

PART I

THE NATURE, STRUCTURE, AND TYPES OF SOCIOLOGICAL THEORY

The five authors whose ideas compose Part 1 are largely engaged in some form of *metatheorizing*. They, in their own ways, theorize about the nature, structure, and types of sociological theory. More specifically, these writers tell what they believe sociological theory should be, what it should look like, and what it should do. The fact that there is little or no agreement concerning their views on sociological theory points to the unsettled condition of sociology as a knowledge field and to the complexity of its subject matter: social reality.

We begin with the essay, “On Intellectual Craftsmanship,” where C. Wright Mills offers beginning students of sociology practical advice on how to stimulate the *sociological imagination*—the quality of mind that will help them use information and develop reason in order to achieve lucid summations of what is going on in the world and of what may be happening within themselves. While Mills is not typically considered a theorist, his recommendations for activating the sociological imagination can nonetheless be helpful in doing theoretical work.

Mills advocates an unbroken continuity between what students of sociology pursue intellectually and what they, as persons, observe and experience in their everyday lives. In other words, the intellectual’s professional activities should always be fused with his or her personal life. Intellectual work may be properly described as a craft. Mills uses the phrase *intellectual craftsmanship* in referring to a style of work, as well as to the joyful experience of mastering the medium—language—used in that work. In order to engage in intellectual craftsmanship, Mills recommends that sociology students keep a “file,” a journal of sorts, in which notes are habitually taken in an effort to join the personal with the professional, to record studies underway, as well as studies planned. The file should consist of a continually growing collection of facts and ideas, from the most vague to the most finished: personal notes, excerpts from books, bibliographical items, outlines of projects, and so forth. At a later point in time, the sociologist rearranges the file by playfully combining previously isolated ideas and notes on different topics and finding unsuspected connections between them. Rearranging the file releases the imagination, as the sociologist becomes receptive to unforeseen and unplanned linkages, all the while keeping in mind the several problems on which he or she is actively working. Then, through the use of ideal types, polar types, and cross-classification techniques, the sociologist attempts to systematically order the findings. On completing this, the findings are then paired down

to essentials by relating them to one another in order to form a working model. Finally, the sociologist relates the working model to whatever he or she is endeavoring to explain.

While Mills was advocating the use of a pragmatic “working” model—a more-or-less systematic inventory of findings that can be used to understand something of social significance—Talcott Parsons was proposing the formulation of a universal conceptual scheme for the social sciences.

In 1949, Parsons promoted the formulation of a social theory that has the most *general* implications possible. In “The Importance of General Theory,” Parsons maintains that the reason for engaging in general theorizing is that the cumulative development of knowledge is based on the degree of abstraction by which different findings and interpretations in the various social sciences can be systematically related to each other. At the time, Parsons was developing a master analytical scheme that would encompass the entire subject matter of anthropology, social psychology, and sociology. He would later articulate this comprehensive “system” theory in his landmark volumes *Toward a General Theory of Action* and *The Social System*.

In “Middle-Range Theories,” Robert K. Merton proposes a distinctly different type of sociological theory from that of Parsons’s general conceptual model, which is far removed from empirical confirmation. For Merton, sociological theorizing should be done at *midrange*—intermediate between an all-inclusive unified theory of social systems and minor and prosaic descriptions of observed data. Accordingly, middle-range theory involves neither broad abstractions nor trivial details; rather, it consists of logically interconnected propositions that can be empirically investigated. Examples of middle-range theories include a theory of reference groups, a theory of relative deprivation, and a theory of role sets. Merton argues that only by developing such specialized theories with limited conceptual ranges and gradually consolidating them will sociology advance its knowledge.

In “Theory as Explanation,” George C. Homans asserts that any science, including sociology, has two main jobs to perform: discovery and explanation. Discovery involves stating and testing general relationships between properties of nature. A discovery takes the form of a *proposition*, or a statement of relationship between properties of nature. But in science, discovery alone isn’t enough, there must also be *explanation*; there has to be a statement saying why, under given conditions, the relationship holds well. In other words, if there is some change in one of the properties—one of the variables—the proposition must specify what the change in the other variable will be, or if one of the variables increases in value, it must say how the other will too. In sum, then, a theory should be an explanation in the form of x varies as y . Thus, for Homans, an explanation of an empirical phenomenon can only be a *theory* of the phenomenon. But how does one arrive at a theory? Simply put, one does so through the method of *deduction*. The proposition to be explained is called the *explicandum*. The *explicandum* is explained in that it follows a logical conclusion, as a deduction, from more general propositions. For Homans, the purpose of sociological theory is to deduce a wide variety of empirical propositions under different given conditions.

In “The Oversocialized View of Human Nature,” Dennis H. Wrong maintains that sociological theory should be an effort to find answers to eternal questions about human nature, such as the so-called Hobbesian problem of order: Why do people conform to institutionalized norms? Talcott Parsons gives the following answer: Because they have, through socialization, internalized the norms. Wrong critiques this implicit “oversocialized” view of human nature for dismissing other characteristics of people who are resistant to socialization—such as their material interests, their sexual drives, and their quest for power—and characteristics that explain their motivations to conform (or, for that matter, not to conform). Sociological theory, says Wrong, must consider people as *social* beings, without treating them as entirely *socialized* beings.

Finally, in “The Theoretical Infrastructure,” Alvin W. Gouldner introduces the notion of *domain assumptions*, by which he means those existential and normative *beliefs* that people have learned in their culture. Domain assumptions elicit certain *sentiments* that people have concerning their experience with the social world. Whether they realize it or not and admit it or not, Gouldner contends that the theories sociologists create reflect their domain assumptions and sentiments. What is more, these

social theories also arouse certain sentiments in the students who study them. Whether students accept or reject a theory is based on the feeling—of satisfaction or discomfort, optimism or pessimism—that the theory evokes in them. Depending on which feeling it produces, the theory will also take on different political meanings. It will, for instance, be seen as progressive or conservative, as idealistic or practical. Gouldner refers to sociological theorists' domain assumptions and private sentiments as the "infrastructure" that determines the nature of the social theory they construct.

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THE SOCIOLOGICAL IMAGINATION

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Oxford University Press

ON INTELLECTUAL CRAFTSMANSHIP

To the individual social scientist who feels himself a part of the classic tradition, social science is the practice of a craft. A man at work on problems of substance, he is among those who are quickly made impatient and weary by elaborate discussions of method-and-theory-in-general; so much of it interrupts his proper studies. It is much better, he believes, to have one account by a working student of how he is going about his work than a dozen 'codifications of procedure' by specialists who as often as not have never done much work of consequence. Only by conversations in which experienced thinkers exchange information about their actual ways of working can a useful sense of method and theory be imparted to the beginning student. I feel it useful, therefore, to report in some detail how I go about my craft. This is necessarily a personal statement, but it is written with the hope that others, especially those beginning independent work, will make it less personal by the facts of their own experience.

It is best to begin, I think, by reminding you, the beginning student, that the most admirable thinkers within the scholarly community you have chosen to join do not split their work from their lives. They seem to take both too seriously to allow such dissociation, and they want to use each for the enrichment of the other. Of course, such a split is the prevailing convention among men in general, deriving, I suppose, from the hollowness of the work which men in general now do. But you will have recognized that as a scholar you have the exceptional opportunity of designing a way of living which will encourage the habits of good workmanship. Scholarship is a choice of how to live as well as a choice of career; whether he knows it or not, the intellectual workman forms his own self as he works toward the perfection of his craft; to realize his own potentialities, and any opportunities that come his way, he constructs a character which has as its core the qualities of the good workman.

What this means is that you must learn to use your life experience in your intellectual work: continually to examine and interpret it. In this sense craftsmanship is the center of yourself and

you are personally involved in every intellectual product upon which you may work. To say that you can 'have experience,' means, for one thing, that your past plays into and affects your present, and that it defines your capacity for future experience. As a social scientist, you have to control this rather elaborate interplay, to capture what you experience and sort it out; only in this way can you hope to use it to guide and test your reflection, and in the process shape yourself as an intellectual craftsman. But how can you do this? One answer is: you must set up a file, which is, I suppose, a sociologist's way of saying: keep a Journal. Many creative writers keep journals; the sociologist's need for systematic reflection demands it.

In such a file as I am going to describe, there is joined personal experience and professional activities, studies under way and studies planned. In this file, you, as an intellectual craftsman, will try to get together what you are doing intellectually and what you are experiencing as a person. Here you will not be afraid to use your experience and relate it directly to various work in progress. By serving as a check on repetitive work, your file also enables you to conserve your energy. It also encourages you to capture 'fringe-thoughts': various ideas which may be byproducts of everyday life, snatches of conversation overheard on the street, or, for that matter, dreams. Once noted, these may lead to more systematic thinking, as well as lend intellectual relevance to more directed experience.

You will have often noticed how carefully accomplished thinkers treat their own minds, how closely they observe their development and organize their experience. The reason they treasure their smallest experiences is that, in the course of a lifetime, modern man has so very little personal experience and yet experience is so important as a source of original intellectual work. To be able to trust yet to be skeptical of your own experience, I have come to believe, is one mark of the mature workman. This ambiguous confidence is indispensable to originality in any intellectual pursuit, and the file is one way by which you can develop and justify such confidence.

By keeping an adequate file and thus developing self-reflective habits, you learn how to keep your inner world awake. Whenever you feel strongly about events or ideas you must try not to

let them pass from your mind, but instead to formulate them for your files and in so doing draw out their implications, show yourself either how foolish these feelings or ideas are, or how they might be articulated into productive shape. The file also helps you build up the habit of writing. You cannot 'keep your hand in' if you do not write something at least every week. In developing the file, you can experiment as a writer and thus, as they say, develop your powers of expression. To maintain a file is to engage in the controlled experience.

But, you may ask, how do ideas come? How is the imagination spurred to put all the images and facts together, to make images relevant and lend meaning to facts? I do not think I can really answer that; all I can do is talk about the general conditions and a few simple techniques which have seemed to increase my chances to come out with something.

The sociological imagination, I remind you, in considerable part consists of the capacity to shift from one perspective to another, and in the process to build up an adequate view of a total society and of its components. It is this imagination, of course, that sets off the social scientist from the mere technician. Adequate technicians can be trained in a few years. The sociological imagination can also be cultivated; certainly it seldom occurs without a great deal of often routine work.¹ Yet there is an unexpected quality about it, perhaps because its essence is the combination of ideas that no one expected were combinable—say, a mess of ideas from German philosophy and British economics. There is a playfulness of mind back of such combining as well as a truly fierce drive to make sense of the world, which the technician as such usually lacks. Perhaps he is too well trained, too precisely trained. Since one can be trained only in what is already known, training sometimes incapacitates one from learning new ways; it makes one rebel against what is bound to be at first loose and even sloppy. But you must cling to such vague images and notions, if they are yours, and you must work them out. For it is in such forms that original ideas, if any, almost always first appear.

¹ See the excellent articles on 'insight' and 'creative endeavor' by Hutchinson, in *Study of Interpersonal Relations*, edited by Patrick Mullahy, New York, Nelson, 1949.

There are definite ways, I believe, of stimulating the sociological imagination:

(1) On the most concrete level, the re-arranging of the file, as I have already said, is one way to invite imagination. You simply dump out heretofore disconnected folders, mixing up their contents, and then re-sort them. You try to do it in a more or less relaxed way. How often and how extensively you re-arrange the files will of course vary with different problems and with how well they are developing. But the mechanics of it are as simple as that. Of course, you will have in mind the several problems on which you are actively working, but you will also try to be passively receptive to unforeseen and unplanned linkages.

(2) An attitude of playfulness toward the phrases and words with which various issues are defined often loosens up the imagination. Look up synonyms for each of your key terms in dictionaries as well as in technical books, in order to know the full range of their connotations. This simple habit will prod you to elaborate the terms of the problem and hence to define them less wordily and more precisely. For only if you know the several meanings which might be given to terms or phrases can you select the exact ones with which you want to work. But such an interest in words goes further than that. In all work, but especially in examining theoretical statements, you will try to keep close watch on the level of generality of every key term, and you will often find it useful to break down a high-level statement into more concrete meanings. When that is done, the statement often falls into two or three components, each lying along different dimensions. You will also try to move up the level of generality: remove the specific qualifiers and examine the re-formed statement or inference more abstractly, to see if you can stretch it or elaborate it. So from above and from below, you will try to probe, in search of clarified meaning, into every aspect and implication of the idea.

(3) Many of the general notions you come upon, as you think about them, will be cast into types. A new classification is the usual beginning of fruitful developments. The skill to make up types and then to search for the conditions and consequences of each type will, in short, become an automatic procedure with you. Rather than

rest content with existing classifications, in particular, common-sense ones, you will search for their common denominators and for differentiating factors within and between them. Good types require that the criteria of classification be explicit and systematic. To make them so you must develop the habit of cross-classification.

The technique of cross-classifying is not of course limited to quantitative materials; as a matter of fact, it is the best way to imagine and to get hold of new types as well as to criticize and clarify old ones. Charts, tables, and diagrams of a qualitative sort are not only ways to display work already done; they are very often genuine tools of production. They clarify the 'dimensions' of the types, which they also help you to imagine and build. As a matter of fact, in the past fifteen years, I do not believe I have written more than a dozen pages first-draft without some little cross-classification—although, of course, I do not always or even usually display such diagrams. Most of them flop, in which case you have still learned something. When they work, they help you to think more clearly and to write more explicitly. They enable you to discover the range and the full relationships of the very terms with which you are thinking and of the facts with which you are dealing.

For a working sociologist, cross-classification is what diagramming a sentence is for a diligent grammarian. In many ways, cross-classification is the very grammar of the sociological imagination. Like all grammar, it must be controlled and not allowed to run away from its purposes.

(4) Often you get the best insights by considering extremes—by thinking of the opposite of that with which you are directly concerned. If you think about despair, then also think about elation; if you study the miser, then also the spendthrift. The hardest thing in the world is to study one object; when you try to contrast objects, you get a better grip on the materials and you can then sort out the dimensions in terms of which the comparisons are made. You will find that shuttling between attention to these dimensions and to the concrete types is very illuminating. This technique is also logically sound, for without a sample, you can only guess about statistical frequencies anyway: what you can do is to give the range and the major types of some phenomenon, and for that it is more economical to begin by constructing

'polar types,' opposites along various dimensions. This does not mean, of course, that you will not strive to gain and to maintain a sense of proportion—to look for some lead to the frequencies of given types. One continually tries, in fact, to combine this quest with the search for indices for which one might find or collect statistics.

The idea is to use a variety of viewpoints: you will, for instance, ask yourself how would a political scientist whom you have recently read approach this, and how would that experimental psychologist, or this historian? You try to think in terms of a variety of viewpoints and in this way to let your mind become a moving prism catching light from as many angles as possible. In this connection, the writing of dialogues is often very useful.

You will quite often find yourself thinking against something, and in trying to understand a new intellectual field, one of the first things you might well do is to lay out the major arguments. One of the things meant by 'being soaked in the literature' is being able to locate the opponents and the friends of every available viewpoint. By the way, it is not well to be too 'soaked in the literature'; you may drown in it, like Mortimer Adler. Perhaps the point is to know when you ought to read, and when you ought not to.

(5) The fact that, for the sake of simplicity, in cross-classification, you first work in terms of yes-or-no, encourages you to think of extreme opposites. That is generally good, for qualitative analysis cannot of course provide you with frequencies or magnitudes. Its technique and its end is to give you the range of types. For many purposes you need no more than that, although for some, of course, you do need to get a more precise idea of the proportions involved.

The release of imagination can sometimes be achieved by deliberately inverting your sense of proportion.² If something seems very minute, imagine it to be simply enormous, and ask yourself: What difference might that make? And vice versa, for gigantic phenomena. What would pre-literate villages look like with populations of 30 millions? Nowadays at least, I should never think of actually counting or measuring anything, before I

² By the way, some of this is what Kenneth Burke, in discussing Nietzsche, has called 'perspective by incongruity.' See, by all means, Burke, *Permanence and Change*, New York, New Republic Books, 1936.

had played with each of its elements and conditions and consequences in an imagined world in which I control the scale of everything. This is one thing statisticians ought to mean, but never seem to, by that horrible little phrase about 'knowing the universe before you sample it.'

(6) Whatever the problem with which you are concerned, you will find it helpful to try to get a comparative grip on the materials. The search for comparable cases, either in one civilization and historical period or in several, gives you leads. You would never think of describing an institution in twentieth-century America without trying to bear in mind similar institutions in other types of structures and periods. That is so even if you do not make explicit comparisons. In time you will come almost automatically to orient your reflection historically. One reason for doing so is that often what you are examining is limited in number: to get a comparative grip on it, you have to place it inside an historical frame. To put it another way, the contrasting-type approach often requires the examination of historical materials. This sometimes results in points useful for a trend analysis, or it leads to a typology of phases. You will use historical materials, then, because of the desire for a fuller range, or for a more convenient range of some phenomenon—by which I mean a range that includes the variations along some known set of dimensions. Some knowledge of world history is indispensable to the sociologist; without such knowledge, no matter what else he knows, he is simply crippled.

(7) There is, finally, a point which has more to do with the craft of putting a book together than with the release of the imagination. Yet these two are often one: how you go about arranging materials for presentation always affects the content of your work. The idea I have in mind I learned from a great editor, Lambert Davis, who, I suppose, after seeing what I have done with it, would not want to acknowledge it as his child. It is the distinction between theme and topic.

