of Chicago, the present semester has been devoted to attempting to practice what I have preached, namely to press forward with systematic work in the field of general theory. Partly because of the intrinsic importance of the fields, partly because of its urgency in a department committed to the synthesis of sociology with parts of psychology and anthropology, we have been devoting our principal energies to the *inter-*relations and common ground of the three branches of the larger field of social relations.

This new development, which is still too new for anything like adequate assessment, seems to consist essentially in a method of considerably increasing the number of theoretically known islands in the sea of social phenomena and thereby narrowing the stretches of uncharted water between them. The essential new insight, which unfortunately is not easy to state, concerns the most general aspects of the conception of the components of systems of social action and their relations to each other.

It seems to have been the previous assumption, largely implicit, for instance, in the thinking of Weber, of W. I. Thomas, and in my own, that there was, as it were, *one* "action-equation." The actor was placed on one side—"oriented to" a situation or a world of objects which constituted the other side. The difficulty concerned the status of "values" in action, not as the motivational *act* of "evaluation" of an object, but as the *standard* by which it was evaluated—in short, the concept "value-attitudes" which some of you will remember from my *Structure of Social Action*. I, following Weber, had tended to put value-standards or modes of value-orientation into the actor. Thomas and Znaniecki in their basic distinction between attitudes and values had put them into the object-system.

We have all long been aware that there were three main problem foci in the most general theory of human behavior which we may most generally call those of personality, of culture, and of social structure. But in spite of this awareness, I think we have tended to follow the biological model of thought—an organism and its environment, an actor and his situations. We have not *really* treated culture as independent, or if that has been done, as by some anthropologists, the tendency has been for them in turn to absorb either personality or social structure *into* culture, especially the latter, to the great discomfort of many sociologists. What we have done, which I wish to report is, I think, to take an important step toward drawing out for *working* theory the implications of the fundamental fact that *man is a culture-bearing animal*.

Our conclusion then is that value-standards or modes of value-orientation should be treated as a *distinct* range of components of action. In the older view the basic components could be set forth in a single "table" by classifying the modes of action or motivational orientation which we have found it convenient to distinguish as cognitive mapping (in Tolman's sense), cathetic (in the psychoanalytic sense) and evaluative, against a classification of the significant aspects or modalities of objects. These latter we have classified as quality complexes or attributes of persons and collectivities, action or performance complexes, and non-human environmental factors. By adding values as a fourth column to this classification, this had seemed to yield an adequate paradigm for the structural components of action-systems.

But something about this paradigm did not quite "click." It almost suddenly occurred to us to "pull" the value-element out and put it into a separate range, with a classification of its own into three modes of value-orientation: cognitive (in the *standard*, not content, sense), appreciative and moral. This gave us a paradigm of *three* "dimensions" in which *each* of the three ranges or sets of modes is classified against *each* of the other two.

This transformation opened up new possibilities of logical development and elaboration which are much too complex and technical to enter into here. Indeed the implications are as yet only very incompletely worked out or critically evaluated and it will be many months before they are in shape for publication. But certain of them are sufficiently clear to give *me* at any rate the conviction that they are of considerable importance, and taken together, will constitute a substantial further step in the direction of unifying our theoretical knowledge and broadening the range of generality of implication, with the probable consequence of contributing substantially to the cumulativeness of our empirical research.

Certain of these implications, which in broad outline already seem clear, touch two of the subjects on which I intended to speak anyway and can, I think, now speak much better. The first of these is the very fundamental one of the connection of the theories of motivation and personality structure on the psychological level with the sociological analysis of social structure. The vital importance of this connection is evident to all of us, and many sociologists have been working away at the field for a long time. Seen in the perspective of the years, I think great progress has been made. The kind of impasse where "psychology is psychology" and "sociology is sociology" and "never the twain shall meet," which was a far from uncommon feeling in the early stages of my career, has almost evaporated. There is a rapidly increasing and broadening area of mutual supplementation.

What has happened in our group opens up, I think, a way to eliminating the sources of some of the remaining theoretical difficulties in this field, and still more important, building the foundations for establishing more direct and specific connections than we have hitherto been able to attain. I should like to indicate some of these in two fields.

