

BRINGING MEN BACK IN*

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A theory of a phenomenon is an explanation of it, showing how it follows as a conclusion from general propositions in a deductive system. With all its empirical achievements, the functional school never produced a theory that was also an explanation, since from its general propositions about the conditions of social equilibrium no definite conclusions could be drawn. When a serious effort is made, even by functionalists, to construct an explanatory theory, its general propositions turn out to be psychological—propositions about the behavior of men, not the equilibrium of societies.

I AM going to talk about an issue we have worried over many times. I have worried over it myself. But I make no excuses for taking it up again. Although it is an old issue, it is still not a settled one, and I think it is the most general intellectual issue in sociology. If I have only one chance to speak *ex cathedra*, I cannot afford to say something innocuous. On the contrary, now if ever is the time to be nocuous.

In the early 'thirties a distinct school of sociological thought was beginning to form. Its chief, though certainly not its only, intellectual parents were Durkheim and Radcliffe-Brown. I call it a school, though not all its adherents accepted just the same tenets; and many sociologists went ahead and made great progress without giving a thought to it. The school is usually called that of structural-functionalism, or functionalism for short. For a whole generation it has been the dominant, indeed the only distinct, school of sociological thought. I think it has run its course, done its work, and now positively gets in the way of our understanding social phenomena. And I propose to ask, Why?

THE INTERESTS OF FUNCTIONALISM

I begin by reminding you of the chief interests and assumptions of functionalism, especially as contrasted with what it was not interested in and took for granted, for the questions it did not ask have returned to plague it. If what I say seems a caricature, remember that a caricature emphasizes a person's most characteristic features.

First, the school took its start from the study of norms, the statements the members of a group make about how they ought to behave, and indeed often do behave, in various circumstances. It was especially interested in the cluster of norms called a role and in the cluster of roles called an institution. It never tired of asserting that its concern was with institutionalized behavior, and that the unit of social analysis was not the acting individual but the role. The school did not ask why there should be roles at all.

Second, the school was empirically interested in the interrelations of roles, the interrelations of institutions: this was the structural side of its work. It was the sort of thing the social anthropologists had been doing, showing how the institutions of a primitive society fitted together; and the

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sociologists extended the effort to advanced societies. They would point out, for instance, that the nuclear family rather than some form of extended kinship was characteristic of industrialized societies. But they were more interested in establishing *what* the interrelations of institutions were than in *why* they were so. In the beginning the analyses tended to be static, as it is more convincing to speak of a social structure in a society conceived to be stable than in one undergoing rapid change. Recently the school has turned to the study of social change, but in so doing it has had to take up the question it disregarded earlier. If an institution is changing, one can hardly avoid asking why it is changing in one direction rather than another.

Third, the school was, to put it crudely, more interested in the consequences than in the causes of an institution, particularly in the consequences for a social system considered as a whole. These consequences were the *functions* of the institution. Thus the members of the school never tired of pointing out the functions and dysfunctions of a status system, without asking why a status system should exist in the first place, why it was there to have functions. They were especially interested in showing how its institutions helped maintain a society in equilibrium, as a going concern. The model for research was Durkheim's effort to show, in *The Elementary Forms of the Religious Life*, how the religion of a primitive tribe helped hold the tribe together.

Such were the empirical interests of functionalism. As empirically I have been a functionalist myself, I shall be the last to quarrel with them. It is certainly one of the jobs of a sociologist to discover what the norms of a society are. Though a role is not actual behavior, it is for some purposes a useful simplification. Institutions *are* interrelated, and it is certainly one of the jobs of a sociologist to show what the interrelations are. Institutions do have consequences, in the sense that, if one institution may be taken as given, the other kinds of institution that may exist in the society are probably not infinite in number. It is certainly one of the jobs of a sociologist to search out these consequences and even, though this is more difficult, to determine

whether their consequences are good or bad for the society as a whole. At any rate, the empirical interests of functionalism have led to an enormous amount of good work. Think only of the studies made by Murdock¹ and others on the cross-cultural interrelations of institutions.

As it began to crystallize, the functional school developed theoretical interests as well as empirical ones. There was no necessity for the two to go together, and the British social anthropologists remained relatively untheoretical. Not so the American sociologists, particularly Talcott Parsons, who claimed that they were not only theorists but something called general theorists, and strongly emphasized the importance of theory.