A topic is a subject, like 'the careers of corporation executives' or 'the increased power of military officials' or 'the decline of society matrons.' Usually most of what you have to say about a topic can readily be put into one chapter or a section of a chapter. But the order in which all your topics are arranged often brings you into the realm of themes.

A theme is an idea, usually of some signal trend, some master conception, or a key distinction, like rationality and reason, for example. In working out the construction of a book, when you come to realize the two or three, or, as the case may be, the six or seven themes, then you will know that you are on top of the job. You will recognize these themes because they keep insisting upon being dragged into all sorts of topics and perhaps you will feel that they are mere repetitions. And sometimes that is all they are! Certainly very often they will be found in the more clotted and confused, the more badly written, sections of your manuscript.

What you must do is sort them out and state them in a general way as clearly and briefly as you can. Then, quite systematically, you must cross-classify them with the full range of your topics. This means that you will ask of each topic: Just how is it affected by each of these themes? And again: Just what is the meaning, if any, for each of these themes of each of the topics?

Sometimes a theme requires a chapter or a section for itself, perhaps when it is first

introduced or perhaps in a summary statement toward the end. In general, I think most writers—as well as most systematic thinkers—would agree that at some point all the themes ought to appear together, in relation to one another. Often, although not always, it is possible to do this at the beginning of a book. Usually, in any well-constructed book, it must be done near the end. And, of course, all the way through you ought at least to try to relate the themes to each topic. It is easier to write about this than to do it, for it is usually not so mechanical a matter as it might appear. But sometimes it is—at least if the themes are properly sorted out and clarified. But that, of course, is the rub. For what I have here, in the context of literary craftsmanship, called themes, in the context of intellectual work are called ideas.

Sometimes, by the way, you may find that a book does not really have any themes. It is just a string of topics, surrounded, of course, by methodological introductions to methodology, and theoretical introductions to theory. These are indeed quite indispensable to the writing of books by men without ideas. And so is lack of intelligibility.

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

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

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 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 "proto-sociology" 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 Spence-rian 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 empiricism," 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 standing, 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 interrelations 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), cathectic (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 formulae 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 inter-dependences 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 solid 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 opportunity? 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.

3

SOCIAL THEORY AND SOCIAL STRUCTURE

ROBERT K. MERTON

ON SOCIOLOGICAL THEORIES OF THE MIDDLE RANGE

Like so many words that are bandied about, the word theory threatens to become meaningless. Because its referents are so diverse—including everything from minor working hypotheses, through comprehensive but vague and unordered speculations, to axiomatic systems of thought—use of the word often obscures rather than creates understanding.

Throughout this book, the term *sociological theory* refers to logically interconnected sets of propositions from which empirical uniformities can be derived. Throughout we focus on what I have called *theories of the middle range*: theories that lie between the minor but necessary working hypotheses that evolve in abundance during day-to-day research¹ and the all-inclusive systematic efforts to develop a unified theory that will

¹“A ‘working hypothesis’ is little more than the common-sense procedure used by all of us everyday. Encountering certain facts, certain alternative explanations come to mind and we proceed to test them.” James B. Conant, *On Understanding Science* (New Haven: Yale University Press, 1947), 137, n. 4.

explain all the observed uniformities of social behavior, social organization and social change.²

Middle-range theory is principally used in sociology to guide empirical inquiry. It is intermediate to general theories of social systems which are too remote from particular classes of social behavior, organization and change to account for what is observed and to those detailed orderly descriptions of particulars that are not generalized at all. Middle-range theory involves abstractions, of course, but they are close enough to observed data to be incorporated in propositions that permit empirical testing. Middle-range theories deal with delimited aspects of social phenomena, as is indicated by their labels. One speaks of a theory of reference groups, of social

²This discussion draws upon and expands a critique of Parsons’ paper on the position of sociological theory at the 1947 meetings of the American Sociological Society as briefly published in the *American Sociological Review*, 1949, 13, 164–8. It draws also upon subsequent discussions: R. K. Merton, “The role-set: problems in sociological theory,” *The British Journal of Sociology*, June 1957, 8, 106–20, at 108–10; R. K. Merton, “Introduction” to Allen Barton, *Social Organization under Stress: A Sociological Review of Disaster Studies* (Washington, D.C.: National Academy of Sciences—National Research Council, 1963), xvii–xxxvi, at xxix–xxxvi.

mobility, or role-conflict and of the formation of social norms just as one speaks of a theory of prices, a germ theory of disease, or a kinetic theory of gases.

The seminal ideas in such theories are characteristically simple: consider Gilbert on magnetism, Boyle on atmospheric pressure, or Darwin on the formation of coral atolls. Gilbert *begins* with the relatively simple idea that the earth may be conceived as a magnet; Boyle, with the simple idea that the atmosphere may be conceived as a 'sea of air'; Darwin, with the idea that one can conceive of the atolls as upward and outward growths of coral over islands that had long since subsided into the sea. Each of these theories provides an image that gives rise to inferences. To take but one case: if the atmosphere is thought of as a sea of air, then, as Pascal inferred, there should be less air pressure on a mountain top than at its base. The initial idea thus suggests specific hypotheses which are tested by seeing whether the inferences from them are empirically confirmed. The idea itself is tested for its fruitfulness by noting the range of theoretical problems and hypotheses that allow one to identify new characteristics of atmospheric pressure.

In much the same fashion, the theory of reference groups and relative deprivation starts with the simple idea, initiated by James, Baldwin, and Mead and developed by Hyman and Stouffer, that people take the standards of significant others as a basis for self-appraisal and evaluation. Some of the inferences drawn from this idea are at odds with common-sense expectations based upon an unexamined set of 'self-evident' assumptions. Common sense, for example, would suggest that the greater the actual loss experienced by a family in a mass disaster, the more acutely it will feel deprived. This belief is based on the unexamined assumption that the magnitude of objective loss is related linearly to the subjective appraisal of the loss and that this appraisal is confined to one's own experience. But the theory of relative deprivation leads to quite a different hypothesis—that self-appraisals depend upon people's comparisons of their own situation with that of other people perceived as being comparable to themselves. This theory therefore suggests that, under specifiable conditions, families suffering serious losses will feel less deprived than those suffering smaller losses if they are in

situations leading them to compare themselves to people suffering even more severe losses. For example, it is people in the area of greatest impact of a disaster who, though substantially deprived themselves, are most apt to see others around them who are even more severely deprived. Empirical inquiry supports the theory of relative deprivation rather than the common-sense assumptions: "the feeling of being relatively *better off* than others *increases with objective loss up* to the category of highest loss" and only then declines. This pattern is reinforced by the tendency of public communications to focus on "the *most extreme sufferers* [which] tends to fix them as a reference group against which even other sufferers can compare themselves favorably." As the inquiry develops, it is found that these patterns of self-appraisal in turn affect the distribution of morale in the community of survivors and their motivation to help others.³ Within a particular *class* of behavior, therefore, the theory of relative deprivation directs us to a set of hypotheses that can be empirically tested. The confirmed conclusion can then be put simply enough: when few are hurt to much the same extent, the pain and loss of each seems great; where many are hurt in greatly varying degree, even fairly large losses seem small as they are compared with far larger ones. The probability that comparisons will be made is affected by the differing visibility of losses of greater and less extent.

The specificity of this example should not obscure the more general character of middle-range theory. Obviously, behavior of people confronted with a mass disaster is only one of an indefinitely large array of particular situations to which the theory of reference groups can be instructively applied, just as is the case with the theory of change in social stratification, the theory of authority, the theory of institutional interdependence, or the theory of anomie. But it is equally clear that such middle-range theories have not been logically *derived* from a single all-embracing theory of social systems, though once developed they may be consistent with one. Furthermore, each theory is more than a mere empirical generalization—an isolated proposition summarizing observed uniformities of

³Barton, *op. cit.*, 62–63, 70–72, 140, and the Introduction, xxiv–xxv.

relationships between two or more variables. A theory comprises a set of assumptions from which empirical generalizations have themselves been derived.

Another case of middle-range theory in sociology may help us to identify its character and uses. The theory of role-sets⁴ begins with an image of how social status is organized in the social structure. This image is as simple as Boyle's image of the atmosphere as a sea of air or Gilbert's image of the earth as a magnet. As with all middle-range theories, however, the proof is in the using not in the. immediate response to the originating ideas as obvious or odd, as derived from more general theory or conceived of to deal with a particular class of problems.

Despite the very diverse meanings attached to the concept of *social status*, one sociological tradition consistently uses it to refer to a position in a social system, with its distinctive array of designated rights and obligations. In this tradition, as exemplified by Ralph Linton, the related concept of *social role* refers to the behavior of status-occupants that is oriented toward the patterned expectations of others (who accord the rights and exact the obligations). Linton, like others in this tradition, went on to state the long recognized and basic observation that each person in society inevitably occupies multiple statuses and that each of these statuses has its associated role.

It is at this point that the imagery of the role-set theory departs from this long-established tradition. The difference is initially a small one—some might say so small as to be insignificant—but the shift in the angle of vision leads to successively more fundamental theoretical differences. Role-set theory begins with the concept that each social status involves not a single associated role, but an array of roles. This feature of social structure gives rise to the concept of role-set: that complement of social relationships in which persons are involved simply because they occupy a particular social status. Thus, a person in the status of medical student plays not only the role of student *vis-à-vis* the correlative status of his teachers, but also an array of other roles relating him diversely to others in the system: other students, physicians, nurses, social workers, medical technicians, and the like. Again, the

⁴The following pages draw upon Merton, "The role-set," *op. cit.*

status of school teacher has its distinctive role-set which relates the teacher not only to the correlative status, pupil, but also to colleagues, the school principal and superintendent, the Board of Education, professional associations and, in the United States, local patriotic organizations.

Notice that the role-set differs from what sociologists have long described as 'multiple roles.' The latter term has traditionally referred not to the complex of roles associated with a single social status but to the various social statuses (often, in different institutional spheres) in which people find themselves—for example, one person might have the diverse statuses of physician, husband, father, professor, church elder, Conservative Party member and army captain. (This complement of distinct statuses of a person, each with its own role-set, is a status-set.)

Up to this point, the concept of role-set is *merely* an image for thinking about a component of the social structure. But this image is a beginning, not an end, for it leads directly to certain analytical problems. The notion of the role-set at once leads to the inference that social structures confront men with the task of articulating the components of countless role-sets—that is, the functional task of managing somehow to organize these so that an appreciable degree of social regularity obtains, sufficient to enable most people most of the time to go about their business without becoming paralyzed by extreme conflicts in their role-sets.

If this relatively simple idea of role-set has theoretical worth, it should generate distinctive problems for sociological inquiry. The concept of role-set does this.⁵ It raises the general but definite problem of identifying the social mechanisms—that is, the social processes having designated consequences for designated parts of the social structure—which articulate the expectations of those in the role-set sufficiently to reduce conflicts for the occupant of a status. It generates the further problem of discovering how these mechanisms come into being, so that we can also explain

⁵For an early version of this developing idea, see Merton, "The social-cultural environment and *anomie*," in Helen L. Witmer and Ruth Kotinsky, editors, *New Perspective for Research on Juvenile Delinquency: Report on a conference on the relevance and interrelations of certain concepts from sociology and psychiatry for delinquency*, held May 6 and 7, 1955 (Washington, D.C.: U.S. Department of Health, Education, and Welfare, 1956), 24–50, at 47–48.

why the mechanisms do not operate effectively or fail to emerge at all in some social systems. Finally, like the theory of atmospheric pressure, the theory of role-set points directly to relevant empirical research. Monographs on the workings of diverse types of formal organization have developed empirically-based theoretical extensions of how role-sets operate in practice.⁶

The theory of role-sets illustrates another aspect of sociological theories of the middle range. They are frequently consistent with a variety of so-called systems of sociological theory. So far as one can tell, the theory of role-sets is not inconsistent with such broad theoretical orientations as Marxist theory, functional analysis, social behaviorism, Sorokin's integral sociology, or Parsons' theory of action. This may be a horrendous observation for those of us who have been trained to believe that systems of sociological thought are logically close-knit and mutually exclusive sets of doctrine. But in fact, as we shall note later in this introduction, comprehensive sociological theories are sufficiently loose-knit, internally diversified, and mutually overlapping that a *given theory of the middle range*, which has a measure of empirical confirmation, can often be subsumed under comprehensive theories which are themselves discrepant in certain respects.

This reasonably unorthodox opinion can be illustrated by reexamining the theory of role-sets as a middle-range theory. We depart from the traditional concept by assuming that a single status in society involves, not a single role, but an array of associated roles, relating the status-occupant to diverse others. Second, we note that this concept of the role-set gives rise to distinctive theoretical problems, hypotheses, and so to

⁶If we are to judge from the dynamics of development in science, sketched out in the preceding part of this introduction, theories of the middle range, being close to the research front of science, are particularly apt to be products of multiple and approximately simultaneous discovery. The core idea of the role-set was independently developed in the important empirical monograph, Neal Gross, Ward S. Mason and A. W. McEachern, *Explorations in Role Analysis: Studies of the School Superintendency Role* (New York: John Wiley & Sons, Inc., 1958). Significant extensions of the theory coupled with empirical investigation will be found in the monographs: Robert L. Kahn et al., *Organizational Stress: Studies in Role Conflict and Ambiguity* (New York: John Wiley & Sons, 1964), see 13–17 and *passim*; Daniel Katz and Robert L. Kahn, *The Social Psychology of Organizations* (New York: John Wiley & Sons, 1966) 172 ff. and *passim*.

empirical inquiry. One basic problem is that of identifying the social mechanisms which articulate the role-set and reduce conflicts among roles. Third, the concept of the role-set directs our attention to the structural problem of identifying the social arrangements which integrate as well as oppose the expectations of various members of the role-set. The concept of multiple roles, on the other hand, confines our attention to a different and no doubt important issue: how do *individual* occupants of statuses happen to deal with the many and sometimes conflicting demands made of them? Fourth, the concept of the role-set directs us to the further question of how these social mechanisms come into being; the answer to this question enables us to account for the many concrete instances in which the role-set operates ineffectively. (This no more assumes that all social mechanisms are functional than the theory of biological evolution involves the comparable assumption that no dysfunctional developments occur.) Finally, the logic of analysis exhibited in this sociological theory of the middle-range is developed wholly in terms of the elements of social structure rather than in terms of providing concrete *historical descriptions* of particular social systems. Thus, middle-range theory enables us to transcend the mock problem of a theoretical conflict between the nomothetic and the idiothetic, between the general and the altogether particular, between generalizing sociological theory and historicism.

From all this, it is evident that according to role-set theory there is always a *potential* for differing expectations among those in the role-set as to what is appropriate conduct for a status-occupant. The basic source of this potential for conflict—and it is important to note once again that on this point we are at one with such disparate general theorists as Marx and Spencer, Simmel, Sorokin and Parsons—is found in the structural fact that the other members of a role-set are apt to hold different social positions differing from that of the status-occupant in question. To the extent that members of a role-set are diversely located in the social structure, they are apt to have interests and sentiments, values and moral expectations, differing from those of the status-occupant himself. This, after all, is one of the principal assumptions of Marxist theory as it is of much other sociological theory: social differentiation generates distinct interests among those variously located in the structure of

the society. For example, the members of a school board are often in social and economic strata that differ significantly from the stratum of the school teacher. The interests, values, and expectations of board members are consequently apt to differ from those of the teacher who may thus be subject to conflicting expectations from these and other members of his role-set: professional colleagues, influential members of the school board and, say, the Americanism Committee of the American Legion. An educational essential for one is apt to be judged as an educational frill by another, or as downright subversion, by the third. What holds conspicuously for this one status holds, in identifiable degree, for occupants of other statuses who are structurally related through their role-set to others who themselves occupy differing positions in society.

As a theory of the middle range, then, the theory of role-sets begins with a concept and its associated imagery and generates an array of theoretical problems. Thus, the assumed structural basis for potential disturbance of a role-set gives rise to a double question (which, the record shows, has not been raised in the absence of the theory): which social mechanisms, if any, operate to counteract the theoretically assumed instability of role-sets and, correlatively, under which circumstances do these social mechanisms fail to operate, with resulting inefficiency, confusion, and conflict? Like other questions that have historically stemmed from the general orientation of functional analysis, these do not assume that role-sets invariably operate with substantial efficiency. For this middle-range theory is not concerned with the historical generalization that a degree of social order or conflict prevails in society but with the analytical problem of identifying the social mechanisms which produce a greater degree of order or less conflict than would obtain if these mechanisms were not called into play.

TOTAL SYSTEMS OF SOCIOLOGICAL THEORY

The quest for theories of the middle range exacts a distinctly different commitment from the sociologist than does the quest for an all-embracing, unified theory. The pages that follow assume that this search for a total system of sociological theory, in which observations about every aspect of social

behavior, organization, and change promptly find their preordained place, has the same exhilarating challenge and the same small promise as those many all-encompassing philosophical systems which have fallen into deserved disuse. The issue must be fairly joined. Some sociologists still write as though they expect, here and now, formulation of the general sociological theory broad enough to encompass the vast ranges of precisely observed details of social behavior, organization, and change and fruitful enough to direct the attention of research workers to a flow of problems for empirical research. This I take to be a premature and apocalyptic belief. We are not ready. Not enough preparatory work has been done.