The first is the less radical. We have long suspected, indeed on some level, known, that the basic structure of the human personality was intimately involved with the social

structure as well as vice versa. Indeed some have gone so far as to consider personality to be a direct "microcosm" of the society. Now, however, we have begun to achieve a considerable clarification of the bases on which this intimacy of involvement rests, and to bring personality, conceptually as well as genetically, into relation with social structure. It goes back essentially to the insight that the major axis around which the expectation-system of any personality becomes organized in the process of socialization is its *interlocking* with the expectation-systems of others, so that the mutuality of socially structured relationship patterns can no longer be thought of as a *resultant* of the motivation-systems of a plurality of actors, but becomes directly and fundamentally *constitutive* of those motivation systems. It has seemed to us possible in terms of this reoriented conception to bring large parts both of Tolman's type of behavior theory and the psychoanalytic type of theory of personality, including such related versions as that of Murray, together in a close relation to sociological theory. Perhaps the farthest we had dared to go before was to say something like that we considered social structure and personality were very closely related and intimately *interacting* systems of human action. Now I think it will probably prove safe to say that they are in a theoretical sense different phases or aspects of the *same* fundamental action-system. This does not in the least mean, I hasten to add, that personality is in danger of being "absorbed" into the social system, as one version of Durkheim's theory seemed to indicate. The distinction between the personality "level" of the organization of action and the social system level remains as vital as it ever was. But the *theoretical* continuity, and hence the possibility of using psychological theory in the motivation field for sociological explanation, have been greatly enhanced.

The second point I had in mind is essentially an extension of this one or an application of it. As those of you familiar with some of my own writing since the *Structure of Social Action* know, for some years I

have been "playing" with a scheme of what I have found it convenient to call "pattern variables" in the field of social structure, which were originally derived by an analytical breakdown of Toennies' *Gemeinschaft-Gesellschaft* pair into what seemed to be more elementary components. This yielded such distinctions as that between universalism, as illustrated in technical competence or the "rule of law," and particularism as given in kinship or friendship relations, or to take another case, between the "functional specificity" of an economic exchange relationship and the "functional diffuseness" of marriage. Thus to take an illustration from my own work, the judgment of his technical competence on which the choice of a physician is supposed to rest is a universalistic criterion. Deviantly from the ideal pattern, however, some people choose a physician because he is Mary Smith's brother-in-law. This would be a particularistic criterion. Similarly the basis on which a physician may validate his claim to confidential information about his patient's private life is that it is necessary if he is to perform the specific function of caring for the patient's health. But the basis of a wife's claim to a truthful answer to the question "what were you doing last night that kept you out till three in the morning?" is the generally diffuse obligation of loyalty in the marriage relationship.

Again I cannot take time to go into the technicalities. But the theoretical development of which I have spoken has already indicated two significant results. First it has brought a scheme of five such pattern variables—the four I had been using, with the addition of the distinction of ascription and achievement which Linton first introduced into our conceptual armory—into a direct and fundamental relation to the structure of action systems themselves. These concepts can now be systematically derived from the basic frame of reference of action theory, which was not previously possible.

Secondly, however, it appears that the same basic distinctions, which were all worked out for the analysis of *social* structure, can, when rephrased in accord with

psychological perspective, be identified as fundamental points of reference for the structuring of personality also. Thus what sociologically is called universalism in a social role definition can be psychologically interpreted as the impact of the mechanism of generalization in object-orientation and object choice. Correspondingly, what on the sociological level has been called the institutionalization of "affective neutrality" turns out to be essentially the same as the imposition of renunciation of immediate gratification in the interests of the disciplined organization and longer-run goals of the personality.

If this correspondence holds up, and I feel confident that it will, its implications for social science may be far reaching. For what these variables do on the personality level is to serve as foci for the structuring of the system of predispositions or needs. But it is precisely this aspect of psychological theory which is of most importance for the sociologist since it yields the *differentiations* of motivational orientation which are crucial to the understanding of socially structured behavior. *Empirically* we have known a good deal about these differentiations, but *theoretically* we have not been able to connect them up in a systematically generalized way. It looks as though an important step in this direction had now become possible. With regard to its potential importance, I may only mention the extent to which studies of the distribution of attitudes have come to occupy a central place in the empirical work both of sociologists and of social psychologists. The connection of these distribution data with the social structure on the one hand and the structure of motivational predispositions on the other has had to a high degree to be treated in empirically *ad hoc* terms. Any step in the direction of "reducing the degree of empiricism" in such an area will constitute a substantial scientific advance. I think it is probable that such an advance is in sight, which, if validated, will have developed from work in *general* theory.

