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Social Theory, Social Research, and a Theory of Action¹

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After an extraordinarily promising beginning in 1937 with *The Structure of Social Action*, Talcott Parsons abandoned his attempt to ground social theory in a theory of purposive action. The functionalism that resulted moved in one direction, while social research has progressively moved in an individual-behavioristic direction, resulting in an ever-widening divergence between research and theory. This paper describes paths in research and in theory development that will reconstitute relevance of each for the other. The essential elements are two. The first is use of a theory of purposive action as a foundation for social theory; this entails acceptance of a form of methodological individualism and rejection of holism. The second is a focus in social research and theory on the movement from the level of individual actions to macrosocial functioning, that is, the level of system behavior.

THE PROMISE AND LOSS OF A THEORY OF ACTION

In 1937, in *The Structure of Social Action*, Talcott Parsons sketched an initial attempt to construct what he described as a voluntaristic theory of action, extending the model of rationality used by economists and systematizing the historians' conception of purposive action. Parsons thus introduced into American sociology the theory of action underlying much of the work in European social thought. In doing so, he was making a natural extension of the orientation shared by three of the four theorists whose work he examined: Max Weber, Alfred Marshall, and Vilfredo Pareto. This orientation, a form of methodological individualism, is one that grounds social theory in a theory of individual action.

The same orientation was shared by social and political philosophers of the 17th, 18th, and 19th centuries such as Hobbes, Smith, Locke, Rous-

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seau, and Mill.² A single theory of action, differing only in details, was shared by all these theorists: individuals were seen as purposeful and goal directed, guided by interests (or “values,” depending on the theorist) and by the rewards and constraints imposed by the social environment.

Why was a theory of action fundamental to the work of these and other theorists, when in fact each was concerned with macrosocial phenomena, with the functioning of political and economic systems, with large-scale social change? It was fundamental because it allowed connecting intentions of persons with macrosocial consequences. Thus the functioning of society as well as the engine of social change could be grounded in the purposive actions of individuals, taken in particular institutional and structural settings that shaped the incentives and thus the action. Social theory with this kind of grounding made possible a connection between the individual and society, and it even made possible a conception of how social systems might be shaped by human will.³ Perhaps most important, it made possible a link between positive social theory and normative social philosophy, by connecting individual interests with their realization or lack of realization.⁴

But Parsons’s 1937 program of theory construction did not work out. The extraordinarily ambitious and integrative program that he outlined was not pursued systematically in his further work. In his subsequent theoretical treatises, *Toward a General Theory of Action* (1951), *The Social System* (1951), *Working Papers in the Theory of Action* (1953), and part II of the introduction to *Theories of Society* (1961), Parsons progressively abandoned a theory of action (despite the titles of two of these works) and chose instead to characterize the equilibrium states and “phases” of social systems. Possibly because he was unable to derive, in a theoretical fashion, systemic action from the combination of individual actions, he made a conceptual leap to the systemic level and subsequently

² Although Marx, from a continental philosophical tradition, did not fully share this orientation, he did so in part. For a discussion of the issue of methodological individualism in Marx’s work, see Elster (1985, pp. 5–18).

³ Some social theorists accept or reject this approach because of an optimistic belief that individuals *can* shape the functioning of social systems or a pessimistic view that they cannot but are merely products of their environments. But the theoretical stance is logically independent of the answer to this question. A theoretical position of methodological individualism is fully compatible with recognition of the constraints on action that social structure creates.

⁴ A good example of the way positive theory and normative social philosophy can interact is provided by the Spring 1985 issue of *Social Philosophy and Policy*, devoted to “ethics and economics.” In that issue, philosophers and economists examine—with a common conceptual framework—the moral standing of the market and other economic institutions. Such an interaction based on a conceptual framework from contemporary sociological theory is difficult to visualize—largely, I suggest, because the conceptual frameworks of sociology are not grounded in a theory of action.

concerned himself with ways of classifying social equilibria. By so doing (and in the absence of serious contemporary contenders for social theory at that level of generality), he broke the links with earlier social theorists, with political philosophy, with political economy, with legal theory, and he ushered in a period of simple functionalism in sociological theory. It was a kind of social theory that could account for any institution and any social configuration by showing its functions, but it had no place for individuals, except as deviants from norms, and no place for social change except by theoretical fiat, as in the AGIL scheme. It provided no possibility for the normative evaluation of social institutions or social systems, for it never descended to the level of individuals, whose satisfaction (or dissatisfaction) provides our soundest basis for evaluating social configurations.

Modifications to functional analysis toward “structural-functional” analysis were made by other theorists, in particular Robert Merton in his *Social Theory and Social Structure* (1949). In showing that a social form may have positive functions (“eufunctions”) for some actors and dysfunctions for others, Merton refocused attention on *actors*, and in showing that the form’s continuation was contingent on actions of those actors for whom it had positive functions, he reintroduced purposive action. But these modifications removed the theoretical uniqueness of functional analysis—its homeostatic principle, explaining a social configuration not by proximate causes but by its consequences—leaving a theoretical approach that in its logical properties was not different from others. The effect on the discipline was not to reintroduce the theory of action that Parsons had discarded but to move away from functional explanation via final causes toward explanation by (proximate) causes, that is, toward causal analyses.

There were also direct challenges to Parsons’s functionalism. The strongest was that by George Homans, best exemplified by the title of one paper, “Bringing Men Back In.” Homans (1958) did introduce actors and a theory of action, perhaps a more explicit purposive action theory than had been set forth in sociology before. But this never moved beyond the social-psychological or small-groups level, and its effect was soon dissipated by his move from purposive action to a reductionism that was little different from the operant conditioning that B. F. Skinner demonstrated with pigeons. As did Parsons, Homans saw the essential problem for sociological theory as the refining of the theory of action. Parsons, failing to find a solution, moved to the macrosocial level and discarded the microfoundation. Homans moved in the other direction, away from goal-directed action, to reductionist behaviorism. Merton reshaped Parsons’s functional theory away from final causes but did not bring back in an explicit microfoundation.

Subsequent challenges to functionalism (the principal one being “conflict theory”) have acquiesced in remaining at the collective or systemic level, thus failing to provide a theory grounded in purposive action of individuals. The program outlined by Parsons in 1937, despite the promise it held, remains unfulfilled.