An historical sense of the changing intellectual contexts of sociology should be sufficiently humbling to liberate these optimists from this extravagant hope. For one thing, certain aspects of our historical past are still too much with us. We must remember that early sociology grew up in an intellectual atmosphere⁷ in which vastly comprehensive systems of philosophy were being introduced on all sides. Any philosopher of the eighteenth and early nineteenth centuries worth his salt had to develop his own philosophical system—of these, Kant, Fichte, Schelling, Hegel were only the best known. Each system was a personal bid for the definitive overview of the universe of matter, nature and man.

These attempts of philosophers to create total systems became a model for the early sociologists, and so the nineteenth century was a century of sociological systems. Some of the founding fathers, like Comte and Spencer, were imbued with the *esprit de systeme*, which was expressed in their sociologies as in the rest of their wider-ranging philosophies. Others, such as Gumplovicz, Ward, and Giddings, later tried to provide intellectual legitimacy for this still “new science of a very ancient subject.” This required that a general and definitive framework of sociological thought be built rather than developing special theories designed to guide the investigation of specific sociological problems within an evolving and provisional framework.

Within this context, almost all the pioneers in sociology tried to fashion his own system.

⁷See the classical work by John Theodore Merz, *A History of European Thought in the Nineteenth Century* (Edinburgh and London: William Blackwood, 1904), 4 vols.

The multiplicity of systems, each claiming to be the genuine sociology, led naturally enough to the formation of schools, each with its cluster of masters, disciples and epigoni. Sociology not only became differentiated with other disciplines, but it became internally differentiated. This differentiation, however, was not in terms of specialization, as in the sciences, but rather, as in philosophy, in terms of total systems, typically held to be mutually exclusive and largely at odds. As Bertrand Russell noted about philosophy, this total sociology did not seize “the advantage, as compared with the [sociologies] of the system-builders, of being able to tackle its problems one at a time, instead of having to invent at one stroke a block theory of the whole [sociological] universe.”⁸

Another route has been followed by sociologists in their quest to establish the intellectual legitimacy of their discipline: they have taken as their prototype systems of scientific theory rather than systems of philosophy. This path too has sometimes led to the attempt to create total systems of sociology—a goal that is often based on one or more of three basic misconceptions about the sciences.

The first misinterpretation assumes that systems of thought can be effectively developed before a great mass of basic observations has been accumulated. According to this view, Einstein might follow hard on the heels of Kepler, without the intervening centuries of investigation and systematic thought about the results of investigation that were needed to prepare the terrain. The systems of sociology that stem from this tacit assumption are much like those introduced by the system-makers in medicine over a span of 150 years: the systems of Stahl, Boissier de Sauvages, Broussais, John Brown and Benjamin Hush. Until well into the nineteenth century eminent personages in medicine thought it necessary to develop a theoretical system of disease long before the antecedent empirical inquiry had been adequately developed.⁹ These

garden-paths have since been closed off in medicine but this sort of effort still turns up in sociology. It is this tendency that led the biochemist and avocational sociologist, L. J. Henderson, to observe:

A difference between most system-building in the social sciences and systems of thought and classification in the natural sciences is to be seen in their evolution. In the natural sciences both theories and descriptive systems grow by adaptation to the increasing knowledge and experience of the scientists. *In the social sciences, systems often issue fully formed from the mind of one man.* Then they may be much discussed if they attract attention, but *progressive adaptive modification as a result of the concerted efforts of great numbers of men is rare.*¹⁰

The second misconception about the physical sciences rests on a mistaken assumption of historical contemporaneity—that *all cultural products existing at the same moment of history have the same degree of maturity.* In fact, to perceive differences here would be to achieve a sense of proportion. The fact that the discipline of physics and the discipline of sociology are both identifiable in the mid-twentieth century does not mean that the achievements of the one should be the measure of the other. True, social scientists today live at a time when physics has achieved comparatively great scope and precision of theory and experiment, a great aggregate of tools of investigation, and an abundance of technological by-products. Looking about them, many sociologists take the achievements of physics as the standard for self-appraisal. They want to compare biceps with their bigger brothers. They, too, want to count. And when it becomes evident that they neither have the rugged physique nor pack the murderous wallop of their big brothers, some sociologists despair. They begin to ask: is a science of society really possible unless we institute a total system of sociology? But this perspective ignores the fact that between twentieth-century physics and

⁸Bertrand Russell, *A History of Western Philosophy* (New York: Simon and Schuster, 1945), 834.

⁹Wilfred Trotter, *Collected Papers* (Oxford University Press, 1941), 150. The story of the system-makers is told in every history of medicine; for example, Fielding H. Garrison, *An Introduction to the History of Medicine* (Philadelphia: Saunders, 1929) and Ralph H. Major, *A History of Medicine* (Oxford: Blackwell Scientific Publications, 1954), 2 vols.

¹⁰Lawrence J. Henderson, *The Study of Man* (Philadelphia: University of Pennsylvania Press, 1941), 19–20, italics supplied; for that matter, the entire book can be read with profit by most of us sociologists.

twentieth-century sociology stand billions of man-hours of sustained, disciplined, and cumulative research. Perhaps sociology is not yet ready for its Einstein because it has not yet found its Kepler—to say nothing of its Newton, Laplace, Gibbs, Maxwell or Planck.

Third, sociologists sometimes misread the actual state of theory in the physical sciences. This error is ironic, for physicists agree that they have not achieved an all-encompassing system of theory, and most see little prospect of it in the near future. What characterizes physics is an array of special theories of greater or less scope, coupled with the historically-grounded hope that these will continue to be brought together into families of theory. As one observer puts it: “though most of us hope, it is true, for an all embracing future theory which will unify the various postulates of physics, we do not wait for it before proceeding with the important business of science.”¹¹ More recently, the theoretical physicist, Richard Feynman, reported without dismay that “today our theories of physics, the laws of physics, are a multitude of different parts and pieces that do not fit together very well.”¹² But perhaps most telling is the observation by that most comprehensive of theoreticians who devoted the last years of his life to the unrelenting and unsuccessful search “for a unifying theoretical basis for all these single disciplines, consisting of a minimum of concepts and fundamental relationships, from which all the concepts and relationships of the single disciplines might be derived by logical process.” Despite his own profound and lonely commitment to this quest, Einstein observed:

The greater part of physical research is devoted to the development of the various branches in physics, in each of which the object is the theoretical understanding of more or less restricted fields of experience, and in each of which the laws and concepts remain as closely as possible related to experience.¹³

¹¹ Henry Margenau, “The basis of theory in physics,” unpublished ms., 1949, 5–6.

¹² Richard Feynman, *The Character of Physical Law* (London: Cox & Wyman Ltd., 1965), 30.

¹³ Albert Einstein, “The fundamentals of theoretical physics,” in L. Hamalian and E. L. Volpe, eds. *Great Essays by Nobel Prize Winners* (New York: Noonday Press, 1960), 219–30 at 220.

These observations might be pondered by those sociologists who expect a sound general system of sociological theory in our time—or soon after. If the science of physics, with its centuries of enlarged theoretical generalizations, has not managed to develop an all-encompassing theoretical system, then *a fortiori* the science of sociology, which has only begun to accumulate empirically grounded theoretical generalizations of modest scope, would seem well advised to moderate its aspirations for such a system.

UTILITARIAN PRESSURES FOR TOTAL SYSTEMS OF SOCIOLOGY

The conviction among some sociologists that we must, here and now, achieve a grand theoretical system not only results from a misplaced comparison with the physical sciences, it is also a response to the ambiguous position of sociology in contemporary society. The very uncertainty about whether the accumulated knowledge of sociology is adequate to meet the large demands now being made of it—by policymakers, reformers and reactionaries, by business-men and government-men, by college presidents and college sophomores—provokes an overly-zealous and defensive conviction on the part of some sociologists that they must somehow be equal to these demands, however premature and extravagant they may be.

This conviction erroneously assumes that a science must be adequate to meet *all* demands, intelligent or stupid, made of it. This conviction is implicitly based on the sacrilegious and masochistic assumption that one must be omniscient and omnicompetent—to admit to less than total knowledge is to admit to total ignorance. So it often happens that the exponents of a fledgling discipline make extravagant claims to total systems of theory, adequate to the entire range of problems encompassed by the discipline. It is this sort of attitude that Whitehead referred to in the epigraph to this book: “It is characteristic of a science in its earlier stages . . . to be both ambitiously profound in its aims and trivial in its handling of details.”

Like the sociologists who thoughtlessly compared themselves with contemporary physical scientists because they both are alive at the same

instant of history, the general public and its strategic decision-makers often err in making a definitive appraisal of social science on the basis of its ability to solve the urgent problems of society today. The misplaced masochism of the social scientist and the inadvertent sadism of the public both result from the failure to remember that social science, like all science, is continually developing and that there is no providential dispensation providing that at any given moment it will be adequate to the entire array of problems confronting men. In historical perspective this expectation would be equivalent to having forever prejudged the status and promise of medicine in the seventeenth century according to its ability to produce, then and there, a cure or even a preventative for cardiac diseases. If the problem had been widely acknowledged—look at the growing rate of death from coronary thrombosis!—its very importance would have obscured the *entirely independent question* of how adequate the medical knowledge of 1650 (or 1850 or 1950) was for solving a wide array of other health problems. Yet it is precisely this illogic that lies behind so many of the practical demands made on the social sciences. Because war and exploitation and poverty and racial discrimination and psychological insecurity plague modern societies, social science must justify itself by providing solutions for all of these problems. Yet social scientists may be no better equipped to solve these urgent problems today than were physicians, such as Harvey or Sydenham, to identify, study, and cure coronary thrombosis in 1655. Yet, as history testifies, the inadequacy of medicine to cope with this particular problem scarcely meant that it lacked powers of development.

If everyone backs only the sure thing, who will support the colt yet to come into its own?

My emphasis upon the gap between the practical problems assigned to the sociologist and the state of his accumulated knowledge and skills does not mean of course, that the sociologist should not seek to develop increasingly comprehensive theory or should not work on research directly relevant to urgent practical problems. Most of all, it does not mean that sociologists should deliberately seek out the pragmatically trivial problem. Different sectors in the spectrum of basic research and theory have different probabilities of being germane to particular practical

problems; they have differing potentials of relevance.¹⁴ But it is important to re-establish an historical sense of proportion. The urgency or immensity of a practical social problem does not ensure its immediate solution.¹⁵ At any given moment, men of science are close to the solutions of some problems and remote from others. It must be remembered that necessity is only the mother of invention; socially accumulated knowledge is its father. Unless the two are brought together, necessity remains infertile. She may of course conceive at some future time when she is properly mated. But the mate requires time (and sustenance) if he is to attain the size and vigor needed to meet the demands that will be made upon him.

This book's orientation toward the relationship of current sociology and practical problems of society is much the same as its orientation toward the relationship of sociology and general sociological theory. It is a developmental orientation, rather than one that relies on the sudden mutations of one sociologist that suddenly bring solutions to major social problems or to a single encompassing theory. Though this orientation makes no marvellously dramatic claims, it offers a reasonably realistic assessment of the current condition of sociology and the ways in which it actually develops.

TOTAL SYSTEMS OF THEORY AND THEORIES OF THE MIDDLE RANGE

From all this it would seem reasonable to suppose that sociology will advance insofar as its major (but not exclusive) concern is with developing theories of the middle range, and it will be retarded if its

¹⁴This conception is developed in R. K. Merton, "Basic research and potentials of relevance," *American Behavioral Scientist*, May 1963, VI, 86–90 on the basis of my earlier discussion, "The role of applied social science in the formation of policy," *Philosophy of Science*, 1949, 16, 161–81.

¹⁵As can be seen in detail in such works as the following: Paul F. Lazarsfeld, William Sewell and Harold Wilensky, eds., *The Uses of Sociology* (New York: Basic Books, in press); Alvin W. Gouldner and S. M. Miller, *Applied Sociology: Opportunities and Problems* (New York: The Free Press, 1965); Bernard Rosenberg, Israel Gerver and F. William Howton, *Mass Society in Crisis: Social Problems and Social Pathology* (New York: The Macmillan Company, 1964); Barbara Wootton, *Social Science and Social Pathology* (New York: The Macmillan Company, 1959).

primary attention is focussed on developing total sociological systems. So it is that in his inaugural address at the London School of Economics, T. H. Marshall put in a plea for sociological “stepping-stones in the middle distance.”¹⁶ Our major task today is to develop special theories applicable to limited conceptual ranges—theories, for example, of deviant behavior, the unanticipated consequences of purposive action, social perception, reference groups, social control, the interdependence of social institutions—rather than to seek immediately the total conceptual structure that is adequate to derive these and other theories of the middle range.

Sociological theory, if it is to advance significantly, must proceed on these interconnected planes: (1) by developing special theories from which to derive hypotheses that can be empirically investigated and (2) by evolving, not suddenly revealing, a progressively more general conceptual scheme that is adequate to consolidate groups of special theories.

To concentrate entirely on special theories is to risk emerging with specific hypotheses that account for limited aspects of social behavior, organization and change but that remain mutually inconsistent.

To concentrate entirely on a master conceptual scheme for deriving all subsidiary theories is to risk producing twentieth-century sociological equivalents of the large philosophical systems of the past, with all then-varied suggestiveness, their architectonic splendor, and their scientific sterility. The sociological theorist who is *exclusively* committed to the exploration of a total system with its utmost abstractions runs the risk that, as with modern decor, the furniture of his mind will be bare and uncomfortable.

The road to effective general schemes in sociology will only become clogged if, as in the early days of sociology, each charismatic sociologist tries to develop his own general system of theory. The persistence of this practice can only make for the balkanization of sociology, with each principality governed by its own theoretical system. Though this process has periodically marked the development of other sciences—conspicuously, chemistry, geology and medicine—it need not be reproduced in sociology if we learn from the

¹⁶The inaugural lecture was delivered 21 February 1946. It is printed in T. H. Marshall, *Sociology at the Crossroads* (London: Heinemann, 1963), 3–24.

history of science. We sociologists can look instead toward progressively comprehensive sociological theory which, instead of proceeding from the head of one man, gradually consolidates theories of the middle range, so that these become special cases of more general formulations.

Developments in sociological theory suggest that emphasis on this orientation is needed. Note how few, how scattered, and how unimpressive are the specific sociological hypotheses which are *derived* from a master conceptual scheme. The proposals for an all-embracing theory run so far ahead of confirmed special theories as to remain unrealized programs rather than *consolidations* of theories that at first seemed discrete. Of course, as Talcott Parsons and Pitirim Sorokin (in his *Sociological Theories of Today*) have indicated, significant progress has recently been made. The gradual convergence of streams of theory in sociology, social psychology and anthropology records large theoretical gains and promises even more.¹⁷ Nonetheless, a large part

¹⁷I attach importance to the observations made by Talcott Parsons in his presidential address to the American Sociological Society subsequent to my formulation of this position. For example: “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, . . . 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 [*n.b.*] . . . It can also 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 [*n.b.*], it is indispensable to the systematic clarification of problems and the fruitful formulation of hypotheses.” (italics supplied)

Parsons, “The prospects of sociological theory,” *American Sociological Review*, February 1950, 15, 3–16 at 7. It is significant that a general theorist, such as Parsons, acknowledges (1) that in fact general sociological theory seldom provides for specific hypotheses to be derived from it; (2) that, in comparison with a field such as physics, such derivations for most hypotheses are a remote objective; (3) that general theory provides only a general orientation and (4) that it serves as a basis for codifying empirical generalizations and specific theories. Once all this is acknowledged, the sociologists who are committed to developing general theory do not differ significantly in principle from those who see the best promise of sociology today in developing theories of the middle range and consolidating them periodically.

of what is now described as sociological theory consists of *general orientations toward data, suggesting types of variables which theories must somehow take into account, rather than clearly formulated, verifiable statements of relationships between specified variables*. We have many concepts but fewer confirmed theories; many points of view, but few theorems; many “approaches” but few arrivals. Perhaps some further changes in emphasis would be all to the good.

Consciously or unconsciously, men allocate their scant resources as much in the production of sociological theory as they do in the production of plumbing supplies, and their allocations reflect their underlying assumptions. Our discussion of middle range theory in sociology is intended to

make explicit a policy decision faced by all sociological theorists. Which shall have the greater share of our collective energies and resources: the search for confirmed theories of the middle range or the search for an all-inclusive conceptual scheme? I believe—and beliefs are of course notoriously subject to error—that theories of the middle range hold the largest, provided that the search for them is coupled with a pervasive concern with consolidating special theories into more general sets of concepts and mutually consistent propositions. Even so, we must adopt the provisional outlook of our brothers and of Tennyson:

Our little systems have their day;
They have their day and cease to be.

4

SOCIAL BEHAVIOR AS EXCHANGE

GEORGE C. HOMANS

To consider social behavior as an exchange of goods may clarify the relations among four bodies of theory: behavioral psychology, economics, propositions about the dynamics of influence, and propositions about the structure of small groups.

THE PROBLEMS OF SMALL-GROUP RESEARCH

This essay will hope to honor the memory of Georg Simmel in two different ways. So far as it pretends to be suggestive rather than conclusive, its tone will be Simmer's; and its subject, too, will be one of his. Because Simmel, in essays such as those on sociability, games, coquetry, and conversation, was an analyst of elementary social behavior, we call him an ancestor of what is known today as small-group research. For what we are really studying in small groups is elementary social behavior: what happens when two or three persons are in a position to influence one another, the sort of thing of which those massive structures called "classes," "firms," "communities," and "societies" must ultimately be composed.