Let us now turn to the other major theoretical field, the systematization of the bases

for *comparative* analysis of social structures. First I should like to call attention to the acute embarrassment we have had to suffer in this field. On the level of what I have made bold to call "proto-sociology" it was thought that this problem was solved by the implications of the evolutionary formulæ which arranged all possible structural types in a neat evolutionary series which *ipso facto* established both their comparability and their dynamic relationships. Unfortunately, from one point of view, this synthesis turned out to be premature; but from another this was fortunate, for in one sense the realization of this fact was the starting point of the transition from proto-sociology to real sociology. At any rate, in spite of the magnificence of Max Weber's attempt, the basic classificatory problem, the solution of which must underlie the achievement of high theoretical generality in much of our field, has remained basically unsolved.

As so often happens there has been a good deal of underground ferment going on in such a field before the results have begun to become widely visible. There are, I think, signs of important progress. One of these is the great step toward the systematization of the variability of kinship structure which our anthropological colleague, Professor Murdock, has reported in his recent book. For one critically important structural field we can now say that many of the basic problems have been solved. But this still leaves much to be worked out, particularly in the fields of more complex institutional variability in the literate societies, in such areas as occupation, religion, formal organization, social stratification and government.

Just as in the problem of the motivation of socially structured behavior our relations to psychology become peculiarly crucial and intimate, so in that of systematizing the structural variability of social systems, our relations to anthropology are correspondingly crucial. This, of course, is because of the ways in which the basic cultural orientations underlie and interpenetrate the structuring of social systems on the action level. Anything, therefore, which can help to clarify the most fundamental problems of

the ways in which values and other cultural orientation elements are involved in action systems should sooner or later contribute to this sociological problem.

In general, anthropological theory in the culture field has in this respect been disappointing, not that it has not provided many empirical insights, which it certainly has, but precisely in terms of the present interest in systematization. I am happy to report that my colleague, Dr. Florence Kluckhohn has, in yet unpublished work, made some promising suggestions the implications of which will, I think, turn out to be of great importance. In what follows I wish gratefully to acknowledge my debt to her work.

In this connection it is important that the central new theoretical insight to which I have referred above came precisely in this field, in a new view of the way values are related to action. The essence of this is the *analytical* independence of value-orientation relative to the psychological aspects of motivation. It introduces an element of "play" into what had previously been a much more rigid relation, this rigidity having much to do with the unfortunate clash of sociological and anthropological "imperialisms."

The independence of value-orientation encourages the search for elements of structural focus in that area. The "problem areas" of value-choice seem to provide one set of such foci, that is, the evaluation of man's relation to the natural environment, to his biological nature and the like. But along with these there are foci differentiating the alternatives of the basic "directionality" of value-orientation itself. In this connection, it has become possible to see that a fundamental congruence exists between at least one part in the set of "pattern variables" mentioned above, that of universalism and particularism, and Max Weber's distinction, which runs throughout his sociology of religion, between transcendent and immanent orientations, the Western, especially Calvinistic orientation, illustrating the former, the Chinese the latter.

Bringing such a differentiation in relation to basic orientation-foci together with the

problem foci seems to provide at least an initial and tentative basis for working out a systematic classification of some major possibilities of cultural orientation in their relevance to differentiations of social structure. Then through the congruence of these with the possible combinations of the values of pattern variables in the structuring of social roles themselves, it seems possible further to clarify some of the modes of articulation of the variability of cultural orientations with that of the structure of the social systems which are their bearers and, in the processes of culture change, their creators.

In this field even more than that of the relation between social structure and motivation, what I am in a position to give you now is not a report of theoretical work accomplished, but a vision of what *can* be accomplished if the requisite hard and competent work is done. This vision is not, however, I think, mere wishful thinking. I think we have gone far enough so that we can see real possibilities. We are in a position to organize a directed and concerted effort with definite goals, not merely to grope about in the hope that something will come out of it.