It is useful to explicate my premises here. Implicit in the rejection of functionalism as a theory of social organization and the acceptance of a theory of purposive action as a grounding for social theory is a simultaneous rejection and acceptance of purpose. Purpose is rejected at the level of the system, but not at the level of its component actors. A theory of action as a basis for social theory is indeed a functional theory at the level of the actor: the actor is regarded as acting purposively. Actions are “caused” by their (anticipated) consequences.

Purposive action of individuals can be taken as a starting point by sociologists, who can assume well-organized individuals, though not by psychologists, for whom the individual’s psychological organization is centrally problematic. But just as psychologists would lose their problem if they assumed individuals to be internally well organized, sociologists lose their problem when they assume purposes and goal-directed action of societies as units. It may well be that, for some investigations, corporate bodies such as formal organizations are usefully regarded as purposive actors, though in other research and theory in sociology, the coherence of their action would itself be taken as problematic.

The appropriate theoretical strategy for sociology, if I am correct, is not to discard notions of purpose, goal-directedness, and homeostasis (as is true in causal analyses that remain at the social system level), but to limit their employment to the level of *actors* in the social system—not positing them for the system itself. The action, or behavior, of the system composed of actors is an emergent consequence of the interdependent actions of the actors who make up the system.

The rule, in its most simple form, is as follows. Purpose and goal-directedness are useful in theory construction, but not if they characterize the entity or system whose behavior is to be explained. They must instead characterize elements of the system, which in the case of sociology can be regarded as actors in the system, either persons or corporate actors. The central theoretical problems then come to be two: how the purposive actions of the actors combine to bring about system-level behavior, and how those purposive actions are in turn shaped by constraints that result from the behavior of the system. The two problems when taken together provide the elusive result that functional analysis seeks: to characterize the ongoing and sometimes self-equilibrating functioning of a social system.

An especially unfortunate consequence of the loss of a theory of action

was loss of contact with that one discipline that arguably should have the strongest intellectual links to social theory: common or constitutional law. One might even argue that law, as a set of rules having a high degree of internal consistency, as well as principles behind those rules, has as strong a claim to constitute social theory as does any alternative body of principles offered up by sociologists. All case law is based inherently on a theory of action. For example, modern Western law, both continental law and English common law, is based on the conception of purposive individuals with rights and interests, who are responsible for their actions.⁵ In central Europe in the Middle Ages, this was not the underlying theory of action: guilds, households, and other social units were the responsible, purposive, interested actors with rights; the law had little to do with the individual person per se. Similarly in the case of the informal law governing relations between nomadic tribes or clans: the common prescription, “an eye for an eye and a tooth for a tooth,” refers not to individual retribution but to clan retribution visited on any member of the offending clan.

Because the theory of action underlying modern economic theory and that underlying Western legal theory have much in common, they have a meeting ground (especially in the area of rights, but also in such branches of law as agency and contracts). Such economic theorists as Joseph Schumpeter or Friedrich Hayek can move easily between economic theory and legal philosophy, with each infusing the other. Richard Posner’s book *The Economic Analysis of Law* (1977) is also able to have a strong impact on legal theory. The failure to provide a theory of action as a common basis for discourse prevents social theory from having a similarly fruitful interaction with legal theory.⁶

THE WATERSHED IN EMPIRICAL RESEARCH AND THE GROWTH OF INDIVIDUALISTIC BEHAVIORISM

Concurrently with the emerging dominance in sociology of functional theory at the level of the collectivity came a movement of empirical research that led in precisely the opposite direction. The 1940s constituted

⁵ One implication of this is that the law has special difficulties with corporate action, especially in cases of criminal law where the corporation is held to “have committed a crime.” Where does responsibility or liability lie? Only with the corporation per se, or with some members? If the latter, which ones? Why? For an examination of these issues see Hopt and Teubner (1985); Coleman (1985); Stone (1975). It is clear that, if organization theory in sociology were grounded in a theory of action (i.e., were a theory in which rights, interests, and responsibilities played an important part), it could make strong contributions to the evolution of legal precedent in this area.

⁶ There are beginnings of such interaction. Scheppele (in press) does just this for the law’s treatment of information.

a kind of watershed in empirical research in sociology. Before the watershed, community studies of the *Middletown* variety constituted the dominant empirical mode; after the watershed, survey research was dominant. The watershed was brought into existence in part by new empirical methods; some of the early postwatershed studies were Stouffer's (with others) *The American Soldier* (1949) and Lazarsfeld's (with Berelson and Gaudet) *The People's Choice* (1944) and (with Stanton) *Radio Research, 1941* (1941). The first cases following the watershed contained elements of the prewatershed focus. For example, the samples for Lazarsfeld's early survey research at Columbia were nearly always localized in communities, and some attempts were made (not always successfully) to introduce community structure into the analysis. In the American Soldier studies, social structure entered into the survey design through the organizational structure of the military service. In a few cases, this research lent itself to theoretical developments, as in the work on reference groups by Merton and Kitt (1950) based on the American Soldier analysis, but this was not common.

Although the empirical, statistical survey research was highly individualistic, it lacked one element that could bring about a connection with social theory grounded in a theory of action. The element that was absent was an explicit purposive or intentional orientation. The descriptive community studies that in American sociology preceded this watershed had necessarily incorporated a purposive orientation. This was a natural part of describing how the social conditions affected various persons' orientations to action and how these orientations to action, given the existing structure of relations, combined to produce the system of action that resulted in community action.

But the statistical association basis for inference in survey analysis seemed to have little natural affinity for the intentions or purposes of individuals. Lazarsfeld, one of its pioneers, was very interested in a theory of action.⁷ However, it was his paper on the logic of causal inference in survey research (Kendall and Lazarsfeld 1950) that influenced further work in the discipline, not his paper titled "The Art of Asking Why" (1935) or his work on reason analysis. Succeeding work based on survey data has increasingly led toward "causal explanation of behavior," with the causes either social characteristics of the individual or characteristics of the individual's environment, and without recourse to an intervening action orientation on the part of the actor. Purpose or intention in

⁷ See, e.g., his "Historical Notes on the Empirical Study of Action" (1972) and his paper with Oberschall on Max Weber's empirical work (1965). Much of Lazarsfeld's work on decision making in voting and consumer behavior expressed this action orientation.

this work has not vanished altogether, but has been relegated to post hoc accounts that can provide an intuitively appealing set of reasons why the causal structure takes the form it does.