As I survey small-group research today, I feel that, apart from just keeping on with it, three sorts of things need to be done. The first is to show the relation between the results of

experimental work done under laboratory conditions and the results of *quasi*-anthropological field research on what those of us who do it are pleased to call "real-life" groups in industry and elsewhere. If the experimental work has anything to do with real life—and I am persuaded that it has everything to do—its propositions cannot be inconsistent with those discovered through the field work. But the consistency has not yet been demonstrated in any systematic way.

The second job is to pull together in some set of general propositions the actual results, from the laboratory and from the field, of work on small groups—propositions that at least sum up, to an approximation, what happens in elementary social behavior, even though we may not be able to explain why the propositions should take the form they do. A great amount of work has been done, and more appears every day, but what it all amounts to in the shape of a set of propositions from which, under specified conditions, many of the observational results might be

derived, is not at all clear—and yet to state such a set is the first aim of science.

The third job is to begin to show how the propositions that empirically hold good in small groups may be derived from some set of still more general propositions. “Still more general” means only that empirical propositions other than ours may also be derived from the set. This derivation would constitute the explanatory stage in the science of elementary social behavior, for explanation is derivation.¹ (I myself suspect that the more general set will turn out to contain the propositions of behavioral psychology. I hold myself to be an “ultimate psychological reductionist,” but I cannot know that I am right so long as the reduction has not been carried out.)

I have come to think that all three of these jobs would be furthered by our adopting the view that interaction between persons is an exchange of goods, material and non-material. This is one of the oldest theories of social behavior, and one that we still use every day to interpret our own behavior, as when we say, “I found so-and-so rewarding”; or “I got a great deal out of him”; or, even, “Talking with him took a great deal out of me.” But, perhaps just because it is so obvious, this view has been much neglected by social scientists. So far as I know, the only theoretical work that makes explicit use of it is Marcel Mauss’s *Essai sur le don*, published in 1925, which is ancient as social science goes.² It may be that the tradition of neglect is now changing and that, for instance, the psychologists who interpret behavior in terms of transactions may be coming back to something of the sort I have in mind.³

An incidental advantage of an exchange theory is that it might bring sociology closer to economics—that science of man most advanced, most capable of application, and, intellectually, most isolated. Economics studies exchange carried out under special circumstances and with a most useful built-in numerical measure of value.

¹See R. B. Braithwaite, *Scientific Explanation* (Cambridge: Cambridge University Press, 1953).

²Translated by I. Cunnison as *The Gift* (Glen-coe, Ill.: Free Press, 1954).

³In social anthropology D. L. Oliver is working along these lines, and I owe much to him. See also T. M. Newcomb, “The Prediction of Interpersonal Attraction,” *American Psychologist*, XI (1956), 575–86.

What are the laws of the general phenomenon of which economic behavior is one class?

In what follows I shall suggest some reasons for the usefulness of a theory of social behavior as exchange and suggest the nature of the propositions such a theory might contain.

AN EXCHANGE PARADIGM

I start with the link to behavioral psychology and the kind of statement it makes about the behavior of an experimental animal such as the pigeon.⁴ As a pigeon explores its cage in the laboratory, it happens to peck a target, whereupon the psychologist feeds it corn. The evidence is that it will peck the target again; it has learned the behavior, or, as my friend Skinner says, the behavior has been reinforced, and the pigeon has undergone *operant conditioning*. This kind of psychologist is not interested in how the behavior was learned: “learning theory” is a poor name for his field. Instead, he is interested in what determines changes in the rate of emission of learned behavior, whether pecks at a target or something else.

The more hungry the pigeon, the less corn or other food it has gotten in the recent past, the more often it will peck. By the same token, if the behavior is often reinforced, if the pigeon is given much corn every time it pecks, the rate of emission will fall off as the pigeon gets *satiated*. If, on the other hand, the behavior is not reinforced at all, then, too, its rate of emission will tend to fall off, though a long time may pass before it stops altogether, before it is *extinguished*. In the emission of many kinds of behavior the pigeon incurs *aversive stimulation*, or what I shall call “cost” for short, and this, too, will lead in time to a decrease in the emission rate. Fatigue is an example of a “cost.” Extinction, satiation, and cost, by decreasing the rate of emission of a particular kind of behavior, render more probable the emission of some other kind of behavior, including doing nothing. I shall only add that even a hard-boiled psychologist puts “emotional” behavior, as well as such things as pecking, among the unconditioned responses that may be reinforced in

⁴B. F. Skinner, *Science and Human Behavior* (New York: Macmillan Co., 1953).

operant conditioning. As a statement of the propositions of behavioral psychology, the foregoing is, of course, inadequate for any purpose except my present one.

We may look on the pigeon as engaged in an exchange—pecks for corn—with the psychologist, but let us not dwell upon that, for the behavior of the pigeon hardly determines the behavior of the psychologist at all. Let us turn to a situation where the exchange is real, that is, where the determination is mutual. Suppose we are dealing with two men. Each is emitting behavior reinforced to some degree by the behavior of the other. How it was in the past that each learned the behavior he emits and how he learned to find the other's behavior reinforcing we are not concerned with. It is enough that each does find the other's behavior reinforcing, and I shall call the re-inforcers—the equivalent of the pigeon's corn—*values*, for this, I think, is what we mean by this term. As he emits behavior, each man may incur costs, and each man has more than one course of behavior open to him.

This seems to me the paradigm of elementary social behavior, and the problem of the elementary sociologist is to state propositions relating the variations in the values and costs of each man to his frequency distribution of behavior among alternatives, where the values (in the mathematical sense) taken by these variable for one man determine in part their values for the other.⁵

I see no reason to believe that the propositions of behavioral psychology do not apply to this situation, though the complexity of their implications in the concrete case may be great indeed. In particular, we must suppose that, with men as with pigeons, an increase in extinction, satiation, or aversive stimulation of any one kind of behavior will increase the probability of emission of some other kind. The problem is not, as it is often stated, merely, what a man's values are, what he has learned in the past to find reinforcing, but how much of any one value his behavior is getting him now. The more he gets, the less valuable any further unit of that value is to him, and the less often he will emit behavior reinforced by it.

⁵Ibid., pp. 297–329. The discussion of “double contingency” by T. Parsons and E. A. Shils could easily lead to a similar paradigm (see *Toward a General Theory of Action* [Cambridge, Mass.: Harvard University Press, 1951], pp. 14–16).

THE INFLUENCE PROCESS

We do not, I think, possess the kind of studies of two-person interaction that would either bear out these propositions or fail to do so. But we do have studies of larger numbers of persons that suggest that they may apply, notably the studies by Festinger, Schachter, Back, and their associates on the dynamics of influence. One of the variables they work with they call *cohesiveness*, defined as anything that attracts people to take part in a group. Cohesiveness is a value variable; it refers to the degree of reinforcement people find in the activities of the group. Festinger and his colleagues consider two kinds of reinforcing activity: the symbolic behavior we call “social approval” (sentiment) and activity valuable in other ways, such as doing something interesting.

The other variable they work with they call *communication* and others call *interaction*. This is a frequency variable; it is a measure of the frequency of emission of valuable and costly verbal behavior. We must bear in mind that, in general, the one kind of variable is a function of the other.

Festinger and his co-workers show that the more cohesive a group is, that is, the more valuable the sentiment or activity the members exchange with one another, the greater the average frequency of interaction of the members.⁶ With men, as with pigeons, the greater the reinforcement, the more often is the reinforced behavior emitted. The more cohesive a group, too, the greater the change that members can produce in the behavior of other members in the direction of rendering these activities more valuable.⁷ That is, the more valuable the activities that members get, the more valuable those that they must give. For if a person is emitting behavior of a certain kind, and other people do not find it particularly rewarding, these others will suffer their own production of sentiment and activity, in time, to fall off. But perhaps the first person has found their sentiment and activity

⁶K. W. Back, “The Exertion of Influence through Social Communication,” in L. Festinger, K. Back, S. Schachter, H. H. Kelley, and J. Thibaut (eds.), *Theory and Experiment in Social Communication* (Ann Arbor: Research Center for Dynamics, University of Michigan, 1950), pp. 21–36.

⁷S. Schachter, N. Ellertson, D. McBride, and D. Gregory, “An Experimental Study of Cohesiveness and Productivity,” *Human Relations*, IV (1951), 229–38.

rewarding? and, if he is to keep on getting them, he must make his own behavior more valuable to the others. In short, the propositions of behavioral psychology imply a tendency toward a certain proportionality between the value to others of the behavior a man gives them and the value to him of the behavior they give him.⁸

Schachter also studied the behavior of members of a group toward two kinds of other members, “conformers” and “deviates.”⁹ I assume that conformers are people whose activity the other members find valuable. For conformity is behavior that coincides to a degree with some group standard or norm, and the only meaning I can assign to *norm* is “a verbal description of behavior that many members find it valuable for the actual behavior of themselves and others to conform to.” By the same token, a deviate is a member whose behavior is not particularly valuable. Now Schachter shows that, as the members of a group come to see another member as a deviate, their interaction with him—communication addressed to getting him to change his behavior—goes up, the faster the more cohesive the group. The members need not talk to the other conformers so much; they are relatively satiated by the conformers’ behavior: they have gotten what they want out of them. But if the deviate, by failing to change his behavior, fails to reinforce the members, they start to withhold social approval from him: the deviate gets low sociometric choice at the end of the experiment. And in the most cohesive groups—those Schachter calls “high cohesive-relevant”—interaction with the deviate also falls off in the end and is lowest among those members that rejected him most strongly, as if they had given him up as a bad job. But how plonking can we get? These findings are utterly in line with everyday experience.

PRACTICAL EQUILIBRIUM

At the beginning of this paper I suggested that one of the tasks of small-group research was to show the relation between the results of experimental work done under laboratory conditions and the results of field research on real-life small groups. Now the latter often appear to be in practical equilibrium, and by this I mean nothing

⁸Skinner, *op. cit.*, p. 100.

⁹S. Schachter, “Deviation, Rejection, and Communication,” *Journal of Abnormal and Social Psychology*, XLVI (1951), 190–207.

fancy. I do not mean that all real-life groups are in equilibrium. I certainly do not mean that all groups must tend to equilibrium. I do not mean that groups have built-in antidotes to change: there is no homeostasis here. I do not mean that we assume equilibrium. I mean only that we sometimes *observe* it, that for the time we are with a group—and it is often short—there is no great change in the values of the variables we choose to measure. If, for instance, person A is interacting with B more than with C both at the beginning and at the end of the study, then at least by this crude measure the group is in equilibrium.

Many of the Festinger-Schachter studies are experimental, and their propositions about the process of influence seem to me to imply the kind of proposition that empirically holds good of real-life groups in practical equilibrium. For instance, Festinger *et al.* find that, the more cohesive a group is, the greater the change that members can produce in the behavior of other members. If the influence is exerted in the direction of conformity to group norms, then, when the process of influence has accomplished all the change of which it is capable, the proposition should hold good that, the more cohesive a group is, the larger the number of members that conform to its norms. And it does hold good.¹⁰

Again, Schachter found, in the experiment I summarized above, that in the most cohesive groups and at the end, when the effort to influence the deviate had failed, members interacted little with the deviate and gave him little in the way of sociometric choice. Now two of the propositions that hold good most often of real-life groups in practical equilibrium are precisely that the more closely a member’s activity conforms to the norms the more interaction he receives from other members and the more liking choices he gets from them too. From these main propositions a number of others may be derived that also hold good.¹¹

¹⁰L. Festinger, S. Schachter, and K. Back, *Social Pressures in Informal Groups* (New York: Harper & Bros., 1950), pp. 72–100.

¹¹For propositions holding good of groups in practical equilibrium see G. C. Homans, *The Human Group* (New York: Harcourt, Brace & Co., 1950), and H. W. Riecken and G. C. Homans, “Psychological Aspects of Social Structure,” in G. Lindzey (ed.), *Handbook of Social Psychology* (Cambridge, Mass.: Addison-Wesley Publishing Co., 1954), II, 786–832.

Yet we must ever remember that the truth of the proposition linking conformity to liking may on occasion be masked by the truth of other propositions. If, for instance, the man that conforms to the norms most closely also exerts some authority over the group, this may render liking for him somewhat less than it might otherwise have been.¹²

Be that as it may, I suggest that the laboratory experiments on influence imply propositions about the behavior of members of small groups, when the process of influence has worked itself out, that are identical with propositions that hold good of real-life groups in equilibrium. This is hardly surprising if all we mean by equilibrium is that all the change of which the system is, under present conditions, capable has been effected, so that no further change occurs. Nor would this be the first time that statics has turned out to be a special case of dynamics.

PROFIT AND SOCIAL CONTROL

Though I have treated equilibrium as an observed fact, it is a fact that cries for explanation. I shall not, as structural-functional sociologists do, use an assumed equilibrium as a means of explaining, or trying to explain, why the other features of a social system should be what they are. Rather, I shall take practical equilibrium as something that is itself to be explained by the other features of the system.

If every member of a group emits at the end of, and during, a period of time much the same kinds of behavior and in much the same frequencies as he did at the beginning, the group is for that period in equilibrium. Let us then ask why any one member's behavior should persist. Suppose he is emitting behavior of value A_1 . Why does he not let his behavior get worse (less valuable or reinforcing to the others) until it stands at $A_1 - DA$? True, the sentiments expressed by others toward him are apt to decline in value (become less reinforcing to him), so that what he gets from them may be $S_1 - DS$. But it is conceivable that, since most activity carries cost, a

¹² See Homans, *op. cit.*, pp. 244–48, and R. F. Bales, "The Equilibrium Problem in Small Groups," in A. P. Hare, E. F. Borgatta, and R. F. Bales (eds.), *Small Groups* (New York: A. A. Knopf, 1953), pp. 450–56.

decline in the value of what he emits will mean a reduction in cost to him that more than offsets his losses in sentiment. Where, then, does he stabilize his behavior? This is the problem of social control.¹³

Mankind has always assumed that a person stabilizes his behavior, at least in the short run, at the point where he is doing the best he can for himself under the circumstances, though his best may not be a "rational" best, and what he can do may not be at all easy to specify, except that he is not apt to think like one of the theoretical antagonists in the *Theory of Games*. Before a sociologist rejects this answer out of hand for its horrid profit-seeking implications, he will do well to ask himself if he can offer any other answer to the question posed. I think he will find that he cannot. Yet experiments designed to test the truth of the answer are extraordinarily rare.

I shall review one that seems to me to provide a little support for the theory, though it was not meant to do so. The experiment is reported by H. B. Gerard, a member of the Festinger-Schachter team, under the title "The Anchorage of Opinions in Face-to-Face Groups."¹⁴ The experimenter formed artificial groups whose members met to discuss a case in industrial relations and to express their opinions about its probable outcome. The groups were of two kinds: high-attraction groups, whose members were told that they would like one another very much, and low-attraction groups, whose members were told that they would not find one another particularly likable.

At a later time the experimenter called the members in separately, asked them again to express their opinions on the outcome of the case, and counted the number that had changed their opinions to bring them into accord with those of other members of their groups. At the same time, a paid participant entered into a further discussion of the case with each member, always taking, on the probable outcome of the case, a position opposed to that taken by the bulk of the other members of the group to which the person belonged. The experimenter counted the number of persons shifting toward the opinion of the paid participant.

¹³ Homans, *op. cit.*, pp. 281–301.

¹⁴ *Human Relations*, VII (1954), 313–25.

TABLE 1 Percentage of Subjects Changing toward Someone in the Group

	<i>Agreement</i>	<i>Mild Disagreement</i>	<i>Strong Disagreement</i>
High attraction. . . .	0	12	44
Low attraction. . . .	0	15	9

TABLE 2 Percentage of Subjects Changing toward the Paid Participant

	<i>Agreement</i>	<i>Mild Disagreement</i>	<i>Strong Disagreement</i>
High attraction. . . .	7	13	25
Low attraction. . . .	20	38	8

The experiment had many interesting results, from which I choose only those summed up in Tables 1 and 2. The three different agreement classes are made up of people who, at the original sessions, expressed different degrees of agreement with the opinions of other members of their groups. And the figure 44, for instance, means that, of all members of high-attraction groups whose initial opinions were strongly in disagreement with those of other members, 44 per cent shifted their opinion later toward that of others.

In these results the experimenter seems to have been interested only in the differences in the sums of the rows, which show that there is more shifting toward the group, and less shifting toward the paid participant, in the high-attraction than in the low-attraction condition. This is in line with a proposition suggested earlier. If you think that the members of a group can give you much—in this case, liking—you are apt to give them much—in this case, a change to an opinion in accordance with their views—or you will not get the liking. And, by the same token, if the group can give you little of value, you will not be ready to give it much of value. Indeed, you may change your opinion so as to depart from agreement even further, to move, that is, toward the view held by the paid participant.

So far so good, but, when I first scanned these tables, I was less struck by the difference between them than by their similarity. The same classes of people in both tables showed much the same relative propensities to change their opinions, no

matter whether the change was toward the group or toward the paid participant. We see, for instance, that those who change least are the high-attraction, agreement people and the low-attraction, strong-disagreement ones. And those who change most are the high-attraction, strong-disagreement people and the low-attraction, mild-disagreement ones.