It seems to me that the importance of progress in this field of structural analysis which attempts to establish the bases of comparability of social structures can scarcely be exaggerated. I have indeed felt for some time that the fact that we had not been able to go farther in this direction was a more serious barrier to the all-important generality and cumulativeness of our knowledge than was the difficulty of adequately linking the analysis of social structure to psychological levels of the understanding of motivation.

The problem of the importance of structural variability and its analysis is most obvious when we are dealing with the broad structural contrasts between widely differing societies. It is, however, a serious error to suppose that its importance is confined to this level. Every society, seen close to, is to an important degree a *microcosm* of the various possibilities of the structuring of human relationships all over the world and

throughout history. The variability *within* the same society, though subtler and less easy to analyze, is none the less authentic.

Of course in any one society *some* possibilities of structural variability are excluded altogether, or can appear only as radically deviant phenomena. But it must not be assumed that in spite of its conformity to a broad general type, the American middle-class family for instance is, *precisely in terms of social structure*, a uniform cut-and-dried thing. It is a complex of many importantly variant sub-types. For some sociological problems it may be precisely the structural differentiations between and distribution of these sub-types which constitute the most important data. To say merely that these are middle-class families will not solve such problems. But it is not necessary for the sociologist to stop there and resort to "purely psychological" considerations. He can and should push his distinctive type of structural analysis on down to these levels of "minor" variability.

In the present state of knowledge, or that of the foreseeable future, we are bound to a "structural-functional" level of theory. There will continue to be long stretches of open water between our islands of validated theory. In this situation we cannot achieve a high level of dynamic generalization for processes and interdependences even *within* the same society, unless our ranges of structural variability are really systematized so that when we get a shift from one to another we know *what* has changed, *to what* and *in what degree*. This order of systematization can, like all theoretical work, be verified only by empirical research. But experience shows that it cannot be worked out by sheer *ad hoc* empirical induction, letting the facts reveal their own pattern. It must be worked out by rigorous theoretical analysis, continually stimulating and being checked by empirical research. In sum I think this is one of the very few most vital areas for the development of sociological theory, and here as in the other I think the prospects are good.

The above two broad areas of prospective theoretical advance are so close to the most general of general theory that they would

scarcely qualify as falling within the area of "special theories," which was the fourth area about which I wanted to talk. I have precisely taken so much time to discuss these because of their importance for more special theories. I am very far indeed from wishing to disparage the importance of this more special and in one sense more modest type of theoretical work; quite the contrary. It is here that the growing points of theory in their direct working interaction with empirical research are to be found. If the state of affairs at that level cannot be healthy we should indeed despair of our science.

I will go farther. It seems to me precisely that the fact that real working theory at the research levels did not exist and was not developed in connection with them was perhaps the most telling symptom that the "speculative systems" of which I have spoken were only pseudo-scientific, not genuinely so. Most emphatically I wish to say that the general theory on which I have placed such emphasis can *only* be justified in so far as it "spells out" on the research level, providing the more generalized conceptual basis for the frames of reference, problem statements and hypotheses, and many of the operating concepts of research. In these terms it underlies the problem-setting of research, it provides criteria of more generalized significance of the problem and its empirical solution, it provides the basis on which the results of one empirical study become fruitful, not merely in the particular empirical field itself, but beyond it for other fields; that is, for what above I have called its *generality of implication*. In my opinion it is precisely because of its orientation to a sound tradition of general theory, however incomplete and faulty, that the particular theories which are developing so rapidly in many branches of the field are so highly important and promising for the future. Let us, by all means, work most intensively on the middle theory level. That way lies real maturity as a science, and the ultimate test of whether the general theory is any good. And of course many of the most important contributions to general theory will come from this source.

This brings me finally to the fifth point on my agenda, the fitting in of theory with the operational procedures of research. Thus far I have been talking to you about theory, but I was careful to note at the outset that however important an ingredient of the scientific brew theory may be, it is only one of the ingredients. If it is to be *scientific* theory it must be tied in, in the closest possible manner, with the techniques of empirical research by which alone we can come to know whether our theoretical ideas are "really so" or just speculations of peculiar if not disordered minds.