Theirs was to be, moreover, a certain kind of theory. They were students of Durkheim and took seriously his famous definition of *social facts*: "Since their essential characteristic consists in the power they possess of exerting, from outside, a pressure on individual consciousnesses, they do not derive from individual consciousnesses, and in consequence sociology is not a corollary of psychology."² Since Durkheim was a great man, one can find statements in his writings that have quite other implications, but this caricature of himself was the one that made the difference. If not in what they said, then surely in what they did, the functionalists took Durkheim seriously. Their fundamental unit, the role, was a social fact in Durkheim's sense. And their theoretical program assumed, as he did, that sociology should be an independent science, in the sense that its propositions should not be derivable from some other social science, such as psychology. This meant, in effect, that the general propositions of sociology were not to be propositions about the behavior of "individual consciousnesses"—or, as I should say, about men—but propositions about the characteristics of societies or other social groups as such.

¹George P. Murdock, *Social Structure*, New York: Macmillan, 1949.

²Émile Durkheim, *Les règles de la méthode sociologique* (8th ed.), Paris: Alcan, 1927, pp. 124-125.

Where functionalism failed was not in its empirical interests but, curiously, in what it most prided itself on, its general theory. Let me be very careful here. In a recent Presidential Address, Kingsley Davis asserted that we are all functionalists now,³ and there is a sense in which he was quite right. But note that he was talking about functional *analysis*. One carries out functional analysis when, starting from the existence of a particular institution, one tries to find out what difference the institution makes to the other aspects of social structure. That is, one carries out the empirical program of functionalism. Since we have all learned to carry out functional analyses, we are in this sense all functionalists now. But functional analysis, as a method, is not the same thing as functional theory. And if we are all functional analysts, we are certainly not all functional theorists. Count me out, for one.

The only inescapable office of theory is to explain. The theory of evolution is an explanation why and how evolution occurs. To look for the consequences of institutions, to show the interrelationships of institutions is not the same thing as explaining why the interrelationships are what they are. The question is a practical and not a philosophical one—not whether it is legitimate to take the role as the fundamental unit, nor whether institutions are really real, but whether the theoretical program of functionalism has in fact led to explanations of social phenomena, including the findings of functional analysis itself. Nor is the question whether functionalism might not do so, but whether it has done so as of today. I think it has not.

THE NATURE OF THEORY

With all their talk about theory, the functionalists never—and I speak advisedly—succeeded in making clear what a theory was. It must be allowed in their excuse that, in the early days, the philosophers of science had not given as clear an

answer to the question as they have now.⁴ But even then, the functionalists could have done better than they did, and certainly the excuse is valid no longer. Today we should stop talking to our students about sociological theory until we have taught them what a theory is.

A theory of a phenomenon consists of a series of propositions, each stating a relationship between properties of nature. But not every kind of sentence qualifies as such a proposition. The propositions do not consist of definitions of the properties: the construction of a conceptual scheme is an indispensable part of theoretical work but is not itself theory. Nor may a proposition simply say that there is some relationship between the properties. Instead, if there is some change in one of the properties, it must at least begin to specify what the change in the other property will be. If one of the properties is absent, the other will also be absent; or if one of the properties increases in value, the other will too. The properties, the variables, may be probabilities.

Accordingly, to take a famous example, Marx's statement that the economic organization of a society determines the nature of its other institutions is an immensely useful guide to research. For it says: "Look for the social consequences of economic change, and if you look, you will surely find them!" But it is not the sort of proposition that can enter a theory. For by itself it says only that, if the economic infrastructure changes, there will be some change in the social superstructure, without beginning to suggest what the latter change will be. Most of the sentences of sociology, alleged to be theoretical, resemble this one of Marx's, yet few of our theorists realize it. And while we are always asking that theory guide research, we forget that many statements like Marx's are good guides to research without being good theory.

To constitute a theory, the propositions must take the form of a deductive system. One of them, usually called the lowest-order proposition, is the proposition to be ex-

³ "The Myth of Functional Analysis as a Special Method in Sociology and Anthropology," *American Sociological Review*, 24 (December, 1959), pp. 757-773.