How am I to interpret these particular results? Since the experimenter did not discuss them, I am free to offer my own explanation. The behavior emitted by the subjects is opinion and changes in opinion. For this behavior they have learned to expect two possible kinds of reinforcement. Agreement with the group gets the subject favorable sentiment (acceptance) from it, and the experiment was designed to give this reinforcement a higher value in the high-attraction condition than in the low-attraction one. The second kind of possible reinforcement is what I shall call the “maintenance of one’s personal integrity,” which a subject gets by sticking to his own opinion in the face of disagreement with the group. The experimenter does not mention this reward, but I cannot make sense of the results without something much like it. In different degrees for different subjects, depending on their initial positions, these rewards are in competition with one another: they are alternatives. They are not absolutely scarce goods, but some persons cannot get both at once.

Since the rewards are alternatives, let me introduce a familiar assumption from economics—that the cost of a particular course of action

is the equivalent of the foregone value of an alternative¹⁵—and then add the definition.

Now consider the persons in the corresponding cells of the two tables. The behavior of the high-attraction, agreement people gets them much in the way of acceptance by the group, and for it they must give up little in the way of personal integrity, for their views are from the start in accord with those of the group. Their profit is high, and they are not prone to change their behavior. The low-attraction, strong-disagreement people are getting much in integrity, and they are not giving up for it much in valuable acceptance, for they are members of low-attraction groups. Reward less cost is high for them, too, and they change little. The high-attraction, strong-disagreement people are getting much in the way of integrity, but their costs in doing so are high, too, for they are in high-attraction groups and thus foregoing much valuable acceptance by the group. Their profit is low, and they are very apt to change, either toward the group or toward the paid participant, from whom they think, perhaps, they will get some acceptance while maintaining some integrity. The low-attraction, mild-disagreement people do not get much in the way of integrity, for they are only in mild disagreement with the group, but neither are they giving up much in acceptance, for they are members of low-attraction groups. Their rewards are low; their costs are low too, and their profit—the difference between the two—is also low. In their low profit they resemble the high-attraction, strong-disagreement people, and, like them, they are prone to change their opinions, in this case, more toward the paid participant. The subjects in the other two cells, who have medium profits, display medium propensities to change.

If we define profit as reward less cost, and if cost is value foregone, I suggest that we have here some evidence for the proposition that change in behavior is greatest when perceived profit is least. This constitutes no direct demonstration that change in behavior is least when profit is greatest, but if, whenever a man's behavior brought him a balance of reward and cost, he changed his behavior away from what got him, under the circumstances, the less profit, there

might well come a time when his behavior would not change further. That is, his behavior would be stabilized, at least for the time being. And, so far as this were true for every member of a group, the group would have a social organization in equilibrium.

I do not say that a member would stabilize his behavior at the point of greatest conceivable profit to himself, because his profit is partly at the mercy of the behavior of others. It is a commonplace that the short-run pursuit of profit by several persons often lands them in positions where all are worse off than they might conceivably be. I do not say that the paths of behavioral change in which a member pursues his profit under the condition that others are pursuing theirs too are easy to describe or predict; and we can readily conceive that in jockeying for position they might never arrive at any equilibrium at all.

DISTRIBUTIVE JUSTICE

Yet practical equilibrium is often observed, and thus some further condition may make its attainment, under some circumstance, more probable than would the individual pursuit of profit left to itself. I can offer evidence for this further condition only in the behavior of subgroups and not in that of individuals. Suppose that there are two subgroups, working close together in a factory, the job of one being somewhat different from that of the other. And suppose that the members of the first complain and say: "We are getting the same pay as they are. We ought to get just a couple of dollars a week more to show that our work is more responsible." When you ask them what they mean by "more responsible," they say that, if they do their work wrong, more damage can result, and so they are under more pressure to take care.¹⁶ Something like this is a common feature of industrial behavior. It is at the heart of disputes not over absolute wages but over wage differentials—indeed, at the heart of disputes over rewards other than wages.

In what kind of proposition may we express observations like these? We may say that wages and

¹⁵G. J. Stigler, *The Theory of Price* (rev. ed.; New York: Macmillan Co., 1952), p. 99.

¹⁶G. C. Homans, "Status among Clerical Workers," *Human Organization*, XII (1953), 5–10.

responsibility give status in the group, in the sense that a man who takes high responsibility and gets high wages is admired, other things equal. Then, if the members of one group score higher on responsibility than do the members of another, there is a felt need on the part of the first to score higher on pay too. There is a pressure, which shows itself in complaints, to bring the *status factors*, as I have called them, into line with one another. If they are in line, a condition of *status congruence* is said to exist. In this condition the workers may find their jobs dull or irksome, but they will not complain about the relative position of groups.

But there may be a more illuminating way of looking at the matter. In my example I have considered only responsibility and pay, but these may be enough, for they represent the two kinds of thing that come into the problem. Pay is clearly a reward; responsibility may be looked on, less clearly, as a cost. It means constraint and worry—or peace of mind foregone. Then the proposition about status congruence becomes this: If the costs of the members of one group are higher than those of another, distributive justice requires that their rewards should be higher too. But the thing works both ways: If the rewards are higher, the costs should be higher too. This last is the theory of *noblesse oblige*, which we all subscribe to, though we all laugh at it, perhaps because the *noblesse* often fails to *oblige*. To put the matter in terms of profit: though the rewards and costs of two persons or the members of two groups may be different, yet the profits of the two—the excess of reward over cost—should tend to equality. And more than “should.” The less-advantaged group will at least try to attain greater equality, as, in the example I have used, the first group tried to increase its profit by increasing its pay.

I have talked of distributive justice. Clearly, this is not the only condition determining the actual distribution of rewards and costs. At the same time, never tell me that notions of justice are not a strong influence on behavior, though we sociologists often neglect them. Distributive justice may be one of the conditions of group equilibrium.

EXCHANGE AND SOCIAL STRUCTURE

I shall end by reviewing almost the only study I am aware of that begins to show in detail how a

stable and differentiated social structure in a real-life group might arise out of a process of exchange between members. This is Peter Blau's description of the behavior of sixteen agents in a federal law-enforcement agency.¹⁷

The agents had the duty of investigating firms and preparing reports on the firms' compliance with the law. Since the reports might lead to legal action against the firms, the agents had to prepare them carefully, in the proper form, and take strict account of the many regulations that might apply. The agents were often in doubt what they should do, and then they were supposed to take the question to their supervisor. This they were reluctant to do, for they naturally believed that thus confessing to him their inability to solve a problem would reflect on their competence, affect the official ratings he made of their work, and so hurt their chances for promotion. So agents often asked other agents for help and advice, and, though this was nominally forbidden, the supervisor usually let it pass.

Blau ascertained the ratings the supervisor made of the agents, and he also asked the agents to rate one another. The two opinions agreed closely. Fewer agents were regarded as highly competent than were regarded as of middle or low competence; competence, or the ability to solve technical problems, was a fairly scarce good. One or two of the more competent agents would not give help and advice when asked, and so received few interactions and little liking. A man that will not exchange, that will not give you what he has when you need it, will not get from you the only thing you are, in this case, able to give him in return, your regard.

But most of the more competent agents were willing to give help, and of them Blau says:

A consultation can be considered an exchange of values: both participants gain something, and both have to pay a price. The questioning agent is enabled to perform better than he could otherwise have done, without exposing his difficulties to his supervisor. By asking for advice, he implicitly pays his respect to the superior proficiency of his colleague. This acknowledgment of inferiority is the cost of receiving assistance. The consultant gains prestige, in return for which he is willing to

¹⁷ Peter M. Blau, *The Dynamics of Bureaucracy* (Chicago: University of Chicago Press, 1955), 99–116.

devote some time to the consultation and permit it to disrupt his own work. The following remark of an agent illustrates this: "I like giving advice. It's flattering, I suppose, if you feel that others come to you for advice."¹⁸

Blau goes on to say: "All agents liked being consulted, but the value of any one of very many consultations became deflated for experts, and the price they paid in frequent interruptions became inflated."¹⁹ This implies that, the more prestige an agent received, the less was the increment of value of that prestige; the more advice an agent gave, the greater was the increment of cost of that advice, the cost lying precisely in the foregone value of time to do his own work. Blau suggests that something of the same sort was true of an agent who went to a more competent colleague for advice: the more often he went, the more costly to him, in feelings of inferiority, became any further request. "The repeated admission of his inability to solve his own problems . . . undermined the self-confidence of the worker and his standing in the group."²⁰

The result was that the less competent agents went to the more competent ones for help less often than they might have done if the costs of repeated admissions of inferiority had been less high and that, while many agents sought out the few highly competent ones, no single agent sought out the latter much. Had they done so (to look at the exchange from the other side), the costs to the highly competent in interruptions to their own work would have become exorbitant. Yet the need of the less competent for help was still not fully satisfied. Under these circumstances they tended to turn for help to agents more nearly like themselves in competence. Though the help they got was not the most valuable, it was of a kind they could themselves return on occasion. With such agents they could exchange help and liking, without the exchange becoming on either side too great a confession of inferiority.

The highly competent agents tended to enter into exchanges, that is, to interact with many others. But, in the more equal exchanges I have

just spoken of, less competent agents tended to pair off as partners. That is, they interacted with a smaller number of people, but interacted often with these few. I think I could show why pair relations in these more equal exchanges would be more economical for an agent than a wider distribution of favors. But perhaps I have gone far enough. The final pattern of this social structure was one in which a small number of highly competent agents exchanged advice for prestige with a large number of others less competent and in which the less competent agents exchanged, in pairs and in trios, both help and liking on more nearly equal terms.

Blau shows, then, that a social structure in equilibrium might be the result of a process of exchanging behavior rewarding and costly in different degrees, in which the increment of reward and cost varied with the frequency of the behavior, that is, with the frequency of interaction. Note that the behavior of the agents seems also to have satisfied my second condition of equilibrium: the more competent agents took more responsibility for the work, either their own or others', than did the less competent ones, but they also got more for it in the way of prestige. I suspect that the same kind of explanation could be given for the structure of many "informal" groups.

SUMMARY

The current job of theory in small-group research is to make the connection between experimental and real-life studies, to consolidate the propositions that empirically hold good in the two fields, and to show how these propositions might be derived from a still more general set. One way of doing this job would be to revive and make more rigorous the oldest of theories of social behavior—social behavior as exchange.

Some of the statements of such a theory might be the following. Social behavior is an exchange of goods, material goods but also non-material ones, such as the symbols of approval or prestige. Persons that give much to others try to get much from them, and persons that get much from others are under pressure to give much to them. This process of influence tends to work out at equilibrium to a balance in the exchanges. For a person

¹⁸ *Ibid.*, p. 108.

¹⁹ *Ibid.*, p. 108.

²⁰ *Ibid.*, p. 109.

engaged in exchange, what he gives may be a cost to him, just as what he gets may be a reward, and his behavior changes less as profit, that is, reward less cost, tends to a maximum. Not only does he seek a maximum for himself, but he tries to see to it that no one in his group makes more profit than he does. The cost and the value of what he gives and of what he gets vary with the quantity of what he gives and gets. It is surprising how familiar these propositions are; it is surprising, too, how propositions about the dynamics of exchange can begin to generate the static thing we call "group structure" and, in so doing, generate also some of the propositions about group

structure that students of real-life groups have stated.

In our unguarded moments we sociologists find words like "reward" and "cost" slipping into what we say. Human nature will break in upon even our most elaborate theories. But we seldom let it have its way with us and follow up systematically what these words imply.²¹ Of all our many "approaches" to social behavior, the one that sees it as an economy is the most neglected, and yet it is the one we use every moment of our lives—except when we write sociology.

²¹ *The White-Collar Job* (Ann Arbor: Survey Research Center, University of Michigan, 1953), pp. 115–27.

THE OVERSOCIALIZED CONCEPTION OF MAN IN MODERN SOCIOLOGY*

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Sociological theory originates in the asking of general questions about man and society. The answers lose their meaning if they are elaborated without reference to the questions, as has been the case in much contemporary theory. An example is the Hobbesian question of how men become tractable to social controls. The two-fold answer of contemporary theory is that man "internalizes" social norms and seeks a favorable self-image by conforming to the "expectations" of others. Such a model of man denies the very possibility of his being anything but a thoroughly socialized being and thus denies the reality of the Hobbesian question. The Freudian view of man, on the other hand, which sociologists have misrepresented, sees man as a social though never a fully socialized creature. Sociologists need to develop a more complex, dialectical conception of human nature instead of relying on an implicit conception that is tailor-made for special sociological problems.

Gertrude Stein, bed-ridden with a fatal illness, is reported to have suddenly muttered, "What, then, is the answer?" Pausing, she raised her head, murmured, "But what is the question?" and died. Miss Stein presumably was pondering the ultimate meaning of human life, but her brief final soliloquy has a broader and humbler relevance. Its point is that answers are meaningless

apart from questions. If we forget the questions, even while remembering the answers, our knowledge of them will subtly deteriorate, becoming rigid, formal, and catechistic as the sense of indeterminacy, of rival possibilities, implied by the very putting of a question is lost.

Social theory must be seen primarily as a set of answers to questions we ask of social reality. If

*This is a slightly revised version of a paper read at the meetings of the American Sociological Association in New York City, August 30, 1960.

the initiating questions are forgotten, we readily misconstrue the task of theory and the answers previous thinkers have given become narrowly confining conceptual prisons, degenerating into little more than a special, professional vocabulary applied to situations and events that can be described with equal or greater precision in ordinary language. Forgetfulness of the questions that are the starting points of inquiry leads us to ignore the substantive assumptions “buried” in our concepts and commits us to a one-sided view of reality.

Perhaps this is simply an elaborate way of saying that sociological theory can never afford to lose what is usually called a “sense of significance;” or, as it is sometimes put, that sociological theory must be “problem-conscious.” I choose instead to speak of theory as a set of answers to questions because reference to “problems” may seem to suggest too close a linkage with social criticism or reform. My primary reason for insisting on the necessity of holding constantly in mind the questions that our concepts and theories are designed to answer is to preclude defining the goal of sociological theory as the creation of a formal body of knowledge satisfying the logical criteria of scientific theory set up by philosophers and methodologists of natural science. Needless to say, this is the way theory is often defined by contemporary sociologists.

Yet to speak of theory as interrogatory may suggest too self-sufficiently intellectual an enterprise. Cannot questions be satisfactorily answered and then forgotten, the answers becoming the assumptions from which we start in framing new questions? It may convey my view of theory more adequately to say that sociological theory concerns itself with questions arising out of problems that are inherent in the very existence of human societies and that cannot therefore be finally “solved” in the way that particular social problems perhaps can be. The “problems” theory concerns itself with are problems *for* human societies which, because of their universality, become intellectually problematic for sociological theorists.

Essentially, the historicist conception of sociological knowledge that is central to the thought of Max Weber and has recently been ably restated

by Barrington Moore, Jr. and C. Wright Mills¹ is a sound one. The most fruitful questions for sociology are always questions referring to the realities of a particular historical situation. Yet both of these writers, especially Mills, have a tendency to underemphasize the degree to which we genuinely wish and seek answers to trans-historical and universal questions about the nature of man and society. I do not, let it be clear, have in mind the formalistic quest for social “laws” or “universal propositions,” nor the even more formalistic effort to construct all-encompassing “conceptual schemes.” Moore and Mills are rightly critical of such efforts. I am thinking of such questions as, “How are men capable of uniting to form enduring societies in the first place?”; “Why and to what degree is change inherent in human societies and what are the sources of change?”; “How is man’s animal nature domesticated by society?”

Such questions—and they are existential as well as intellectual questions—are the *raison d’être* of social theory. They were asked by men long before the rise of sociology. Sociology itself is an effort, under new and unprecedented historical conditions, to find novel answers to them. They are not questions which lend themselves to successively more precise answers as a result of cumulative empirical research, for they remain eternally problematic. Social theory is necessarily an interminable dialogue. “True understanding,” Hannah Arendt has written, “does not tire of interminable dialogue and ‘vicious circles’ because it trusts that imagination will eventually catch at least a glimpse of the always frightening light of truth.”²

I wish briefly to review the answers modern sociological theory offers to one such question, or rather to one aspect of one question. The question may be variously phrased as, “What are the sources of social cohesion?”; or, “How is social order possible?”; or, stated in social-psychological terms, “How is it that man becomes tractable to social

¹Barrington Moore, Jr., *Political Power and Social Theory*, Cambridge: Harvard University Press, 1958; C. Wright Mills, *The Sociological Imagination*, New York: Oxford University Press, 1959.

²Hannah Arendt, “Understanding and Politics,” *Partisan Review*, 20 (July–August, 1953), p. 392. For a view of social theory close to the one adumbrated in the present paper, see Theodore Abel, “The Present Status of Social Theory,” *American Sociological Review*, 17 (April, 1952), pp. 156–164.

discipline?" I shall call this question in its social-psychological aspect the "Hobbesian question" and in its more strictly sociological aspect the "Marxist question." The Hobbesian question asks how men are capable of the guidance by social norms and goals that makes possible an enduring society, while the Marxist question asks how, assuming this capability, complex societies manage to regulate and restrain destructive conflicts between groups. Much of our current theory offers an oversocialized view of man in answering the Hobbesian question and an over-integrated view of society in answering the Marxist question.

A number of writers have recently challenged the overintegrated view of society in contemporary theory. In addition to Moore and Mills, the names of Bendix, Coser, Dahrendorf, and Lockwood come to mind.³ My intention, therefore, is to concentrate on the answers to the Hobbesian question in an effort to disclose the oversocialized view of man which they seem to imply.