In such an analysis, success of the explanation is ordinarily measured by the amount of variation in behavior “accounted for” by these characteristics. Thus, for example, in causal modeling of individual status attainment or of school achievement, those variables that appear in the causal model are social or environmental factors that taken together account for variations in attainment or achievement, and a successful analysis is one in which a large fraction of the variance in attainment or achievement is explained.

A second important element in the replacement of community studies by survey research—almost unnoticed, it seems, by the discipline—was a shift in the unit of analysis (the unit about which empirical statements were made) from the community to the individual. In much of the work following this change, the focus shifted from social processes within the community shaping the system’s behavior to psychological or demographic processes shaping individual behavior. Indeed, as survey research secured its dominance, its practitioners moved more forthrightly toward a focus on individual behavior. Dense community or organizational samples were replaced by national samples, snowball sampling died in its infancy,⁸ and the struggling effort to use survey research to make statements about communities, organizations, or social subsystems was overwhelmed by the greater statistical rigor of characterizing “populations” and analyzing behavior of individuals as “independently drawn” members of the population.

Thus one could say that as social theory was moving to a functionalism that remained at the collectivity level, the main body of empirical re-

⁸ I gave a seminar in 1956 at the University of Chicago on these methods. Leo Goodman heard it, became interested, and wrote a paper (1961) on statistical inference in snowball samples, but the statistical development died there. More generally, there was a variety of early attempts at modifying the new statistical tools for the analysis of functioning social systems. Some of these on the part of sociologists at Columbia at the time took two forms. One, which is hard to generalize from specific cases, is the use of survey data to characterize social subsystems and is exemplified by *The Adolescent Society* and *Union Democracy* (see esp. app. 1). A second comprised attempts to develop more formal techniques of analysis. It is exemplified by a paper of mine appropriately titled “Relational Analysis: The Study of Social Organization with Survey Methods” (1958). It is an interesting footnote in the sociology of knowledge that none of the social and intellectual forces impinging on the discipline was conducive to the development of these analytical tools. My own efforts in this direction were diverted in 1965 by the demands of government for policy research, which resulted in *Equality of Educational Opportunity*.

search was abandoning analysis of the functioning of collectivities to concentrate on analysis of the behavior of individuals.

On two grounds, then, the empirical research that became the dominant mode in sociology came to be of limited usefulness for social theory. First, it was lacking a theory of action, replacing "action" with "behavior" and eliminating any recourse to purpose or intention in its causal explanations; second, it focused on explaining the behavior of individuals *per se*, seldom moving up to the level of a community or other social systems.

One may ask just why there came to be such a radical shift toward a focus on individual behavior in a discipline whose subject matter, after all, is the social system. Part of the answer lies in the invention of techniques. The statistical tools of survey design and analysis began in the 1940s to make possible quantitatively precise statements about samples of independent individuals and the populations (again of independent individuals) they represent, as well as analysis of factors affecting individual behavior. There was no comparable development of tools for analysis of the behavior of interacting systems of individuals or for capturing the interdependencies of individual actions as they combine to produce a system-level outcome. The far greater complexity required of tools for these purposes constituted a serious impediment to their development and continues to do so (though some methods such as those generally labeled "network analysis" move in that direction). The end result is extraordinarily elaborated methods for analysis of the behavior of a set of independent entities (most often individuals), with little development of methods for characterizing systemic action resulting from the interdependent actions of members of the system.

This technical development of survey methods for studying individual behavior (and the subsequent development of computers for data processing) helped bring social research and the tradition of demographic research closer together. The influence of the demographic tradition led research even further in the direction of studying individuals, as can be seen most strikingly in research on social stratification.⁹

However, the technical developments and developments in the discipline provide only a part of the explanation of the shift to a focus on individual behavior. Another part derives from a change in the structure of society itself. That change is one that has brought about a change in the very relation of social research to society.

⁹ Demography is an area in which the micro-to-macro movement can in many cases be carried out purely by aggregation of individual behavior. Only in a few areas, such as the two-sex problem (which has never been solved), do demographers need to have a device more complicated than simple aggregation to move from micro to macro levels.

CHANGES IN SOCIETY AND CHANGES IN THE RELATION OF
SOCIAL RESEARCH TO ACTION¹⁰

In the middle of this century, while Parsons was turning away from his attempt to build social theory on the basis of a theory of action and empirical research was discovering the techniques of survey research and statistical analysis, there were changes in American society with important implications for social research. These changes (which followed somewhat later in Europe) were ones that shifted the nation from a set of local communities, largely internally focused, to a place in which the focus was no longer local, but national. Manufacture changed in many product areas; instead of local firms selling to local markets, national firms sold to national markets. Concurrently there was an emergence of national media of communication. The national magazines were an important medium, gaining their growth in the 1930s. Radio was a second medium of importance. Through their advertising, these media helped create national markets that facilitated national manufacture. Also, they themselves had national markets, focusing the attention of the population as a whole on common objects.

One consequence of this change was the emergence of a new set of sociological problems. These were problems related to the national markets and national audiences—in short, problems of market research and audience research.

The research problems generated by these social changes differed in an important way from the research problems before this watershed. They were problems of particular actors in society, and the results were of direct interest to those actors, who were prepared to act on them. The earlier research, initiated by disinterested investigators or by philanthropic sponsors, was designed sometimes purely as a “contribution to knowledge,” with no action implications but more often with an implicit theory of the relation of research to action, that of the exposé. This implicit theory was based on the premise that exposure of a particular social ill or social problem would set in motion the forces for its elimination. *Middletown*, or the Yankee City studies of Lloyd Warner, or Zorbaugh’s *Gold Coast and the Slum* all had this implicit premise, a premise that remained the impetus behind such works as Lynd’s *Knowledge for What?* (1939), written as a critique of the postwatershed applied social research.

The new applied research was initiated by corporate actors holding a different premise: that research focusing directly on problems of interest to them would provide information relevant to their actions. This change initiated a new component of macrosocial organization supplied by social

¹⁰ I have discussed these changes in greater detail elsewhere (Coleman 1978, 1980b).

research itself—systematic means of information feedback to large corporate actors in society—and a new relation of social research to action.