⁴ See especially R. B. Braithwaite, *Scientific Explanation*, Cambridge: Cambridge University Press, 1953.

plained, for example, the proposition that the more thoroughly a society is industrialized, the more fully its kinship organization tends towards the nuclear family. The other propositions are either general propositions or statements of particular given conditions. The general propositions are so called because they enter into other, perhaps many other, deductive systems besides the one in question. Indeed, what we often call a theory is a cluster of deductive systems, sharing the same general propositions but having different *explicanda*. The crucial requirement is that each system shall be deductive. That is, the lowest-order proposition follows as a logical conclusion from the general propositions under the specified given conditions. The reason why statements like Marx's may not enter theories is that no definite conclusions may in logic be drawn from them. When the lowest-order proposition does follow logically, it is said to be explained. The explanation of a phenomenon is the theory of the phenomenon. A theory is nothing—it is not a theory—unless it is an explanation.

One may define properties and categories, and one still has no theory. One may state that there *are* relations between the properties, and one still has no theory. One may state that a change in one property will produce a definite change in another property, and one still has no theory. Not until one has properties, and propositions stating the relations between them, and the propositions form a deductive system—not until one has all three does one have a theory. Most of our arguments about theory would fall to the ground, if we first asked whether we had a theory to argue about.

FUNCTIONAL THEORIES

As a theoretical effort, functionalism never came near meeting these conditions. Even if the functionalists had seriously tried to meet them, which they did not, I think they would still have failed. The difficulty lay in the characteristic general propositions of functionalism. A proposition is not functional just because it uses the word *function*. To say that a certain institution is functional for individual men in the sense of meeting their needs is not a characteristic proposi-

tion of functionalism. Instead it belongs to the class of psychological propositions. Nor is the statement that one institution is a function of another, in the quasi-mathematical sense of function, characteristic. Though many functional theorists make such statements, non-functionalists like myself may also make them without a qualm. The characteristic general propositions of functional theory in sociology take the form: "If it is to survive, or remain in equilibrium, a social system—any social system—must possess institutions of Type X." For instance, if it is to survive or remain in equilibrium, a society must possess conflict-resolving institutions. By general propositions of this sort the functionalists sought to meet Durkheim's demand for a truly independent sociological theory.

The problem was, and is, to construct deductive systems headed by such propositions. Take first the terms *equilibrium* and *survival*. If the theorist chose *equilibrium*, he was able to provide no criterion of social equilibrium, especially "dynamic" or "moving" equilibrium, definite enough to allow anything specific to be deduced in logic from a proposition employing the term. I shall give an example later. When indeed was a society not in equilibrium? If the theorist chose *survival*, he found this, too, surprisingly hard to define. Did Scotland, for instance, survive as a society? Though it had long been united with England, it still possessed distinctive institutions, legal and religious. If the theorist took *survival* in the strong sense, and said that a society had not survived if all its members had died without issue, he was still in trouble. As far as the records went, the very few societies of this sort had possessed institutions of all the types the functionalists said were necessary for survival. The evidence put in question, to say the least, the empirical truth of the functionalist propositions. Of course the functionalists were at liberty to say: "If a society is to survive, its members must not all be shot dead," which was true as true could be but allowed little to be deduced about the social characteristics of surviving societies.

Indeed the same was true of the other functional propositions. Even if a statement like: "If it is to survive, a society must

possess conflict-resolving institutions," were accepted as testable and true, it possessed little explanatory power. From the proposition the fact could be deduced that, given a certain society did survive, it did possess conflict-resolving institutions of some kind, and the fact was thus explained. What remained unexplained was why the society had conflict-resolving institutions of a particular kind, why, for instance, the jury was an ancient feature of Anglo-Saxon legal institutions. I take it that what sociology has to explain are the actual features of actual societies and not just the generalized features of a generalized society.

I do not think that members of the functional school could have set up, starting with general propositions of their distinctive type, theories that were also deductive systems. More important, they did not. Recognizing, perhaps, that they were blocked in one direction, some of them elaborated what they called theory in another. They used what they asserted were a limited and exhaustive number of functional problems faced by any society to generate a complex set of categories in terms of which social structure could be analyzed. That is, they set up a conceptual scheme. But analysis is not explanation, and a conceptual scheme is not a theory. They did not fail to make statements about the relations between the categories, but most of the statements resembled the one of Marx's I cited earlier: they were not of the type that enter deductive systems. From their lower-order propositions, as from their higher-order ones, no definite conclusions in logic could be drawn. Under these conditions, there was no way of telling whether their choice of functional problems and categories was not wholly arbitrary. What the functionalists actually produced was not a theory but a new language for describing social structure, one among many possible languages; and much of the work they called theoretical consisted in showing how the words in other languages, including that of everyday life, could be translated into theirs. They would say, for instance, that what other people called making a living was called in their language goal-attainment. But what makes a theory is deduction, not translation.