Anyone who has observed the social science scene in this country over the past quarter century cannot fail to be impressed by the very great development of research technique in our field, in very many of its branches. Sampling has come in to make it possible for the social scientist to manufacture his own statistical data, instead of having to work only with the by-products of other interests. Techniques of statistical analysis themselves have undergone an immense amount of refinement, for example, in the development of scaling procedures. An altogether new level has already been attained in the collection and processing of raw data, as through questionnaire and interview, and the development of coding skills and the like. I used to think that the construction of a questionnaire was something any old dub could dream up if he only knew what information he wanted. I have learned better. The whole immense development of interviewing techniques with its range from psychoanalysis to Gallup and Roper lies almost within the time period we are talking about. The possibilities of the use of projective techniques in *sociological* research are definitely exciting. The Cross-Cultural Survey (now rechristened) and Mr. Watson of I.B.M. vie with each other to create more elaborate gadgets for the social scientist to play with. We have even, as in the communications and the small groups fields, begun to get somewhere with relatively rigorous experimental methods in sociology, no longer only in psychology among the sciences of human behavior.

This whole development is, in my opinion, in the larger picture *at least* as important as that of theory. It is, furthermore, exceedingly impressive, not merely for its accomplishments to date, important as these are, but *still more* for its promise for the future. There is a veritable ferment of invention going on in this area which is in the very best American tradition.

If I correctly assess the recipe for a really good brew of social science it is *absolutely imperative* that these two basic ingredients should get together and blend with each other. I do not think it fair to say that we are still in the stage of proto-science. But we are unquestionably in that of a distinctly *immature* science. If it is really to grow up and not regress into either of the two futilities of empiricist sterility or empirically irrelevant speculation, the synthesis must take place. In this as in other respects the beginning certainly has already been made but we must be quite clear that it is *only* a beginning.

This is a point where a division of labor is very much in order. It surely is not reasonable to suppose that all sociologists should become fully qualified specialists in theory and the most highly skilled research technicians at the same time. Some will, indeed must, have high orders of competence on both sides, but this will not be true of all. But the essential is that there should be a *genuine* division of labor. That means that all parties should directly contribute to the effectiveness of the whole. For the theoretical side this imposes an obligation to get together with the best research people and make every effort to make their theory researchable in the highest sense. For the research technician it implies the obligation to fit his operational procedures to the needs of theory as closely as he can.

It has been in the nature of the circumstances and processes of the historical development of theory that much of its empirical relevance has heretofore been made clear and explicit only on the level of "broad" observations of fact which were not checked and elaborated by really technical procedures. The value of this, as for instance

it has appeared in the comparative institutional field, should not be minimized. But clearly this order of empirical validation is *only* a beginning. For opening the doors to much greater progress it is necessary to be able to put the relevant content of theory in terms which the empirical research operator can directly build into his technical operations. This is a major reason why the middle theories are so important, because it is on that level that theory will get *directly* into research techniques and vice versa. Again in this field the beginnings I happen to know about are sufficiently promising so that I think we can say that the prospects are good.

Theory has its justification *only* as part of the larger total of sociological science as a whole. Perhaps in closing I may be permitted a few general remarks about the prospects of sociology as a science. I have great confidence that they are good, a solidier and stronger confidence than at any time in my own professional lifetime, provided of course that the social setting for its development remains reasonably stable and favorable.

These prospects are, however, bound up with the fulfillment of certain internal as well as external conditions. One of the most important of these on which I would like to say a word, is a proper balance between fundamental research, including its theoretical aspect, and applied or "engineering" work. This problem is of course of particular interest to our friends in the Conference on Family Welfare. Both the urgencies of the times and the nature of our American ethos make it unthinkable that social scientists as a professional group should shirk their social responsibilities. They, like the medical profession, must do what they can where they are needed. Indeed it is only on this assumption that they will do so that not only the very considerable financial investment of society in their work, but the interferences in other people's affairs which are inevitably bound up with our research, can be justified.