The principal locus of the early postwatershed research focused on individual behavior was in these areas of market research and audience research. This was most evident in Lazarsfeld's work, but it can also be seen in the growth of programs in mass communications research in a number of universities in the 1940s and 1950s.

Yet this was only the first stage of the transformation. The change in American society in the structure of interaction from personal and local to impersonal and national induced another change by the 1960s: a change in the structure of responsibility from private and local to public and national. The changed structure of communication increasingly generated claims on the national government and an assumption by the national government of responsibilities that would never have arisen before the changed structure of interaction. Certain of these responsibilities, such as Social Security and emergency work programs, arose in the 1930s, early in the shift from local to national interaction. A later spurt came with the "Great Society" legislation of Lyndon Johnson, beginning in 1964: the Civil Rights Act of 1964, the Elementary and Secondary Education Act of 1965, the Office of Economic Opportunity, the Headstart program, Medicare, and a number of other innovations. With these policies came a new kind of social research: social policy research. This has come to take a number of forms, some with names that were unknown in 1960: large-scale social experimentation, process evaluation, summative evaluation, planned variations, intervention research, and national longitudinal studies.

The end result of these changes is that much if not most applied social research (not only in American society, for these changes have occurred also in Europe) has come to be research directed to problems of policy, that is, designed to inform the actions of large corporate actors, most often government but also business corporations, trade unions, and various voluntary associations.

This changed relation of social research to social action raises two issues, one an issue for social theory and the other a normative issue. The issue for social theory involves incorporating information into a theory of action involving corporate actors at the societal level and persons who are their clients. Purposive action requires information, and in a social structure in which information is valuable (i.e., a scarce commodity), its possession can affect the distribution of power. Social policy research will ordinarily be initiated by the largest corporate actors and be designed to provide information that will allow them better to pursue their interests. In the asymmetric structure of society that has emerged in this century, natural social processes will result (given the problems of paying the cost

of a public good) in an asymmetry of information, leading to increased asymmetries of power between corporate actors and persons.¹¹

This points directly to the normative issue, for it raises questions about the distribution of information rights in society and the way this distribution affects the interests of persons. These questions have been addressed already in legislation (e.g., in the United States, the Freedom of Information Act of 1974), but they have not been incorporated into a normative theory of the initiation of policy research, its design, and its dissemination. If a society's political system is based on a principle of democracy, this principle provides the value premise for a normative theory of the distribution of information rights in policy research.

When such a theory is more fully developed, it can provide the basis not only for legal theory about information rights but also for the conduct of social policy research. Thus sociology finds itself in a reflexive position: social theory could guide the role in society of social policy research. There have been a few contributions that could aid such theory, perhaps the most notable a new book by Duncan MacRae, Jr., titled *Policy Indicators: Links between Social Science and Public Debate* (1985). Yet if I am correct, social theory will be in no position to accept this positive-cum-normative challenge until it rediscovers a theory of action that it has abandoned.

What I have been describing in this and the preceding section is a complex array of changes in the structure of society, in social research, and in the relations between them, changes that can be captured by an appropriate orientation to social theory. The changes that I have described are as follows:

1. Society has become more individualistic, with individuals pursuing paths disconnected from family and community.

2. The mainstream of social research has shifted from explaining the functioning of social systems (e.g., communities) to accounting for individual behavior. Properties of social systems have largely been relegated to the status of factors affecting individual behavior and are seldom the focus of investigation. This shift toward explaining individual behavior was in part of direct consequence of the change in social structure, in part an indirect consequence, through a new research technology that it encouraged, survey research.

3. Simultaneously with this shift in focus from the social system to the individual, the dominant mode of explanation in social research shifted away from one in which purposive action of individuals, taken in combination and subject to various constraints, explained the functioning of social systems. This was replaced by a form of behaviorism, in which

¹¹ For further discussion, see Coleman (1982); Habermas (1971).

various factors external to the individual's consciousness are introduced to account for variations in individual behavior. This change followed naturally from the shift in focus to individual behavior because purposive explanation becomes trivial at the level of individual action ("He carried out action x in order to achieve goal y ") unless psychological complexity is introduced, thereby changing the problem from one in sociology to one in psychology.¹²

4. The shift toward individualism was accompanied by a growing structural asymmetry in Western society, with large corporate actors (corporations, government) on one side and individuals (not communities, not neighborhoods, not families) on the other, linked together by mass media rather than direct communication.

5. In this social structure, a new kind of social research has arisen, as part of the articulation between corporate actors and persons, first in the form of market research and then in the form of social policy research. With this move, social research has come for the first time directly into the functioning of society—no longer standing outside it but instead modifying the articulation between corporate actors and persons—primarily as the agent of corporate actors. As such, it becomes not only part of sociology but also properly an object of social theory, as part of the larger task of social theory to characterize this articulation between actors of different types and very different size and power.

But this task can be accomplished only by a social theory that has two properties. First, it explicitly recognizes that social action requires not only a verb, "to act," but also a noun as subject, the actor. Second, it is able to make satisfactorily the transition from the micro level to the macro level, from the purposive action of individual actors to the functioning of a system of action. It is to a discussion of this second task that I now turn.

THE MICRO-TO-MACRO PROBLEM

The program that the Parsons of 1937 had was presumably based on a diagnosis of what was lacking in order to move beyond the theorists whose work he described. He saw the theory of action itself as the point at which major modification was necessary and developed an elaborate description of the necessary modifications (1937, pp. 77–82). Later he attempted to use psychoanalytic theory to develop a theory of personality that would constitute his "theory of action." But what neither Parsons nor

¹² By "psychological complexity" here I mean to include work that ranges from the kind of complexity that Freud introduced to the kind that Kahneman and Tversky (1979) introduce, so long as it continues to view the individual as purposive or goal seeking.

others engaged in similar attempts seem to have realized is that the major theoretical obstacle to social theory built on a theory of action is not the proper refinement of the action theory itself, but the means by which purposive actions of individuals *combine* to produce a social outcome. Insofar as Parsons did attempt to move explicitly from the individual level to the social level, it was through a personality-culture leap, disregarding the very structural configurations that are the essential element in determining the social outcome of a combination of individual actions (see, e.g., Parsons, Bales, and Shils 1953, chaps. 1, 2).