I have said that the question is not whether, in general, functional theories can be real theories, for there are sciences that possess real functional theories. The question is rather whether this particular effort was successful. If a theory is an explanation, the functionalists in sociology were, on the evidence, not successful. Perhaps they could not have been successful; at any rate they were not. The trouble with their theory was not that it was wrong, but that it was not a theory.

AN ALTERNATIVE THEORY

Here endeth the destructive part of the lesson. I shall now try to show that a more successful effort to explain social phenomena entails the construction of theories different from functional ones, in the sense that their general propositions are of a different kind, precisely the kind, indeed, that the functionalists tried to get away from. I shall try to show this for the very phenomena the functionalists took for granted and the very relations they discovered empirically. I shall even try to show that, when functionalists took the job of explanation seriously, which they sometimes did, this other kind of theory would appear unacknowledged in their own work.

The functionalists insisted over and over again that the minimum unit of social analysis was the role, which is a cluster of norms. In a recent article, James Coleman has written: ". . . sociologists have characteristically taken as their starting-point a social system in which norms exist, and individuals are largely governed by these norms. Such a strategy views norms as the governors of social behavior, and thus neatly bypasses the difficult problem that Hobbes posed."⁵ Hobbes' problem is, of course, why there is not a war of all against all.

Why, in short, should there be norms at all? The answer Coleman gives is that, in the kind of case he considers, norms arise through the actions of men rationally calculating to further their own self-interest in a context of other men acting in the

⁵ James S. Coleman, "Collective Decisions," *Sociological Inquiry*, 34 (1964), pp. 166-181.

same way. He writes: "The central postulate about behavior is this: each actor will attempt to extend his power over those actions in which he has most interest." Starting from this postulate, Coleman constructs a deductive system explaining why the actors adopt a particular sort of norm in the given circumstances.

I do not want to argue the vexed question of rationality. I do want to point out what sort of general proposition Coleman starts with. As he recognizes, it is much like the central assumption of economics, though self-interest is not limited to the material interests usually considered by economists. It also resembles a proposition of psychology, though here it might take the form: the more valuable the reward of an activity, the more likely a man is to perform the activity. But it certainly is not a characteristic functional proposition in sociology: it is not a statement about the conditions of equilibrium for a society, but a statement about the behavior of individual men.

Again, if there are norms, why do men conform to them? Let us lay aside the fact that many men do not conform or conform very indifferently, and assume that they all do so. Why do they do so? So far as the functionalists gave any answer to the question, it was that men have "internalized" the values embodied in the norm. But "internalization" is a word and not an explanation. So far as their own theory was concerned, the functionalists took conformity to norms for granted. They made the mistake Malinowski pointed out long ago in a book now too little read by sociologists, the mistake made by early writers on primitive societies, the mistake of assuming that conformity to norms is a matter of ". . . this automatic acquiescence, this instinctive submission of every member of the tribe to its laws. . . ." ⁶ The alternative answer Malinowski gave was that obedience to norms "is usually rewarded according to the measure of its perfection, while noncompliance is visited upon the remiss agent." ⁷ In short, the answer he

gave is much like that of Coleman and the psychologists. Later he added the suggestive remark: "The true problem is not to study how human life submits to rules—it simply does not; the real problem is how the rules become adapted to life." ⁸

The question remains why members of a particular society find certain of the results of their actions rewarding and not others, especially when some of the results seem far from "naturally" rewarding. This is the real problem of the "internalization" of values. The explanation is given not by any distinctively sociological propositions but by the propositions of learning theory in psychology.