It is not a question of *whether* we try to live up to our social responsibilities, but of *how*. If we should put the overwhelming

bulk of our resources, especially of trained talent, into immediately practical problems it would do some good, but I have no doubt that it would have to be at the expense of our greater usefulness to society in the future. For it is only by systematic work on problems where the probable *scientific* significance has priority over any immediate possibility of application that the greatest and most rapid scientific advance can be made. And it is in proportion as sociology attains stature as a science, with a highly generalized and integrated body of fundamental knowledge, that practical usefulness far beyond the present levels will become possible. This conclusion follows most directly from the role of theory, as I have tried to outline it above. If the prospects of sociological theory are good, so are, I am convinced, those of sociology as a science, but *only* if the scientifically fundamental work is done. Let us, by all means, not be stingy with the few golden eggs we now have. But let us also breed a flock of geese of the sort that we can hope will lay many more than we have yet dreamed of.

One final word. Like all branches of American culture, the roots of sociology as a science are deep in Europe. Yet I like to think of sociology as in some sense peculiarly an American discipline, or at least an American opportunity. There is no doubt that we have the leadership now. Our very lack of traditionalism perhaps makes it in some ways easier for us than for some others to delve deeply into the mysteries of how human action in society ticks. We certainly have all the makings for developing the technical know-how of research. We are good at organization which is coming to play an increasingly indispensable part in research.

It is my judgment that a great opportunity exists. Things have gone far enough so that it seems likely that sociology, in the closest connection with its sister-sciences of psychology and anthropology, stands near the beginning of one of those important configurations of culture growth which Professor Kroeber has so illuminatingly analyzed. Can American sociology seize this oppor-

tunity? One of our greatest national resources is the capacity to rise to a great challenge once it is put before us.

We can do it if we can put together the right *combination* of ingredients of the brew. Americans as scientists generally have been exceptionally strong on experimental work and empirical research. I have no doubt whatever of the capacity of American sociologists in this respect. But as *theorists* Americans have, relative to Europeans, not been so strong—hence the *special* challenge

of the theoretical development of our field which justifies the theme of this address. If we American sociologists can rise to this part of the challenge the job will really get done. We are not in the habit of listening too carefully to the timid souls who say, why try, it can't be done. I think we have already taken up the challenge all along the line. "The sociology," as my children called it, is not *about* to begin. It has been gathering force for a generation and is now really under way.

A MIDDLEMAN LOOKS AT SOCIAL SCIENCE*

CHARLES DOLLARD

Carnegie Corporation of New York

IN INVITING me to speak to you tonight, your president emphasized the fact that it was the desire of the Executive Committee (and I quote) "to secure someone outside the field who could speak about some of the general problems of the place of sociology in particular and the social sciences in general in our society." At first reading, this characterization of me as an outsider saddened me immeasurably, especially when I recalled that it could be proved on the record that I was enrolled as a part-time graduate student in sociology for about ten years. On reflection, I have come to accept the justice of Mr. Parsons' estimate. I am not a social scientist. Worse still, I see no possibility that I shall ever develop into one. The fault is entirely my own. Some of the ablest men in the guild—Ross, Young, Gillin, McCormick, Linton—labored patiently to make a silk purse out of this particular sow's ear. Alas, their efforts were in vain. Since I left graduate school, younger members of the craft—John Dollard, Samuel Stouffer, Leonard Cottrell, Donald Young, Raymond Bowers, Carl Hovland, Pendleton Herring, my classmate Clyde Kluckhohn, Alexander Leighton and my office partner, John Gard-

ner—have taken up the task with no better success. I am now resigned to the inevitable, and wholly content to be regarded merely as a friend of the social sciences rather than as a member of the guild.

If my behavior over the past ten years has not identified me as a friend, nothing which I can say here tonight will improve my credentials. My strong desire to be accepted as such is explained in part by the fact that some of the things I shall have to say later in the evening will be critical, and it is somewhat easier to take criticism from friends than from enemies.

At this point, I must make a conventional disclaimer. I am not speaking tonight for the Carnegie Corporation of New York. That Corporation is composed of fourteen trustees, who have elected me as the fifteenth to serve at their pleasure and to be their chief representative in the daily conduct of the Corporation's business. These fourteen are very independent persons and anyone who essayed to express their joint judgment on any problem except sin or taxes would be foolish indeed. I can say, however, and now I am speaking also for my colleagues in the staff of the Corporation, that a conviction as to the importance of the social sciences is an implicit condition of my employment. I can say also in all honesty, that

* Address delivered at the annual meeting of the American Sociological Society held in New York, December 28-30, 1949.