This micro-to-macro problem is sometimes called by European sociologists the problem of transformation. In economics, it is (misleadingly) termed the problem of aggregation; in political science, a major instance of it is the problem of social choice. It is the process through which individual preferences become collective choices; the process through which dissatisfaction becomes revolution; through which simultaneous fear in members of a crowd turns into a mass panic; through which preferences, holdings of private goods, and the possibility of exchange create market prices and a redistribution of goods; through which individuals' task performance in an organization creates a social product; through which the reduction of usefulness of children to parents leads families to disintegrate; through which interest cleavages lead (or fail to lead) to overt social conflict.

One way to see the role of the micro-to-macro problem in social theory is to examine different types of relations in sociology. The characteristic problem in sociology is that of accounting for some aspect of the functioning of a social system. Put in causal diagram form, it can be seen as the effect of one macro-level variable on another, such as the effect of religious doctrine on the economic system (e.g., Max Weber's general thesis in *The Protestant Ethic and the Spirit of Capitalism*). It can be diagrammed as shown in figure 1.

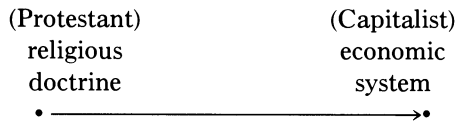


FIG. 1.—Macro-level relation: methodological holism

Some social theory and some social research are based entirely on relations of this sort. They exhibit a methodological holism that contrasts to the methodological individualism that grounds sociology in a theory of action.

One of the most serious defects of a program of theory building and research based on such macro-level relations is data inadequacy: At a

macrosocial level, there is ordinarily too little variation, either in a single social system over time or among different social systems, to test the relation empirically.¹³ Another defect is that, unless the theory is functionalist, and the system itself is treated as homeostatic (a solution that eliminates the possibility of immanent change), there is no explanation or understanding of why one relation holds rather than another. A third defect is that such an approach must assume the existence of a social system as a starting point. It can never address questions like the Hobbesian problem of order.

A second theoretical approach to the central problems of sociology is not to remain at the macrosocial level but to move down to the level of individual actions and back up again. This approach, methodological individualism, can be diagrammed as shown in figure 2.

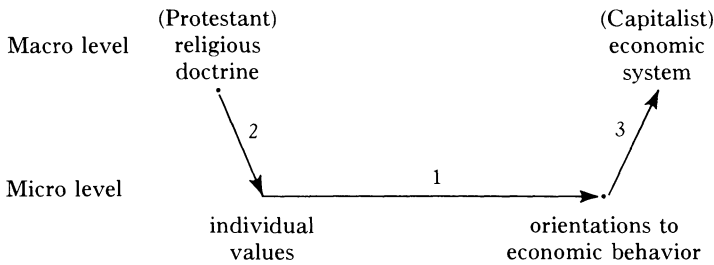


FIG. 2.—Macro-micro-macro relations: methodological individualism

In the context of this diagram, it is possible to see the Parsonian program and the source of its failure. Parsons recognized that the theorists whose work he examined were concerned with relations at the level of the social system (as in fig. 1), that they moved down to the individual level to study these relations (as in fig. 2),¹⁴ and that at the individual level (type-1 relation in fig. 2) they shared roughly the same theory of action. Parsons proposed to develop a general social theory by refining that theory of action on which the relations of type 1 are based.

But it is the type-3 relation that has proved the main intellectual hurdle both for empirical research and for theory that treats macro-level rela-

¹³ This defect is exhibited even in the domain of economic activity, where fluctuations occur much more rapidly than do changes in other aspects of social functioning. The business cycle analysis of the 1930s and 1940s, which attempted to correlate changes in macro-level variables and thus predict changes in some macro-level variables on the basis of changes in others, proved unfruitful. Although there are continued attempts in economics to carry out such analysis, its usefulness has not been demonstrated.

¹⁴ His inclusion of Durkheim seems incorrect here, since Durkheim's work takes the form of fig. 1, involving only macro-level relations, or macro-to-micro relations like that of the relation labeled 2 in fig. 2.

tions via methodological individualism. For example, in Max Weber's analysis of Protestantism and capitalism, he shows through illustration the effect of Protestant doctrine on individual values (type-2 relation) and, again through illustration, the effect of these values on individual orientations to economic behavior. What he fails to show is how these individual orientations combined to produce the structure of economic organization that we call capitalism (if in fact they did in combination produce this effect).¹⁵ For Marx similarly, the heart of his theory is contained in a type-2 relation, where the macro-level variable is the means of production and the micro-level variable is individual consciousness of economic and social interests (a relation that is expressed in his statement, "it is his social existence that determines his consciousness"). He is at his weakest in showing how the common interests thus generated are transformed into class-conscious social action, that is, a relation of type 3.

All historical research on macrosocial systems must move back and forth between macro and micro levels to show how the macro-level changes occurred. But there are characteristic shortcuts that some historians have used to bypass the sociological problems involved. One is the "great man theory of history," in which macrosocial changes result from the actions of a single person. A second is the "conspiracy theory of history," in which macrosocial outcomes are the intentional result of calculations on the part of some subset of actors, rather than the emergent (and often unintended) consequence of interactions among actors with numerous differing purposes. (Here, the very phrase "unintended consequences" aids in reminding that Mertonian modifications of functionalism went in the same direction proposed here, but without letting the second shoe drop by bringing actors and a theory of action explicitly back into social theory.)