Since my view of theory is obviously very different from that of Talcott Parsons and has, in fact, been developed in opposition to his, let me pay tribute to his recognition of the importance of the Hobbesian question—the "problem of order," as he calls it—at the very beginning of his first book, *The Structure of Social Action*.⁴ Parsons correctly credits Hobbes with being the first thinker to see the necessity of explaining why human society is not a "war of all against all;" why, if man is simply a gifted animal, men refrain from unlimited resort to fraud and violence in pursuit of their ends and maintain a stable society at all. There is even a sense in which, as Coser and Mills have both noted,⁵ Parsons' entire work represents an effort to solve the Hobbesian problem of order. His solution, however, has tended to become precisely the

kind of elaboration of a set of answers in abstraction from questions that is so characteristic of contemporary sociological theory.

We need not be greatly concerned with Hobbes' own solution to the problem of order he saw with such unsurpassed clarity. Whatever interest his famous theory of the origin of the state may still hold for political scientists, it is clearly inadequate as an explanation of the origin of society. Yet the pattern as opposed to the details of Hobbes' thought bears closer examination.

The polar terms in Hobbes' theory are the state of nature, where the war of all against all prevails, and the authority of Leviathan, created by social contract. But the war of all against all is not simply effaced with the creation of political authority: it remains an ever-present potentiality in human society, at times quiescent, at times erupting into open violence. Whether Hobbes believed that the state of nature and the social contract were ever historical realities—and there is evidence that he was not that simple-minded and unsociological, even in the seventeenth century—is unimportant; the whole tenor of his thought is to see the war of all against all and Leviathan dialectically, as coexisting and interacting opposites.⁶ As R. G. Collingwood has observed, "According to Hobbes . . . *a body politic is a dialectical thing*, a Heraclitean world in which at any given time there is a negative element."⁷ The first secular social theorist in the history of Western thought, and one of the first clearly to discern and define the problem of order in human society long before Darwinism made awareness of it a commonplace, Hobbes was a dialectical thinker who refused to separate answers from questions, solutions to society's enduring problems from the conditions creating the problems.

³ Reinhard Bendix and Bennett Berger, "Images of Society and Problems of Concept Formation in Sociology," in Llewellyn Gross, editor, *Symposium on Sociological Theory*, Evanston, Ill.: Row, Petersen & Co., 1959, pp. 92–118; Lewis A. Coser, *The Functions of Social Conflict*, Glencoe, Ill.: The Free Press, 1956; Ralf Dahrendorf, "Out of Utopia: Towards a Re-Oriented Sociological Analysis," *American Journal of Sociology*, 64 (September, 1958), pp. 115–127; and *Class and Class Conflict in Industrial Society*, Stanford, Calif.: Stanford University Press, 1959; David Lockwood, "Some Remarks on 'The Social System,'" *British Journal of Sociology*, 7 (June, 1956), pp. 134–146.

⁴ Talcott Parsons, *The Structure of Social Action*, New York: McGraw-Hill Book Co., 1937, pp. 89–94.

⁵ Coser, *op. cit.*, p. 21; Mills, *op. cit.*, p. 44.

⁶ A recent critic of Parsons follows Hobbes in seeing the relation between the normative order in society and what he calls "the sub-stratum of social action" and other sociologists have called the "factual order" as similar to the relation between the war of all against all and the authority of the state. David Lockwood writes: "The existence of the normative order . . . is in one very important sense inextricably bound up with potential conflicts of interest over scarce resources . . . ; the very existence of a normative order mirrors the continual potentiality of conflict." Lockwood, *op. cit.*, p. 137.

⁷ R. G. Collingwood, *The New Leviathan*, Oxford: The Clarendon Press, 1942, p. 183.

What is the answer of contemporary sociological theory to the Hobbesian question? There are two main answers, each of which has come to be understood in a way that denies the reality and meaningfulness of the question. Together they constitute a model of human nature, sometimes clearly stated, more often implicit in accepted concepts, that pervades modern sociology. The first answer is summed up in the notion of the “internalization of social norms.” The second, more commonly employed or assumed in empirical research, is the view that man is essentially motivated by the desire to achieve a positive image of self by winning acceptance or status in the eyes of others.

The following statement represents, briefly and broadly, what is probably the most influential contemporary sociological conception—and dismissal—of the Hobbesian problem: “To a modern sociologist imbued with the conception that action follows institutionalized patterns, opposition of individual and common interests has only a very limited relevance or is thoroughly unsound.”⁸ From this writer’s perspective, the problem is an unreal one: human conduct is totally shaped by common norms or “institutionalized patterns.” Sheer ignorance must have led people who were unfortunate enough not to be modern sociologists to ask, “How is order possible?” A thoughtful bee or ant would never inquire, “How is the social order of the hive or ant-hill possible?” for the opposite of that order is unimaginable when the instinctive endowment of the insects ensures its stability and built-in harmony between “individual and common interests.” Human society, we are assured, is not essentially different, although conformity and

⁸Francis X. Sutton and others, *The American Business Creed*, Cambridge: Harvard University Press, 1956, p. 304. I have cited this study and, on several occasions, textbooks and fugitive articles rather than better-known and directly theoretical writings because I am just as concerned with what sociological concepts and theories are taken to mean when they are actually used in research, teaching, and introductory exposition as with their elaboration in more self-conscious and explicitly theoretical discourse. Since the model of human nature I am criticizing is partially implicit and “buried” in our concepts, cruder and less qualified illustrations are as relevant as the formulations of leading theorists. I am also aware that some older theorists, notably Cooley and MacIver, were shrewd and worldly-wise enough to reject the implication that man is ever fully socialized. Yet they failed to develop competing images of man which were concise and systematic enough to counter the appeal of the oversocialized models.

stability are there maintained by non-instinctive processes. Modern sociologists believe that they have understood these processes and that they have not merely answered but disposed of the Hobbesian question, showing that, far from expressing a valid intimation of the tensions and possibilities of social life, it can only be asked out of ignorance.

It would be hard to find a better illustration of what Collingwood, following Plato, calls *eristical* as opposed to dialectical thinking;⁹ the answer destroys the question, or rather destroys the awareness of rival possibilities suggested by the question which accounts for its having been asked in the first place. A reversal of perspective now takes place and we are moved to ask the opposite question: “How is it that violence, conflict, revolution, and the individual’s sense of coercion by society manage to exist at all, if this view is correct?”¹⁰ Whenever a one-sided answer to a question compels us to raise the opposite question, we are caught up in a dialectic of concepts which reflects a dialectic in things. But let us examine the particular processes sociologists appeal to in order to account for the elimination from human society of the war of all against all.

THE CHANGING MEANING OF INTERNALIZATION

A well-known section of *The Structure of Social Action*, devoted to the interpretation of Durkheim’s thought, is entitled “The Changing Meaning of Constraint.”¹¹ Parsons argues that Durkheim originally conceived of society as controlling the individual from the outside by imposing constraints on him through sanctions, best illustrated by codes of law. But in Durkheim’s later work he began to see that social rules do not “merely regulate ‘externally’ . . . they enter directly into the constitution of the actors’ ends

⁹Collingwood, *op. cit.*, pp. 181–182.

¹⁰Cf. Mills, *op. cit.*, pp. 32–33, 42. While Mills does not discuss the use of the concept of internalization by Parsonian theorists, I have argued elsewhere that his view of the relation between power and values is insufficiently dialectical. See Dennis H. Wrong, “The Failure of American Sociology,” *Commentary*, 28 (November, 1959), p. 378.

¹¹Parsons, *op. cit.*, pp. 378–390.

themselves.”¹² Constraint, therefore, is more than an environmental obstacle which the actor must take into account in pursuit of his goals in the same way that he takes into account physical laws: it becomes internal, psychological, and self-imposed as well. Parsons developed this view that social norms are constitutive rather than merely regulative of human nature before he was influenced by psychoanalytic theory, but Freud’s theory of the superego has become the source and model for the conception of the internalization of social norms that today plays so important a part in sociological thinking. The use some sociologists have made of Freud’s idea, however, might well inspire an essay entitled, “The Changing Meaning of Internalization,” although, in contrast to the shift in Durkheim’s view of constraint, this change has been a change for the worse.

What has happened is that internalization has imperceptibly been equated with “learning,” or even with “habit-formation” in the simplest sense. Thus when a norm is said to have been “internalized” by an individual, what is frequently meant is that he habitually both affirms it and conforms to it in his conduct. The whole stress on inner conflict, on the tension between powerful impulses and superego controls the behavioral outcome of which cannot be prejudged, drops out of the picture. And it is this that is central to Freud’s view, for in psychoanalytic terms to say that a norm has been internalized, or introjected to become part of the superego, is to say no more than that a person will suffer guilt-feelings if he fails to live up to it, not that he will in fact live up to it in his behavior.

The relation between internalization and conformity assumed by most sociologists is suggested by the following passage from a recent, highly-praised advanced textbook: “Conformity to institutionalized norms is, of course, ‘normal.’ The actor, having internalized the norms, feels something like a need to conform. His conscience would bother him if he did not.”¹³ What is overlooked here is that the person who conforms may be even more “bothered,” that is, subject to guilt and neurosis, than the person

¹² *Ibid.*, p. 382.

¹³ Harry M. Johnson, *Sociology: A Systematic Introduction*, New York: Harcourt, Brace and Co., 1960, p. 22.

who violates what are not only society’s norms but his own as well. To Freud, it is precisely the man with the strictest superego, he who has most thoroughly internalized and conformed to the norms of his society, who is most wracked with guilt and anxiety.¹⁴

Paul Kecskemeti, to whose discussion I owe initial recognition of the erroneous view of internalization held by sociologists, argues that the relations between social norms, the individual’s selection from them, his conduct, and his feelings about his conduct are far from self-evident. “It is by no means true,” he writes, “to say that acting counter to one’s own norms always or almost always leads to neurosis. One might assume that neurosis develops even more easily in persons who *never* violate the moral code they recognize as valid but repress and frustrate some strong instinctual motive. A person who ‘succumbs to temptation,’ feels guilt, and then ‘purges himself’ of his guilt in some reliable way (e.g., by confession) may achieve in this way a better balance, and be less neurotic, than a person who never violates his ‘norms’ and never feels conscious guilt.”¹⁵

Recent discussions of “deviant behavior” have been compelled to recognize these distinctions between social demands, personal attitudes towards them, and actual conduct, although they have done so in a laboriously taxonomic fashion.¹⁶ They represent, however, largely the rediscovery of what was always central to the Freudian concept of the superego. The main explanatory function of the concept is to show how people repress themselves, imposing checks on their own desires and thus turning the inner life into a battlefield of conflicting motives, no matter which side “wins,” by successfully dictating overt action. So far as behavior is concerned, the psychoanalytic view of man is less deterministic than the sociological. For psychoanalysis is primarily concerned with the inner life, not with overt behavior, and

¹⁴ Sigmund Freud, *Civilization and Its Discontents*, New York: Doubleday Anchor Books, 1958, pp. 80–81.

¹⁵ Paul Kecskemeti, *Meaning, Communication, and Value*, Chicago: University of Chicago Press, 1952, pp. 244–245.

¹⁶ Robert Dubin, “Deviant Behavior and Social Structure: Continuities in Social Theory,” *American Sociological Review*, 24 (April, 1959), pp. 147–164; Robert K. Merton, “Social Conformity, Deviation, and Opportunity Structures: A Comment on the Contributions of Dubin and Cloward,” *Ibid.*, pp. 178–189.

its most fundamental insight is that the wish, the emotion, and the fantasy are as important as the act in man's experience.

Sociologists have appropriated the superego concept, but have separated it from any equivalent of the Freudian id. So long as most individuals are "socialized," that is, internalize the norms and conform to them in conduct, the Hobbesian problem is not even perceived as a latent reality. Deviant behavior is accounted for by special circumstances: ambiguous norms, anomie, role conflict, or greater cultural stress on valued goals than on the approved means for attaining them. Tendencies to deviant behavior are not seen as dialectically related to conformity. The presence in man of motivational forces bucking against the hold social discipline has over him is denied.

Nor does the assumption that internalization of norms and roles is the essence of socialization allow for a sufficient range of motives underlying conformity. It fails to allow for variable "tonicity of the superego," in Kardiner's phrase.¹⁷ The degree to which conformity is frequently the result of coercion rather than conviction is minimized.¹⁸ Either someone has internalized the norms, or he is "unsocialized," a feral or socially isolated child, or a psychopath. Yet Freud recognized that many people, conceivably a majority, fail to acquire superegos. "Such people," he wrote, "habitually permit themselves to do any bad deed that procures them something they want, if only they are sure that no authority will discover it or make them suffer for it; their anxiety relates only to the possibility of detection. Present-day society has to take into account the prevalence of this state of mind."¹⁹ The last sentence suggests that Freud was aware of the decline of "inner-direction," of the Protestant conscience, about which we have heard so much lately. So let us turn to the other elements of human nature that sociologists appeal to in order to explain, or rather explain away, the Hobbesian problem.

¹⁷ Abram Kardiner, *The Individual and His Society*, New York: Columbia University Press, 1939, pp. 65, 72–75.

¹⁸ Mills, *op. cit.*, pp. 39–41; Dahrendorf, *Class and Class Conflict in Industrial Society*, pp. 157–165.

¹⁹ Freud, *op. cit.*, pp. 78–79.

MAN THE ACCEPTANCE-SEEKER²⁰

The superego concept is too inflexible, too bound to the past and to individual biography, to be of service in relating conduct to the pressures of the immediate situation in which it takes place. Sociologists rely more heavily therefore on an alternative notion, here stated—or, to be fair, overstated—in its baldest form: "People are so profoundly sensitive to the expectations of others that all action is inevitably guided by these expectations."²¹

Parsons' model of the "complementarity of expectations," the view that in social interaction men mutually seek approval from one another by conforming to shared norms, is a formalized version of what has tended to become a distinctive sociological perspective on human motivation. Ralph Linton states it in explicit psychological terms: "The need for eliciting favorable responses from others is an almost constant component of [personality]. Indeed, it is not too much to say that there is very little organized human behavior which is not directed toward its satisfaction in at least some degree."²²

The insistence of sociologists on the importance of "social factors" easily leads them to stress the

²⁰ In many ways I should prefer to use the neater, more alliterative phrase "status-seeker." However, it has acquired a narrower meaning than I intend, particularly since Vance Packard appropriated it, suggesting primarily efforts, which are often consciously deceptive, to give the appearance of personal achievements or qualities worthy of deference. "Status-seeking" in this sense is, as Veblen perceived, necessarily confined to relatively impersonal and segmental social relationships. "Acceptance" or "approval" convey more adequately what all men are held to seek in both intimate and impersonal relations according to the conception of the self and of motivation dominating contemporary sociology and social psychology. I have, nevertheless, been unable to resist the occasional temptation to use the term "status" in this broader sense.

²¹ Sutton and others, *op. cit.*, p. 264. Robert Cooley Angell, in *Free Society and Moral Crisis*, Ann Arbor: University of Michigan Press, 1958, p. 34, points out the ambiguity of the term "expectations." It is used, he notes, to mean both a factual prediction and a moral imperative, e.g. "England expects every man to do his duty." But this very ambiguity is instructive, for it suggests the process by which behavior that is non-normative and perhaps even "deviant" but nevertheless "expected" in the sense of being predictable, acquires over time a normative aura and becomes "expected" in the second sense of being socially approved or demanded. Thus Parsons' "interaction paradigm" provides leads to the understanding of social change and need not be confined, as in his use of it, to the explanation of conformity and stability. But this is the subject of another paper I hope to complete shortly.

²² Ralph Linton, *The Cultural Background of Personality*, New York: Appleton-Century Co., 1945, p. 91.

priority of such socialized or socializing motives in human behavior.²³ It is frequently the task of the sociologist to call attention to the intensity with which men desire and strive for the good opinion of their immediate associates in a variety of situations, particularly those where received theories or ideologies have unduly emphasized other motives such as financial gain, commitment to ideals, or the effects on energies and aspirations of arduous physical conditions. Thus sociologists have shown that factory workers are more sensitive to the attitudes of their fellow-workers than to purely economic incentives; that voters are more influenced by the preferences of their relatives and friends than by campaign debates on the “issues;” that soldiers, whatever their ideological commitment to their nation’s cause, fight more bravely when their platoons are intact and they stand side by side with their “buddies.”