The functionalists were much interested in the interrelations of institutions, and it was one of the glories of the school to have pointed out many such interrelations. But the job of a science does not end with pointing out interrelations; it must try to explain why they are what they are. Take the statement that the kinship organization of industrialized societies tends to be that of the nuclear family. I cannot give anything like the full explanation, but I can, and you can too, suggest the beginning of one. Some men organized factories because by so doing they thought they could get greater material rewards than they could get otherwise. Other men entered factories for reasons of the same sort. In so doing they worked away from home and so had to forgo, if only for lack of time, the cultivation of the extended kinship ties that were a source of reward, because a source of help, in many traditional agricultural societies, where work lay closer to home. Accordingly the nuclear family tended to become associated with factory organization; and the explanation for the association is provided by propositions about the behavior of men as such. Not the needs of society explain the relationship, but the needs of men.

Again, functionalists were interested in the consequences of institutions, especially their consequences for a social system as a whole. For instance, they were endlessly concerned with the functions and dysfunctions of status systems. Seldom did they

⁶ Bronislaw Malinowski, *Crime and Custom in Savage Society*, Paterson, N.J.: Littlefield, Adams, 1959, p. 11.

⁷ *Ibid.*, p. 12.

⁸ *Ibid.*, p. 127.

ask why there should be status systems in the first place. Some theorists have taken the emergence of phenomena like status systems as evidence for Durkheim's contention that sociology was not reducible to psychology. What is important is not the fact of emergence but the question how the emergence is to be explained. One of the accomplishments of small-group research is to explain how a status system, of course on a small scale, emerges in the course of interaction between the members of a group.⁹ The explanation is provided by psychological propositions. Certainly no functional propositions are needed. Indeed the theoretical contribution of small-group research has consisted "in showing how the kinds of microscopic variables usually ignored by sociologists can explain the kinds of social situations usually ignored by psychologists."¹⁰

What is the lesson of all this? If the very things functionalists take for granted, like norms, if the very interrelationships they empirically discover can be explained by deductive systems that employ psychological propositions, then it must be that the general explanatory principles even of sociology are not sociological, as the functionalists would have them be, but psychological, propositions about the behavior of men, not about the behavior of societies. On the analogy with other sciences, this argument by itself would not undermine the validity of a functional theory. Thermodynamics, for instance, states propositions about aggregates, which are themselves true and general, even though they can be explained in turn, in statistical mechanics, by propositions about members of the aggregates. The question is whether this kind of situation actually obtains in sociology. So far as functional propositions are concerned, which are propositions about social aggregates, the situation does not obtain, for they have not been shown to be true and general.

⁹ See George C. Homans, *Social Behavior: Its Elementary Forms*, New York: Harcourt, Brace & World, 1961, esp. Ch. 8.

¹⁰ C. N. Alexander, Jr. and R. L. Simpson, "Balance Theory and Distributive Justice," *Sociological Inquiry* 34 (1964), pp. 182-192.

EXPLAINING SOCIAL CHANGE

My next contention is that even confessed functionalists, when they seriously try to explain certain kinds of social phenomena, in fact use non-functional explanations without recognizing that they do so. This is particularly clear in their studies of social change.

Social change provides a searching test for theory, since historical records are a prerequisite for its study. Without history, the social scientist can establish the contemporaneous interrelations of institutions, but may be hard put to it to explain why the interrelations should be what they are. With historical records he may have the information needed to support an explanation. One of the commonest charges against the functionalist school was that it could not deal with social change, that its analysis was static. In recent years some functionalists have undertaken to show that the charge was unjustified. They have chosen for their demonstration the process of differentiation in society, the process, for instance, of the increasing specialization of occupations. In question as usual is not the fact of differentiation—there is no doubt that the over-all trend of social history has been in this direction—but how the process is to be explained.

A particularly good example of this new development in functionalism is Neil Smelser's book, *Social Change in the Industrial Revolution: An Application of Theory to the British Cotton Industry 1770-1840*.¹¹ The book is not just good for my purposes: it is good, very good, in itself. It provides an enormous amount of well organized information, and it goes far to explain the changes that occurred. The amusing thing about it is that the explanation Smelser actually uses, good scientist that he is, to account for the changes is not the functional theory he starts out with, which is as usual a non-theory, but a different kind of theory and a better one.