If the micro-to-macro problem, the type-3 relation of figure 2, is to be seriously addressed in social theory, what must first be recognized is that it is not a single problem but several problems. A start toward addressing these problems is to recognize that interests or goals of actors may stand in different relations to one another. These different relations bring about

¹⁵ It is of course not always the case that type-3 relations are ignored. An instructive case in point is Merton's "Puritanism, Pietism, and Science," in which the thesis parallels that of Weber, except that science replaces capitalism (Merton 1949). Though Merton regards his critical empirical test as a comparison of the *numbers* of Protestants and Catholics engaged or educated in science, he (almost incidentally) shows how the concentration of Puritans generated *institutions* that furthered scientific activity (the Royal Society in England, Puritan academies). It is such evidence that moves toward showing the development of the system of science, for that system depends as much on the institutions as on the bodies that occupy them. It was evidence of this sort that Weber failed to introduce in *The Protestant Ethic*.

actions that result in different social processes, and different kinds of social institutions result. To give an idea of just what I mean, I will list some of these different relations, processes, and institutions.¹⁶

A first configuration is that of independent actors, each with differing private interests or goals and each with resources that can aid others' realization of interests. The actions that purposive actors will engage in when this configuration of interests and resources exists is social exchange, and when a number of these exchange processes are interdependent, we describe the whole set as a market institution.¹⁷ Economic markets for exchange of private goods are the most evident example, but there are many others: courtship and marriage markets (see Waller [1938] for a description of such markets and Becker [1981] for work toward a theory of marriage markets), labor markets, the market in which contributions are exchanged for status in an academic discipline, the market for admissions to universities (see Roth [1984] for a description of the institution through which the matching market for medical residencies occurs). The paradigmatic micro-to-macro theoretical work is in economics in general equilibrium theory, which shows how individual holdings and preferences combine in a setting of competitive exchange to produce equilibrium prices and distribution of goods. Little work has been done toward examining effects of the social and institutional structures within which markets operate, though experimental work by Plott and Smith (1978) has made a start. It may also be that work in network theory (see Laumann 1986) will provide contributions to this field.

More generally, this configuration of interests and resources relating two or more actors and leading to social exchange exists in a wide variety of contexts other than markets and is a component of many institutions (e.g., Peter Blau's [1964] examination of informal exchange within formal organizations or Homans's [1958] classic paper on social exchange in small groups).

A second configuration is distinguishable from the first by use of two terms, market and hierarchy. In contrast to the market as a set of relations among independent actors, a hierarchy is a set of relations in which one actor's actions are carried out under the control of another and to advance the other's interests. The resulting relation can be described as

¹⁶ There are other ways of characterizing the types of micro-macro problems than the way I propose here. Gary Becker, in a comment on an earlier draft, argues for a major distinction between social phenomena that involve purposive action by individuals separately (including both markets and principal-agent relations) and those that have some public goods component, introducing free rider problems and problems of social choice.

¹⁷ Here I neglect for simplicity the fact that some means of insuring performance in exchange is necessary, as well as other particularities of market institutions.

an authority relation, and the institutions consisting of a number of interdependent authority relations we call formal organizations or authority structures. They can ordinarily be seen as brought into existence through exchange processes in which one actor, as entrepreneur or principal, engages in a series of exchanges designed to bring about a coherent product, gaining through these exchanges (as in a labor market) the control of others' actions.¹⁸ The resulting institutions contain characteristic problems. One, which has been described as the agency problem, is the problem for the superordinate or principal of devising a structure of incentives for the agent that will best realize the principal's interests. Complementary to this is the agent's problem of realization of interests, for the agent too has interests.¹⁹

Other characteristic problems in formal organizations, still within a theory of action framework, are those of managerial decision making, involving coordination and other questions of organization. A considerable portion of the existing literature in organization theory addresses these problems. Yet, as with agency theory, it is not these managerial actions alone but their interactions with the purposive actions of subordinates that create the systemic action of the organization.

A somewhat different kind of authority system is one that can be seen as coming into existence, in an action theory framework, through a social contract among a set of independent actors, each of whom sees a benefit in giving up certain rights to a central authority. This is the classic perspective of contractarian political philosophers toward the Hobbesian problem of order. It is appropriately used not only for societal systems but also for various voluntary associations, such as trade unions, professional associations, and clubs.

This origin for authority systems generates characteristic theoretical problems. Because the origin is in a set of independent actors, one problem is that of constitution formation by these independent actors, including the allocation of rights and obligations among members. A second may be described as the free rider problem, in which the very commonness of interests means that others' actions contribute toward the common goal just as do one's own, and thus it may be in one's interest not to contribute to the common good. The classic work on this problem is

¹⁸ A major normative difference between the "individualist" political philosophers such as Hobbes and Locke and the "collectivist" political philosophers is that the latter regard such exchanges as illegitimate, except when the principal is the collectivity as a whole, i.e., the state. For a discussion, see MacPherson (1964).

¹⁹ The problems of agency constitute an area where there is a potential for fruitful interaction of sociology with law and with economics. For a review of work on agency in economics, see McDonald (1984). For a treatment of authority that is compatible with this work, see Coleman (1980a).

Olson's *The Logic of Collective Action* (1965). A third general problem is that of social choice, which is the problem of how to arrive at collective decisions or systemic actions when rights to take corporate action are not vested in a single individual. The classic work here is Arrow's *Social Choice and Individual Values* (1970). These are problems that occupy large portions of political science as a discipline; a considerable amount of formal theoretical work has been done on them under the general rubric of "public choice" (see, e.g., papers in the journal *Public Choice*).

Still a third origin of authority systems stems from the overthrow of an existing authority system via revolution. A central problem in revolutionary theory from the perspective of purposive actors is the conditions under which some subordinates in an authority system will revolt, including the organizational problems posed by the free rider problem (the fact that a potential supporter of the revolution may experience the fruits of its success regardless of participation). Although revolutionary theory is in its infancy, a number of investigators have furthered its development. Their contributions range from historical work on societal revolutions such as that of Tilly (1975) through work on contemporary social movements such as that of Leites and Wolf (1970), Oberschall (1973), and Popkin (1979), to work by psychologists on the restriction of attention that arises in small decision-making groups (e.g., groups of terrorists).

Another broad problem in the theory of revolutions concerns the course that revolutions take from the beginning until a new stable authority system is established. This may be seen as a special case of the dynamics of conflict within a social system, but the tasks that this problem poses for a social theory that has its foundations in a theory of action involve detailed empirical study as well as theory development.

A third configuration, in addition to that of independent actors in exchange relations (in markets or otherwise) and authority structures, is that of common interests within a set of independent actors. These common interests can, as described in the case of constitution formation earlier, create a setting for free rider behavior. They can also, however, lead to the development of social norms. Most social theory not based on methodological individualism assumes the existence of social norms, and most theory that is based on methodological individualism disregards their existence altogether. The central theoretical problem is to characterize the process through which individuals' actions lead norms (with sanctions) to come into existence. This is one of the least well developed areas of work; Ullmann-Margalit (1977) has done some initial work in the area, and current work of my own (1986) is in this direction.