²³ When values are “inferred” from this emphasis and then popularized, it becomes the basis of the ideology of “groupism” extolling the virtues of “togetherness” and “belongingness” that have been attacked and satirized so savagely in recent social criticism. David Riesman and W. H. Whyte, the pioneers of this current of criticism in its contemporary guise, are both aware, as their imitators and epigoni usually are not, of the extent to which the social phenomenon they have described is the result of the diffusion and popularization of sociology itself. See on this point Robert Gutman and Dennis H. Wrong, “Riesman’s Typology of Character” (forthcoming in a symposium on Riesman’s work to be edited by Leo Lowenthal and Seymour Martin Lipset), and William H. Whyte, *The Organization Man*, New York: Simon and Schuster, 1956, Chapters 3–5. As a matter of fact, Riesman’s “inner-direction” and “other-direction” correspond rather closely to the notions of “internalization” and “acceptance-seeking” in contemporary sociology as I have described them. Riesman even refers to his concepts initially as characterizations of “modes of conformity,” although he then makes the mistake, as Robert Gutman and I have argued, of calling them character types. But his view that all men are to some degree both inner-directed and other-directed, a qualification that has been somewhat neglected by critics who have understandably concentrated on his empirical and historical use of his typology, suggests the more generalized conception of forces making for conformity found in current theory. See David Riesman, Nathan Glazer, and Reuel Denny, *The Lonely Crowd*, New York: Doubleday Anchor Books, 1953, pp. 17 ff. However, as Gutman and I have observed: “In some respects Riesman’s conception of character is Freudian rather than neo-Freudian: character is defined by superego mechanisms and, like Freud in *Civilization and Its Discontents*, the socialized individual is defined by what is forbidden him rather than by what society stimulates him to do. Thus in spite of Riesman’s generally sanguine attitude towards modern America, implicit in his typology is a view of society as the enemy both of individuality and of basic drive gratification, a view that contrasts with the at least potentially benign role assigned it by neo-Freudian thinkers like Fromm and Horney.” Gutman and Wrong, “Riesman’s Typology of Character,” p. 4 (typescript).

It is certainly not my intention to criticize the findings of such studies. My objection is that their particular selective emphasis is generalized—explicitly or, more often, implicitly—to provide apparent empirical support for an extremely one-sided view of human nature. Although sociologists have criticized past efforts to single out one fundamental motive in human conduct, the desire to achieve a favorable self-image by winning approval from others frequently occupies such a position in their own thinking. The following “theorem” has been, in fact, openly put forward by Hans Zetterberg as “a strong contender for the position as the major Motivational Theorem in sociology.”²⁴

An actor’s actions have a tendency to become dispositions that are related to the occurrence [*sic*] of favored uniform evaluations of the actor and/or his actions in his action system.²⁵

Now Zetterberg is not necessarily maintaining that this theorem is an accurate factual statement of the basic psychological roots of social behavior. He is, characteristically, far too self-conscious about the logic of theorizing and “concept formation” for that. He goes on to remark that “the maximization of favorable attitudes from others would thus be the counterpart in sociological theory to the maximization of profit in economic theory.”²⁶ If by this it is meant that the theorem is to be understood as a heuristic rather than an empirical assumption, that sociology has a selective point of view which is just as abstract and partial as that of economics and the other social sciences, and if his view of theory as a set of logically connected formal propositions is granted provisional acceptance, I am in agreement. (Actually, the view of theory suggested at the beginning of this paper is a quite different one.)

But there is a further point to be made. Ralf Dahrendorf has observed that structural-functional theorists do not “claim that order is based on a general consensus of values, but that it can be conceived of in terms of such consensus and that, if it is conceived of in these terms, certain propositions follow which are subject to the test of specific observations.”²⁷ The same may be said of the assumption that

²⁴ Hans L. Zetterberg, “Compliant Actions,” *Acta Sociologica*, 2 (1957) p. 189.

²⁵ *Ibid.*, p. 188.

²⁶ *Ibid.*, p. 189.

²⁷ Dahrendorf, *Class and Class Conflict in Industrial Society*, p. 158.

people seek to maximize favorable evaluations by others; indeed this assumption has already fathered such additional concepts as “reference group” and “circle of significant others.” Yet the question must be raised as to whether we really wish to, in effect, define sociology by such partial perspectives. The assumption of the maximization of approval from others is the psychological complement to the sociological assumption of a general value consensus. And the former is as selective and one-sided a way of looking at motivation as Dahrendorf and others have argued the latter to be when it determines our way of looking at social structure. The oversocialized view of man of the one is a counterpart to the over-integrated view of society of the other.

Modern sociology, after all, originated as a protest against the partial views of man contained in such doctrines as utilitarianism, classical economics, social Darwinism, and vulgar Marxism. All of the great nineteenth and early twentieth century sociologists²⁸ saw it as one of their major tasks to expose the unreality of such abstractions as economic man, the gain-seeker of the classical economists; political man, the power-seeker of the Machiavellian tradition in political science; self-preserving man, the security-seeker of Hobbes and Darwin; sexual or libidinal man, the pleasure-seeker of doctrinaire Freudianism; and even religious man, the God-seeker of the theologians. It would be ironical if it should turn out that they have merely contributed to the creation of yet another reified abstraction in socialized man, the status-seeker of our contemporary sociologists.

Of course, such an image of man is, like all the others mentioned, valuable for limited purposes so long as it is not taken for the whole truth. What are some of its deficiencies? To begin with, it neglects the other half of the model of human

nature presupposed by current theory: moral man, guided by his built-in superego and beckoning ego-ideal.²⁹ In recent years sociologists have been less interested than they once were in culture and national character as backgrounds to conduct, partly because stress on the concept of “role” as the crucial link between the individual and the social structure has directed their attention to the immediate situation in which social interaction takes place. Man is increasingly seen as a “role-playing” creature, responding eagerly or anxiously to the expectations of other role-players in the multiple group settings in which he finds himself. Such an approach, while valuable in helping us grasp the complexity of a highly differentiated social structure such as our own, is far too often generalized to serve as a kind of *ad hoc* social psychology, easily adaptable to particular sociological purposes.

But it is not enough to concede that men often pursue “internalized values” remaining indifferent to what others think of them, particularly when, as I have previously argued, the idea of internalization has been “hollowed out” to make it more useful as an explanation of conformity. What of desire for material and sensual satisfactions? Can we really dispense with the venerable notion of material “interests” and invariably replace it with the blander, more integrative “social values”? And what of striving for power, not necessarily for its own sake—that may be rare and pathological—but as a means by which men are able to *impose* a normative definition of reality on others? That material interests, sexual drives, and the quest for power have often been over-estimated as human motives is no reason to deny their reality. To do so is to suppress one term of the dialectic between conformity and rebellion, social norms and their violation, man and social order, as completely as the other term is suppressed by those who deny the reality of man’s “normative orientation” or reduce it to the effect of coercion, rational calculation, or mechanical conditioning.

²⁸ Much of the work of Thorstein Veblen, now generally regarded as a sociologist (perhaps the greatest America has yet produced), was, of course, a polemic against the rational, calculating *homo economicus* of classical economics and a documentation of the importance in economic life of the quest for status measured by conformity to arbitrary and shifting conventional standards. Early in his first and most famous book Veblen made an observation on human nature resembling that which looms so large in contemporary sociological thinking: “The usual basis of self-respect,” he wrote, “is the respect accorded by one’s neighbors. Only individuals with an aberrant temperament can in the long run retain their self-esteem in the face of the disesteem of their fellows.” *The Theory of the Leisure Class*, New York: Mentor Books, 1953, p. 38. Whatever the inadequacies of his psychological assumptions, Veblen did not, however, overlook other motivations to which he frequently gave equal or greater weight.

²⁹ Robin M. Williams, Jr. writes: “At the present time, the literature of sociology and social psychology contains many references to ‘Conformity’— conforming to norms, ‘yielding to social pressure,’ or ‘adjusting to the requirements of the reference group.’ . . . ; the implication is easily drawn that the actors in question are *motivated* solely in terms of conformity or non-conformity, rather than in terms of ‘expressing’ or ‘affirming’ internalized values . . .” (his italics). “Continuity and Change in Sociological Study,” *American Sociological Review*, 23 (December, 1958), p. 630.

The view that man is invariably pushed by internalized norms or pulled by the lure of self-validation by others ignores—to speak archaically for a moment—both the highest and the lowest, both beast and angel, in his nature. Durkheim, from whom so much of the modern sociological point of view derives, recognized that the very existence of a social norm implies and even creates the possibility of its violation. This is the meaning of his famous dictum that crime is a “normal phenomenon.” He maintained that “for the originality of the idealist whose dreams transcend his century to find expression, it is necessary that the originality of the criminal, who is below the level of his time, shall also be possible. One does not occur without the other.”³⁰ Yet Durkheim lacked an adequate psychology and formulated his insight in terms of the actor’s cognitive awareness rather than in motivational terms. We do not have Durkheim’s excuse for falling back on what Homans has called a “social mold theory” of human nature.³¹

SOCIAL BUT NOT ENTIRELY SOCIALIZED

I have referred to forces in man that are resistant to socialization. It is not my purpose to explore the nature of these forces or to suggest how we ought best conceive of them as sociologists—that would be a most ambitious undertaking. A few remarks will have to suffice. I think we must start with the recognition that *in the beginning there is the body*. As soon as the body is mentioned the specter of “biological determinism” raises its head and sociologists draw back in fright. And certainly their view of man is sufficiently disembodied and non-materialistic to satisfy Bishop Berkeley, as well as being de-sexualized enough to please Mrs. Grundy.

Am I, then, urging us to return to the older view of a human nature divided between a “social man” and a “natural man” who is either benevolent, Rousseau’s Noble Savage, or sinister and destructive, as Hobbes regarded him? Freud

is usually represented, or misrepresented, as the chief modern proponent of this dualistic conception which assigns to the social order the purely negative role of blocking and re-directing man’s “imperious biological drives.”³² I say “misrepresented” because, although Freud often said things supporting such an interpretation, other and more fundamental strains in his thinking suggest a different conclusion. John Dollard, certainly not a writer who is oblivious to social and cultural “factors,” saw this twenty-five years ago: “It is quite clear,” he wrote, “. . . that he (Freud) does not regard the instincts as having a fixed social goal; rather, indeed, in the case of the sexual instinct he has stressed the vague but powerful and impulsive nature of the drive and has emphasized that its proper social object is not picked out in advance. His seems to be a drive concept which is not at variance with our knowledge from comparative cultural studies, since his theory does not demand that the ‘instinct’ work itself out with mechanical certainty alike in every varying culture.”³³

So much for Freud’s “imperious biological drives!” When Freud defined psychoanalysis as the study of the “vicissitudes of the instincts,” he was confirming, not denying, the “plasticity” of human nature insisted on by social scientists. The drives or “instincts” of psychoanalysis, far from being fixed dispositions to behave in a particular way, are utterly subject to social channeling and transformation and could not even reveal themselves in behavior without social molding any more than our vocal chords can produce articulate speech if we have not learned a language. To psychoanalysis man is indeed a

³² Robert K. Merton, *Social Theory and Social Structure*, Revised and Enlarged Edition, Glencoe, Ill.: The Free Press, 1957, p. 131. Merton’s view is representative of that of most contemporary sociologists. See also Hans Gerth and C. Wright Mills, *Character and Social Structure*, New York: Harcourt, Brace and Company, 1953, pp. 112–113. For a similar view by a “neo-Freudian,” see Erich Fromm, *The Sane Society*, New York: Rinehart and Company, 1955, pp. 74–77.

³³ John Dollard, *Criteria for the Life History*, New Haven: Yale University Press, 1935, p. 120. This valuable book has been neglected, presumably because it appears to be a purely methodological effort to set up standards for judging the adequacy of biographical and autobiographical data. Actually, the standards serve as well to evaluate the adequacy of general theories of personality or human nature and even to prescribe in part what a sound theory ought to include.

³⁰ Emile Durkheim, *The Rules of Sociological Method*, Chicago: University of Chicago Press, 1938, p. 71.

³¹ George C. Homans, *The Human Group*, New York: Harcourt, Brace and Company, 1950, pp. 317–319.

social animal; his social nature is profoundly reflected in his bodily structure.³⁴

But there is a difference between the Freudian view on the one hand and both sociological and neo-Freudian conceptions of man on the other. To Freud man is a *social* animal without being entirely a *socialized* animal. His very social nature is the source of conflicts and antagonisms that create resistance to socialization by the norms of any of the societies which have existed in the course of human history. "Socialization" may mean two quite distinct things; when they are confused an oversocialized view of man is the result. On the one hand socialization means the "transmission of the culture," the particular culture of the society an individual enters at birth; on the other hand the term is used to mean the "process of becoming human," of acquiring uniquely human attributes from interaction with others.³⁵ All men are socialized in the latter sense, but this does not mean that they have been completely molded by the particular norms and values of their culture. All cultures, as Freud contended, do violence to man's socialized bodily drives, but this in no sense means that men could possibly exist without culture or independently of society.³⁶ From such a standpoint, man may

properly be called as Norman Brown has called him, the "neurotic" or the "discontented" animal and repression may be seen as the main characteristic of human nature as we have known it in history.³⁷

But isn't this psychology and haven't sociologists been taught to foreswear psychology, to look with suspicion on what are called "psychological variables" in contradistinction to the institutional and historical forces with which they are properly concerned? There is, indeed, as recent critics have complained, too much "psychologism" in contemporary sociology, largely, I think, because of the bias inherent in our favored research techniques. But I do not see how, at the level of theory, sociologists can fail to make assumptions about human nature.³⁸ If our assumptions are left implicit, we will inevitably presuppose of a view of man that is tailor-made to our special needs; when our sociological theory over-stresses the stability and integration of society we will end up imagining that man is the disembodied, conscience-driven, status-seeking phantom of current theory. We must do better if we really wish to win credit outside of our ranks for special understanding of man, that plausible creature³⁹ whose wagging tongue so often hides the despair and darkness in his heart.

³⁴One of the few attempts by a social scientist to relate systematically man's anatomical structure and biological history to his social nature and his unique cultural creativity is Weston La Barre's *The Human Animal*, Chicago: University of Chicago Press, 1954. See especially Chapters 4-6, but the entire book is relevant. It is one of the few exceptions to Paul Goodman's observation that anthropologists nowadays "commence with a chapter on Physical Anthropology and then forget the whole topic and go on to Culture." See his "Growing up Absurd," *Dissent*, 7 (Spring, 1960), p. 121.

³⁵Paul Goodman has developed a similar distinction. *Op. cit.*, pp. 123-125.

³⁶Whether it might be possible to create a society that does not repress the bodily drives is a separate question. See Herbert Marcuse, *Eros and Civilization*, Boston: The Beacon Press, 1955; and Norman O. Brown, *Life Against Death*, New York: Random House, Modern Library Paperbacks, 1960. Neither Marcuse nor Brown are guilty in their brilliant, provocative, and visionary books of assuming a "natural man" who awaits liberation from

social bonds. They differ from such sociological Utopians as Fromm, *op. cit.*, in their lack of sympathy for the de-sexualized man of the neo-Freudians. For the more traditional Freudian view, see Walter A. Weisskopf, "The 'Socialization' of Psychoanalysis in Contemporary America," in Benjamin Nelson, editor, *Psychoanalysis and the Future*, New York: National Psychological Association For Psychoanalysis, 1957, pp. 51-56; Hans Meyerhoff, "Freud and the Ambiguity of Culture," *Partisan Review*, 24 (Winter, 1957), pp.117-130.

³⁷Brown, *op. cit.*, pp. 3-19.

³⁸"I would assert that very little sociological analysis is ever done without using at least an implicit psychological theory." Alex Inkeles, "Personality and Social Structure," in Robert K. Merton and others, editors, *Sociology Today*, New York: Basic Books, 1959, p. 250.

³⁹Harry Stack Sullivan once remarked that the most outstanding characteristic of human beings was their "plausibility."

THE COMING CRISIS OF WESTERN SOCIOLOGY

ALVIN W. GOULDNER

SENTIMENTS AND THEORY

One of the reasons that domain assumptions have importance as part of the entire sub-theoretical matrix on which theory rests is that they provide foci for feelings, affective states, and sentiments, although they are by no means the only structures around which sentiments come to be organized. To say, for example, that someone “believes” Negroes are lazy and also “believes” this is bad, is not entirely correct. For, those viewing this as “bad” do more than *believe* it; they *feel* it and may indeed feel it strongly. They may have sentiments of disgust and avoidance, or a wish to punish, associated with their assumptions about what the Negro is and with their devaluation of him. Sentiments entail a hormone-eliciting, muscle-tensing, tissue-embedded, fight-or-flight disposition of the total organism. While sentiments often may be organized around or elicited by domain assumptions, they are not the same thing. And they may, of course, be organized around or elicited by a great many things other than domain assumptions, for instance, individual persons or concrete situations.

Furthermore, people may have sentiments that are not conventionally called for by the domain assumptions that they have learned, but they are not for that reason any the less powerful

and body-gripping. There may, in brief, be various forms of dissonance between the existential and normative beliefs that people learn in connection with domain-constituting categories, and the sentiments that they feel toward members of that category. Thus, for instance, a White woman may *feel* sexually aroused and attracted to a Black man, even though she also believes that Blacks are “dirty” and “disgusting.” A man may *feel* pessimistic and despairing, resigned and quiescent, even though he also believes that men are good and that society progresses, simply because he himself is ill or aging. Correspondingly, a man may, when young, feel optimistic and energetically activist, even though he may believe that the world is on a collision course with disaster and that there is little that can be done about it.

I am, of course, not suggesting that young men are invariably more optimistic than old ones, but what I am intimating, using age only as an example, is that people may feel things at variance with their domain assumptions, with their existential beliefs or normative values; feelings emerge from people’s experience with the world, during which they often come to need and learn things that are somewhat different from what they are supposed to need or were deliberately taught to learn. If Freud and other psychologists are right about the Oedipal Complex,

many men in Western societies feel hostility toward their fathers even though they have never been taught to do so, and in fact have been taught to love and honor them. In short, men may have feelings at variance with those of their culturally prescribed “languages” that is, with the domain assumptions conventional to their group of society. Such sentiments may be idiosyncratic to an individual and derive from his unique experience, or they may be shared by large numbers and derive from an experience common to them, even if not culturally prescribed for them. Thus, at least since about the early nineteenth century, many young people in Western countries seem to be subjected to a common experience that induces them to be somewhat more anti-authoritarian, rebellious, or critical of the political and cultural status quo than were their elders.