Smelser begins like any true functionalist. For him a social system is one kind of system of action, characterized as follows: "A social system . . . is composed

¹¹ Chicago: University of Chicago Press, 1959.

of a set of interrelated roles, collectivities, etc. . . . It is important to remember that the roles, collectivities, etc., not individuals, are the units in this last case." Moreover, "all systems of action are governed by the principle of equilibrium. According to the dominant type of equilibrium, the adjustments proceed in a certain direction: if the equilibrium is stable, the units tend to return to their original position; if the equilibrium is partial, only some of the units need to adjust; if the equilibrium is unstable, the tendency is to change, through mutual adjustment, to a new equilibrium or to disintegrate altogether." Finally, "all social systems are subject to four functional exigencies which must be met more or less satisfactorily if the system is to remain in equilibrium."¹² Note that by this argument all social systems are in equilibrium, even systems in process of disintegration. Though the latter are in unstable equilibrium, they are still in equilibrium. Accordingly they are meeting more or less satisfactorily the four functional exigencies. You see how useful a deductive system can be in social science? More seriously you will see that definitions of equilibrium are so broad that you may draw any conclusion you like from them.

But for all the explanatory use Smelser makes of it, this theory and its subsequent elaboration is so much window-dressing. When he really gets down to explaining the innovations in the British cotton textile industry, especially the introduction of spinning and weaving machinery, he forgets his functionalism. The guts of his actual explanation lie in the seven steps through which he says the process proceeds:

Industrial differentiation proceeds, therefore, by the following steps:

(1) Dissatisfaction with the productive achievements of the industry or its relevant sub-sectors and a sense of opportunity in terms of the potential availability of adequate facilities to reach a higher level of productivity.

(2) Appropriate symptoms of disturbance in the form of "unjustified" negative emotional reactions and "unrealistic" aspirations on the part of various elements of the population.¹³

I shall not give the other five steps, as I should make the same criticism of them as I now make of the first two. I think they provide by implication a good explanation of the innovations of the Industrial Revolution in cotton manufacturing. But what kind of an explanation is it? Whatever it is, it is not a functional one. Where here do roles appear as the fundamental units of a social system? Where are the four functional exigencies? Not a word do we hear of them. Instead, what do we hear of? We hear of dissatisfaction, a sense of opportunity, emotional reactions, and aspirations. And what feels these things? Is a role dissatisfied or emotional? No; Smelser himself says it is "various elements of the population" that do so. Under relentless pressure let us finally confess that "various elements of the population" means men. And what men? For the most part men engaged in making and selling cotton cloth. And what were they dissatisfied with? Not with "the productive achievements of the industry." Though some statesmen were certainly concerned about the contribution made by the industry as a whole to the wealth of Great Britain, let us, again under relentless pressure, confess that most of the men in question were concerned with their own profits. Let us get men back in, and let us put some blood in them. Smelser himself makes the crucial statement: "In Lancashire in the early 1760's there was excited speculation about instantaneous fortunes for the man lucky enough to stumble on the right invention."¹⁴ In short, the men in question were activated by self-interest. Yet not all self-interests are selfish interests, and certainly not all the innovations of the Industrial Revolution can be attributed to selfishness.

Smelser's actual explanation of technical innovation in cotton manufacturing might be sketched in the following deductive system. I have left out the most obvious steps.

1. Men are more likely to perform an activity, the more valuable they perceive the reward of that activity to be.
2. Men are more likely to perform an activity, the more successful they per-

¹² *Ibid.*, pp. 10-11.

¹³ *Ibid.*, p. 29.

¹⁴ *Ibid.*, p. 80.

ceive the activity is likely to be in getting that reward.

3. The high demand for cotton textiles and the low productivity of labor led men concerned with cotton manufacturing to perceive the development of labor-saving machinery as rewarding in increased profits.
4. The existing state of technology led them to perceive the effort to develop labor-saving machinery as likely to be successful.
5. Therefore, by both (1) and (2) such men were highly likely to try to develop labor-saving machinery.
6. Since their perceptions of the technology were accurate, their efforts were likely to meet with success, and some of them did meet with success.

From these first steps, others such as the organization of factories and an increasing specialization of jobs followed. But no different kind of explanation is needed for these further developments: propositions like (1) and (2), which I call the *value* and the *success* propositions, would occur in them too. We should need a further proposition to describe the effect of frustration, which certainly attended some of the efforts at innovation, in creating the "negative emotional reactions" of Smelser's step 2.