In addition to these broad classes of configurations of actors' interests and resources leading to what may be referred to as markets, authority systems, and systems of norms, there are various others that play a part

not only in the creation of stable structures that we call institutions, but also in dynamic processes or transient states. The placement of trust by one actor in another is one such relation, which allows the flow of influence. Communication structures that permit or restrict the flow of information are another. Two processes that are central to the field of demography come under this general heading, population reproduction and geographic migration.

In sketching these various configurations, processes, and institutions, I do not claim comprehensiveness. Rather, I have attempted to indicate some of the directions that a social theory based on a theory of purposive action must take—and in some cases is already taking—in order to make the micro-to-macro transition that Parsons failed to carry out after his 1937 beginning. Work that contributes to this may include qualitative and historical work, quantitative research, and formal models.²⁰ The central criterion for evaluating its contribution is whether it contributes to knowledge of relations as shown in figure 2 above, and in particular to the most elusive of these three relations, the micro-to-macro relation shown as type 3.

EMPIRICAL RESEARCH AND A THEORY OF ACTION

In the empirical research in sociology before the watershed that I described as occurring in the United States in the 1940s (and in some of the research since then), a commonsense theory of purposive action was pervasive. The prototypical research in American sociology was a community study based on ethnographic data, and in description of the functioning of a community, the reasons behind various actors' actions constituted much of the explanation of what went on. An example is provided in the following quotation from *Middletown*, which is used as part of an examination of the importance of dress among young people in high school: "Since one of the chief criteria for eligibility for membership in the exclusive girls' clubs is the ability to attract boys, a plainly dressed girl feels the double force of this taboo by failing to receive a 'bid' which she might otherwise get. 'We have to have boys for the Christmas dances, so we take in the girls who can bring the boys,' explained a senior member of the most exclusive high school girls' club" (Lynd and Lynd 1929, p. 163).

However, in the postwatershed research, largely statistical and largely confined to explaining individual behavior, causal explanation based on statistical evidence has replaced purposive explanation. One way of describing this change is to say that statistical association between variables

²⁰ An example of qualitative work that has begun this is Michael Hechter's *The Microfoundations of Macrosociology* (1983).

has largely replaced meaningful connection between events as the basic tool of description and analysis. The "meaningful connection" was ordinarily provided by the intentions or purposes of an actor or combination of actors.²¹

A frequent virtue of the research based on meaningful connections was its richness of description, which provides an understanding of the course of social action. A frequent defect was an inability to go beyond system description, in order to pose and answer analytical questions.

In the context of the kind of causal explanation that has come to pervade quantitative research in sociology, with behavior "affected by" various individual characteristics and social conditions, it seems strange indeed to conceive of examining those same research problems as involving purposive action. In fact, when it is individual action rather than systemic action that is to be explained, the traditional commonsense use of purpose has two serious defects in explaining action, no matter how sophisticated the "reason analysis" (a mode of explanation developed in the 1940s and 1950s, in which survey respondents were asked their reasons for taking an action and these reasons entered the analysis). First, it includes only those elements of which the actor is aware, and second, the actor's explanation must be seen itself as a social action directed toward a goal in the context of which the reason or purpose was stated.

Yet there is another quite different mode of introducing a theory of action into the analysis of individual behavior. In analysis of individual-level survey data, economists study a number of the same individual-level problems examined by sociologists. They do so through statistical analysis of a kind that differs somewhat from that of sociologists analyzing the same or comparable data.

For example, a sociologist setting out to study the participation of women in the labor force through statistical analysis will introduce various possible "determinants" of labor force participation, such as age, marital status, number and ages of children, and husband's income. The explanation will consist of the relative effects of each of these factors on labor force participation.

An economist setting out to study the same phenomenon will begin differently, by assuming that each woman has a utility function and that she will act to maximize that utility (see, e.g., Heckman 1974; Mincer 1974).²² If her time in the household is of greater value than the wage she

²¹ In some cases, the meaningful connection was provided by other content, such as the objective relatedness of the events themselves. But a textual analysis of one of the prewatershed community studies will show that subjective states of the actors often supplied the meaning that made it possible to connect events that would otherwise be unrelated.

²² Sometimes it is the household that is regarded as having the utility function.

would earn in the labor force, she will not enter the labor force; otherwise she will seek a job—a special case of taking the action that maximizes utility.

In this model of action, there are various “arguments” to the utility function, and here some of the same variables that would be introduced by the sociologist enter. As a result, the econometric analysis carried out by the economist may end up looking similar to the regression analysis carried out by the sociologist. The interpretations, however, are very different. In the sociologist’s analysis, the variables “affect labor force participation,” whereas in the economist’s analysis, the variables affect the value of the woman’s time in the household, and her participation depends on her comparison of that value with what she can earn on the labor market.

There has been no careful evaluation of the virtues and faults of these two approaches to the empirical analysis of action. It is safe to say, however, that there are merits in both approaches: the more open-ended approach that remains agnostic about the mechanism through which a variable affects behavior and the more theoretically structured model that specifies how a variable affects action. But there are additional virtues in the economist’s approach from the point of view of social theory. By containing a purposive (utility maximization) theory of action, it has greater power to predict how action will change as conditions change (e.g., how a woman’s labor force activity will change as the value of her time outside the labor force changes). In addition, it has the merit of compatibility with the conceptual foundations that underlie much of social thought since the 17th-century natural rights political philosophers.