The prescribed domain assumptions, then, are one thing; the sentiments men have may be quite another. When they diverge, when the things men feel are at variance with their domain assumptions, there is a dissonance or tension between the two levels. Sometimes this is dealt with simply by giving ritualistic “lip service” to the domain assumptions required and taught in the culture; sometimes men may openly rebel against them, adopting or seeking new domain assumptions more consonant with the feelings they actually have. But there is likely to be an intrinsic difficulty in such an open and active rebellion: first, unless there are already alternatives formulated, men may find it easier to live with the old uncomfortable assumptions than with none at all; second, men often experience their own deviant feelings as “wrong” and as perilous to their own security, and consequently may conceal their unprescribed feelings even from themselves; third, as a consequence of this, they may not openly communicate their deviant feelings to others who might share and therefore encourage and support them.

In consequence then, when a gap opens between the sentiments men feel and the domain assumptions they have been taught, their most immediate response may be to suppress or privatize the experienced dissonance. They may allow the tension to fester; or they may begin a kind of sporadic, cultural, guerrilla warfare against the prevailing domain assumptions, in which their dissatisfaction is intermittently expressed in squeaks of black humor or by an inertial apathy. This situation, very much like the attitude of some young radicals today toward academic sociology, begins to change importantly when domain categories and

assumptions emerge that are more consonant with what people feel. When resistance to established assumptions lacks alternatives, it may at first be manifested socially among those who, while lacking a new language, do nonetheless recognize their common possession of deviant sentiments, and therefore may enter into informal solidarities with one another against those who they commonly feel share other sentiments. The current “generation gap” seems a case in point. When, however, the new sentiments begin to find or create their own appropriate language, the possibilities of larger solidarities and of rational public discussion are extended.

It is in part because social theories are shaped by and express domain assumptions that they are also sentiment-relevant: reactions to social theories involve the sentiments of the men who read and write them. Whether a theory is accepted or rejected, whether it undergoes change or remains essentially unchanged, is not simply a cerebral decision; it is in some part contingent upon the gratifications or tensions that it generates by dint of its relation to the sentiments of those involved. Social theories may be sentiment-relevant in various ways and to varying degrees may inhibit or arouse the expression of certain sentiments. As a limiting case, the degree to which they impinge upon sentiments may be so small that, for all practical purposes, they may be said to be “neutral” in their sentiment-relevance. Yet even this last case is consequential for reactions to the theory, for the sentiment-neutral theory may simply be eliciting apathetic or disinterested responses, the feeling that the theory is somehow “irrelevant,” and thus induce avoidance of, if not active opposition to, it. Moreover, reactions to a social theory may also depend upon the *kinds* of sentiments that are aroused directly or by association. The activation of particular sentiments may at some times and for some people be enjoyable, or it may be discomfiting and painful.

Max Weber’s theory of bureaucracy, for example, stressing, as it does, the inevitable proliferation of bureaucratic forms in the increasingly large and complex modern social organizations, tends to elicit and resonate sentiments of pessimism concerning the possibilities of large-scale social change that could successfully remedy human alienation. Those committed to efforts at such, change will experience such sentiments as dissonant and may therefore react critically to the theory, attempting to change it in ways that strip it of such consequences, or they may reject

it altogether. Conversely, those who never had—or who once had but then relinquished—aspirations for social change, or whose inclination is to seek limited intra-system reforms, may for their part not experience the Weberian theory as inducing an unpleasant pessimism.

In one case, then, a theory may have a coherence-inducing or integrating effect, while in another it may have a tension- or conflict-inducing effect; each has different consequences for the individual's ability to pursue certain courses of *action* in the world and has different implications for different lines of political conduct. It is thus through its sentiment-relevance as well as through its domain assumptions that a social theory takes on political meanings and implications quite apart from whether these were knowingly intended or recognized either by those who formulated or those who accepted it. In the example mentioned above, concerning Weber's theory of bureaucracy, it is commonly understood that the theory has strongly antisocialist implications, for it implies that change toward socialism will not prevent bureaucratization and alienation.

PERSONAL REALITY AND SOCIAL THEORY

If every social theory is thus a tacit theory of politics, every theory is also a personal theory, inevitably expressing, coping, and infused with the personal experience of the individuals who author it.

Every social theory has both political and personal relevance, which, according to the technical canons of social theory, it is not supposed to have. Consequently, both the man and his politics are commonly screened out in what is deemed the proper presentation of presumably "autonomous" social theory.

Yet, however disguised, an appreciable part of any sociological enterprise devolves from the sociologist's effort to explore, to objectify, and to universalize some of his own most deeply personal experiences. Much of any man's effort to know the social world around him is prompted by an effort, more or less disguised or deliberate, to know things that are personally important to him; which is to say, he aims at knowing himself and the experiences he has had in his social world (his relationship to it), and at *changing* this relationship in

some manner. Like it or not, and know it or not, in confronting the social world the theorist is also confronting himself. While this has no bearing on the validity of the resultant theory, it does bear on another legitimate interest: the sources, the motives, and the aims of the sociological quest.

Whatever their other differences, all sociologists seek to study something in the social world that they take to be real; and, whatever their philosophy of science, they seek to explain it in terms of something that they *feel* to be real. Like other men, sociologists impute reality to certain things in their social world. This is to say, they believe, sometimes with focal and sometimes only with subsidiary awareness, that certain things are truly attributable to the social world. In important part, their conception of what is "real" derives from the domain assumptions they have learned in their culture. These culturally standardized assumptions are, however, differentiated by personal experience in different parts of the social structure. Individually accented by particular sentiment-generating experiences, the common domain assumptions in time assume personal arrangements; they become part of a man's personal reality.

For simplicity's sake, I suggest that there are two kinds of "reality" with which sociologists must come to terms. One consists of "role realities," the things they learn as sociologists; these include what they believe to be the "facts" yielded by previous researches, whether conducted by themselves or others. The "facts," of course, entail imputations made by men about the world. To assign factuality to some imputation about the world is also to express a personal conviction about its truth, as well as about the propriety of the process by which it was made. To believe an imputation to be "factual" is to assign a high value to it, setting it above such things as "opinions" or "prejudices."

Inevitably, to assign factuality to an imputation is to make it an anchor point in the self's relation to the world, to make it or claim it should be central to the self. To assign factuality to an imputation is to invoke an obligation and duty upon the self: one must "take the facts into account" under certain conditions. There is the further obligation to inspect severely and to examine critically (in short to defend against) attacks on one's "factual" beliefs; a denial of beliefs previously thought to be factual is thus a self-mobilizing "challenge." Within scientific communities, therefore, men engage in committed personal efforts—through contest, conflict,

struggle, and negotiation—to establish and maintain the facts. The facts are not automatically produced by the impersonal machinery of research. To assign factuality to a belief is a self-involving commitment; the person makes a claim upon the credence of another, or himself lends credence to the claim of another. In these and other ways, the factual becomes part of the sociologist's personal reality.

In particular those imputations that a sociologist makes about the factuality of beliefs based on research tend to become aspects of his reality, part of his *focal awareness as a sociologist*. Deemed relevant to his work as a sociologist and derived in accordance with methodological decorum, the sociologist commonly feels that he may with propriety publicly endorse such beliefs. Indeed, these *must* explicitly be attended to by him under certain conditions. In short, he must not ignore them, and he need not conceal his belief in them.

A second order of conceptions about reality held by sociologists consists of the "personally real." These are imputations about "realities" in the social world that sociologists make, not because of "evidence" or "research," but simply because of what they have seen, heard, been told, or read. While these beliefs differ from "facts" systematically gathered and scientifically evaluated, the sociologist nonetheless *experiences* them as no less real—and it is well for his sanity that he does. Still, while these are every bit as real to him as facts garnered through research, if not more so, the sociologist *qua* sociologist is not supposed to credit or attend to them in the same way that he treats "facts"; indeed, he may feel obliged as a sociologist to subject them to systematic doubt. Imputations about the world that are part of the sociologist's *personal* reality may therefore sink into his subsidiary awareness rather than remaining consciously available to him, when he acts as a conforming sociologist. But this, of course, is very far from saying that they thereby cease to have consequences for his work as a sociologist or social theorist. In practice, the sociologist's role realities and his personal realities interpenetrate and mutually influence one another.

During the 1940's and 1950's, largely under the influence of Talcott Parsons, many sociologists stressed the importance of theory in structuring research. Starting from the commonplace that sociologists did not view all parts of the social world as equally important, but rather focused their

attention upon it selectively, they concluded that this perceptual organization was largely the result of the "theories," tacit or explicit, which were held. "Facts" were thus seen as the product of an effort to pursue the inferences of theories and, indeed, as being constituted by the conceptual schemes embedded in the theories. Facts were seen, at least primarily, as interacting with theories, confirming or disproving them, and thus as cumulatively shaping theoretical development; perceptual selectivity, and hence the focus of research, was largely accounted for in terms of the sociologist's theoretical commitment.

This emphasis tended to deprecate the earlier tradition of methodological empiricism, which had stressed the primary value of data and research. If the empiricists had stressed that sociologists are or should be guided by the facts yielded by properly conducted research, theory-stressing sociologists tended to reply that sociologists are or should be guided by articulate, explicit, and hence testable theory. From the standpoint presented here, however, both seem to have been at least partially mistaken.

Those who emphasized theory tended unduly to deprecate the self-implicating, perception-anchoring, and stabilizing role of "facts" (as distinct from their validity-testing function); the empiricists tended to miss the importance of previously held theoretical assumptions. Both, in addition, made a common error in limiting themselves to only one order of the imputably real, namely, the "factual." What both missed is that scientific factuality is only a special case of a larger set of beliefs, those imputing reality; both failed to see that whether an aspect of "role reality" or "personal reality," the imputably real has a special force in structuring the perception of the sociologist and shaping his subsequent theorizing and research. The theorists in particular failed to see the importance of the sub-theoretical level, including the "personally real," as consequential for theory and research. A situation defined as real is real in its consequences, for sociologists as for other men.

Whether part of his role reality or his personal reality, things to which the sociologist imputes reality play a role in his work in several ways. They may be elements that he is concerned to explain, in short, as "dependent variables" or effects; they may be part of his explanatory effort, serving as "independent variables" or possible "causes"; or, again, they may be used as explicit models or tacit paradigms that he

employs to clarify the nature of what he wants to explain or the factors that explain it.

To amplify the latter point: the imputably real enters importantly into theory construction by being regarded as possessed of *generalizable* significance, by being treated as an example or case of, or a model or paradigm of, a larger set of things. Sociologists assume that things they have researched, or with which they have otherwise become personally acquainted and hence “know,” are like (and may be used to understand) other things with which they are unacquainted at first hand or have not yet researched. Thus, while aiming to account for a set of events that extend beyond the sociologist’s facts or personal realities, social theories are at the same time also influenced by his prior imputations about what is real in the world, whether these are his facts or personal realities. For example, Max Weber’s general theory of bureaucracy was influenced both by his historical, scholarly researches and by his first-hand acquaintance with German bureaucracy and, in particular, with governmental rather than private bureaucracy. The German governmental bureaucracy, both as experienced social structure and as cultural ideal, constituted for Weber a personal reality that served as his central paradigm for all bureaucracies; it provided a framework for organizing and assimilating the facts yielded by his scholarly researches.

If personal reality shapes scholarly research, scholarly research is also a source of personal reality, not only of role reality. A man’s research or work is commonly more than just a way he spends time; it is often a vital part of his life and a central part of the experience that shapes his personal reality. If this were not so, then all relevant research would be equally significant to a sociologist. But the truth is that researches and discoveries made by the scholar himself have a special importance for him; a man’s own researches become a part of his personal reality in ways that the work of his colleagues usually does not. If nothing else, they become personal commitments that he wishes to defend.

The limited parts of the social world with which a sociologist’s research bring him into contact are endowed with a compelling reality precisely because they are part of his personal experience. Limited though they are, they often come to be used as paradigms for other, unknown parts, and serve as the basis for generalizing about larger wholes. Thus, for example, *one* reason

Malinowski’s theory of magic differed from that of A. R. Radcliffe-Brown was because the different kinds of magic each had first closely studied came to stand for all other kinds of magic. Although Malinowski had focused on work- and subsistence-getting magic, and Radcliffe-Brown on childbirth magic, each treated his limited experience as a paradigm, exemplary of and essentially akin to other kinds of magic. Evidence incorporated into personal experience became part of a permeating personal reality to which the larger world was assimilated and by which it was shaped.

Sociologists, of course, are familiar with these dangers, at least *en principe*, and they seek to use systematic sampling as a way of obviating them. Nonetheless, systematic sampling cannot fully avoid the problem, for it provides a basis for testing a theory only subsequent to its formulation. Disciplined research entails the use of a systematic sample in order to test inferences from a theory, but, in the nature of the case, the theory must be formulated prior to the sample. Indeed, the more the sociologist stresses the importance of articulate theory, the more this is likely to be the case. The theory will therefore tend to devolve around, and consequently be shaped by, the limited facts and personal realities available to the theorist, *and in particular by those imputed realities that he treats as paradigms.*

Systematic sampling serves primarily as a restraint on unjustified generalization from “facts”; but it does not similarly restrain the influence of “personal realities.” Since the latter commonly remains only at the fringes of subsidiary awareness, being deemed scientifically irrelevant, it is often (and mistakenly) assumed that it is scientifically inconsequential. In point of fact, the personally real and problematic often enough becomes the starting point for systematic inquiry—and, indeed, there is no scientific reason this should not be so.

What is personally real to men is real, frequently though not always, primarily because it is not unique to them—in the sense of idiosyncratic to, or uniquely different for, them—but rather is socially and collectively true. Since the sense of the reality of things often depends on mutual agreement or consensual validation, collectively held notions of reality are among the most firmly constituted components of an individual’s personal reality. Yet the personally real does not entirely

consist of or derive from collective definitions of social reality. It may also emerge from recurrent personal experience, whether unique to the person or shared with a few others. What becomes personally real to one individual, then, need not be personally real to others. But whether derived from collective definitions or from recurrent personal experiences, a man believes that some things are real; and these imputed realities are of special importance to the kinds of theories that he formulates, even if he happens to be a sociologist.

THE INFRASTRUCTURE OF SOCIAL THEORY

From this perspective all social theory is immersed in a sub-theoretical level of domain assumptions and sentiments which both liberate and constrain it. This sub-theoretical level is shaped by and shared with the larger culture and society, at least to some extent, as well as being individually organized, accented, differentiated, and changed by personal experience in the world. I call this sub-theoretical level the “infrastructure” of theory.

This infrastructure is important not because it is the ultimate determinant of the character of social theory, but because it is part of the most immediate, local surround from which the theory-work eventuates in theory-performances and theory-products. Theory-work is surely linked to, even if not solely determined by, the character of the theorist doing it. This infrastructure can never really be left behind, even in the most isolated and lonely moments of theory-work, when a man finally puts pen to paper in a room where there is no one but himself. The world is, of course, there in the room with him, in him; he has not escaped it. But it is not *the* world, not *the* society and *the* culture that is there with him, but *his* limited version and partial experience of it.

However individual a work of theory is, nonetheless, some (and perhaps much) of its individuality is conventional in character. The individuality of theory-work is, in part, a socially sanctioned illusion. For there are the assistants who have helped the theorist do his research and writing; there are the colleagues and the students, the friends and the lovers, on whom he has informally “tested” his ideas; there are those from whom he has learned and taken and those whom he opposes. All theory is not merely influenced but actually

produced by a group. Behind each theory-product is not only the author whose name appears upon the work, but an entire shadow group for whom, we might say, the “author” is the emblem; in a way, the author’s name serves as the name of an intellectual team.

Yet the “author” is not merely the puppet of these group forces, because to some extent he selects his team, recruits members to and eliminates them from his theory-working group, responds selectively to the things they suggest and the criticisms they make, accepting some and ignoring others, attending to some more closely than others. Thus, while authorship is always in some measure conventional, it is also in some measure the expression of the real activities and initiatives of an individual theorist whose “infrastructure” helps shape both the ideas and the shadow group whose tacit collaboration eventuates in theoretical performances.

A concern with sub-theory or the infrastructure of theory is not the expression of an inclination to psychologize theory and is certainly not a form of psychological reductionism. It is, rather, the outcome of a concern for empirical realism, an effort to come close to the human systems to which any theoretical work is most visibly and intimately linked. It is an effort that is peculiarly necessary for those working within a sociological tradition that tends to obscure and to cast doubt upon the importance and reality of persons, and to view them as the creatures of grander social structures. For those, such as myself, who have lived within a sociological tradition, the importance of the larger social structures and historical processes is not in doubt. What is intellectually in question, when the significance of theoretical infrastructure is raised, is the analytic means by which we may move between persons and social structures, between society and the local, more narrowly bounded environments from which social theory discernibly derives. My own view is that any sociological explanation or generalization implies (at least tacitly) certain psychological assumptions; correspondingly, any psychological generalization tacitly implies certain sociological conditions. In directing attention to the importance of the theoretical infrastructure, I have sought not to psychologize social theory and remove it from the larger social system, but rather to specify the analytic means by which I hope to *link it more firmly* with the larger social world.