I must insist again on the kind of explanation this is. It is an explanation using psychological propositions (1 and 2 above), psychological in that they are commonly stated and tested by psychologists and that they refer to the behavior of men and not to the conditions of equilibrium of societies or other social groups as such. They are general in that they appear in many, and I think in all, of the deductive systems that will even begin to explain social behavior. There is no assumption that the men in question are all alike in their concrete behavior. They may well have been conditioned to find different things rewarding, but the way conditioning takes place is itself explained by psychological propositions. There is no assumption that their values are all materialistic, but only that their pursuit of non-material values follows the same laws as their pursuit of material

ones. There is no assumption that they are isolated or unsocial, but only that the laws of human behavior do not change just because another person rather than the physical environment provides the rewards for behavior. Nor is there any assumption that psychological propositions will explain everything social. We shall certainly not be able to explain everything, but our failures will be attributable to lack of factual information or the intellectual machinery for dealing with complexity—though the computers will help us here—and not to the propositions themselves. Nor is there any assumption here of psychological reductionism, though I used to think there was. For reduction implies that there are general sociological propositions that can then be reduced to psychological ones. I now suspect that there are no general sociological propositions, propositions that hold good of all societies or social groups as such, and that the only general propositions of sociology are in fact psychological.

What I do claim is that, no matter what we say our theories are, when we seriously try to explain social phenomena by constructing even the veriest sketches of deductive systems, we find ourselves in fact, and whether we admit it or not, using what I have called psychological explanations. I need hardly add that our actual explanations are our actual theories.

I am being a little unfair to functionalists like Smelser and Parsons if I imply that they did not realize there were people around. The so-called theory of action made a very good start indeed by taking as its paradigm for social behavior two persons, the actions of each of whom sanctioned, that is, rewarded or punished, the actions of the other.¹⁵ But as soon as the start was made, its authors disregarded it. As the theory of action was applied to society, it appeared to have no actors and mighty little action. The reason was that it separated the personality system from the social system and proposed to deal with the latter alone. It was the personality system

¹⁵ Talcott Parsons and Edward Shils (eds.), *Toward a General Theory of Action*, Cambridge, Mass.: Harvard University Press, 1951, pp. 14-16.

that had "needs, drives, skills, etc."¹⁶ It was not part of the social system, but only conducted exchanges with it, by providing it, for instance, with disembodied motivation.¹⁷ This is the kind of box you get into when you think of theory as a set of boxes. For this reason, no one should hold their style of writing against the functionalists. The best of writers must write clumsily when he has set up his intellectual problem in a clumsy way. If the theorist will only envisage his problem from the outset as one of constructing explanatory propositions and not a set of categories, he will come to see that the personal and the social are not to be kept separate. The actions of a man that we take to be evidence of his personality are not different from his actions that, together with the actions of others, make up a social system. They are the same identical actions. The theorist will realize this when he finds that the same set of general propositions, including the success and the value proposition mentioned above, are needed for explaining the phenomena of both personality and society.

CONCLUSION

If sociology is a science, it must take seriously one of the jobs of any science, which is that of providing explanations for the empirical relations it discovers. An explanation is a theory, and it takes the

form of a deductive system. With all its talk about theory, the functionalist school did not take the job of theory seriously enough. It did not ask itself what a theory was, and it never produced a functional theory that was in fact an explanation. I am not sure that it could have done so, starting as it did with propositions about the conditions of social equilibrium, propositions from which no definite conclusions could be drawn in a deductive system. If a serious effort is made to construct theories that will even begin to explain social phenomena, it turns out that their general propositions are not about the equilibrium of societies but about the behavior of men. This is true even of some good functionalists, though they will not admit it. They keep psychological explanations under the table and bring them out furtively like a bottle of whiskey, for use when they really need help. What I ask is that we bring what we say about theory into line with what we actually do, and so put an end to our intellectual hypocrisy. It would unite us with the other social sciences, whose actual theories are much like our actual ones, and so strengthen us all. Let us do so also for the sake of our students. I sometimes think that they begin with more understanding of the real nature of social phenomena than we leave them with, and that our double-talk kills their mother-wit. Finally, I must acknowledge freely that everything I have said seems to me obvious. But why cannot we take the obvious seriously?

¹⁶ Smelser, *op. cit.*, p. 10.

¹⁷ *Ibid.*, p. 33.