Yet neither of the approaches I have outlined goes beyond a type-1 relation to address the micro-to-macro problem. The question arises whether systematic quantitative research based on individual-level data can be relevant not merely for explanation of individual behavior but also for explanation of social outcomes, that is, for relations of type 3. A start toward answering this can be made by comparing Featherman and Hauser’s *Opportunity and Change* (1978) with *Inequality* (1972) by Jencks et al. The problem posed by Featherman and Hauser was an *individual-level* problem of status attainment: What are the determinants of an individual’s occupational status attainment? The question posed by Jencks and his colleagues was the *societal-level* problem: Does an increase in the educational level of society reduce income inequality? It was this difference in the question posed, not a question about individual status attainment but a question about societal inequality, which attracted the attention of those outside the social sciences to the Jencks book, whereas interest in the Featherman and Hauser book was largely confined to stratification theorists within sociology. Both books concerned the rela-

tion of education to occupational outcomes, but their questions were on quite different levels. The Jencks book constituted something of a milestone in quantitative social research based on survey data, precisely because it posed a societal-level question (effect of the educational level of society on income inequality) rather than an individual-level type-1 or type-2 question.

Yet the research methods that are common in quantitative sociology led both sets of investigators to individual-level analyses that were very similar in character. Although Jencks and his colleagues posed a societal-level question about education and income, they answered an individual-level type-1 question: What is the effect of change in an individual's education on that individual's income (in effect holding constant all other individuals' education)? But this answer gives no information on another question that is necessary for answering the societal-level question, How does a change in an individual's education affect his income *when the education of all others in his labor market context changes as well?*²³ Only with the answer to this latter question, a contextual one, can we begin to address the societal problem.²⁴

This example illustrates the kinds of steps necessary to examine a type-3 relation. The movement from micro to macro level in this case involves a potentially strong interdependence between one person's education and that of others in the same context, interdependence that can be described as a competitive effect. To carry out an analysis of the type-3 micro-to-macro relation involves specifying the kind of interdependence that may exist, modeling it (more precisely than I have done here), and then setting up an analysis that tests this model. In this case, it is possible to begin such an examination by quantitative analysis of individual behavior of a sort that is familiar to the sociologist. An analysis of the effect of education on income requires inclusion of a contextual term, the education of others in the same context (i.e., the same competitive market). A zero coefficient for the contextual term implies that the individual-level rela-

²³ Economists call this a general equilibrium problem, in contrast to a partial equilibrium problem. In a market, the partial equilibrium problem is based on the question of how a supplier changes the level of production as the price changes or how a consumer changes the level of demand as the price changes. These are type-1 problems. Economists then typically divide type-3 problems into two parts: creating aggregate demand and supply curves for the market; then putting these together to determine what is the price and quantity equilibrium (the general equilibrium) when these two parts function as the components of a market.

²⁴ The difference between the individual-level question and the societal-level question is closely related to the difference posited by economists between the private rate of return to education and the public rate of return, for when the education of all changes—unless the higher education brings higher productivity—no person's income could change because total national productivity would not change.

tion between education and income carries forward to the societal-level (or at least the market-level) outcome, and simple aggregation of individual-level results will suffice. This would imply a social rate of return to education equal to the individual rate. A nonzero coefficient implies that the type-3 relation is more complicated, and simple aggregation will not suffice.²⁵ (A negative coefficient equal in size [but of opposite sign] to the coefficient on individual education implies that the societal rate of return is zero.) An explicit model by which this interdependence leads to a social outcome becomes necessary.

Competition, of course, is only one form of interdependence between individual actions. In attitude or value change, for example, the model linking individual action to behavior of the system would not be one involving competition, but it might involve trust or social influence. Thus a macro-level change that (through a type-2 relation) produces direct change in individual values can, through amplification, have a much greater ultimate effect.²⁶ The type-3 relation shows the process through which the amplification occurs. A failure to develop such models and test them may be in part responsible for the inability of sociologists to find such things as effects of mass media on social norms—since a social norm is a macro-level variable that depends on, but is not the same thing as, individual values.

What I have done here in describing what is necessary to study type-3 relations with quantitative data is merely to sketch the outlines of a research program. Such a program involves extensive changes in the philosophy, design, and analysis of quantitative research based on individual-level data, but these changes are necessary if such data are to be used to study the behavior of social systems rather than merely that of individuals.

²⁵ A simple geometric model of this aspect of the labor market could be constructed with average educational level on the horizontal axis and price paid for education (i.e., salary per unit of education) on the vertical. The partial equilibrium line representing demand for education by employers would be downwardly sloping to the right, and the supply of education by employees would be upwardly sloping to the right. The general equilibrium would be at the intersection of these curves. The displacement of this equilibrium price as the average level of education changes is given by the regression coefficient in a regression of average real income on average education across isolated labor markets. In this case the lack of isolation of labor markets within a society might make an analysis of the sort described infeasible. In other situations, however, where the context is that of a school and the problems concern student outcomes (i.e., the price paid in grades for a given increment in absolute achievement), analysis of similar problems is feasible.

²⁶ The use of survey research to study such interdependence was begun in research like Katz and Lazarsfeld's *Personal Influence* (1955) and Coleman, Katz, and Menzel's *Medical Innovation* (1966), but in the absence of an explicit conception of how individual change may be related to social change, this was not pursued in any extensive way.

Yet it is possible to move with empirical research a step beyond the examination of isolated micro-to-macro relations, toward studying a system of action. That is what social historians and ethnographers have traditionally done using qualitative data (for a still-instructive example at the level of a neighborhood social system, see Whyte's *Street Corner Society* [1943]). This research is ordinarily descriptive in character without theoretical aspirations, though it may well provide insights for theory construction.

To carry out analysis of systemic action with quantitative data, however, requires a formal theoretical model that relates individual actions to systemic functioning.²⁷ Some research of this sort has been carried out on community functioning (see Marsden 1981; Marsden and Laumann 1977; Pappi and Kappelhoff 1984), but little more than beginnings have been made. Here, it can reasonably be said, is where a most promising research-and-theory frontier lies for sociology.

The deficiencies in social research that I have described are not merely impediments to the development of social theory. These deficiencies are in part responsible for quite serious biases in the way certain problems in society—absent believable input from sociologists—are formulated and addressed. For example, commonsense and simple paradigms from psychology lead to the prescription to schoolteachers to “individualize instruction” and attend to “individual needs” of children. The absence of theory and research by sociologists showing the way that interdependent individual actions lead to the social structure and culture of the classroom—and these in turn shape motivation, effort, and learning—allows such misleading prescriptions to go unchallenged. Or the simple perfect-market paradigms from economics lead to prescriptions in economic policy for unlimited free trade. Only “economic consequences” are taken into account, and consequences for social institutions are ignored. When these concerns are reintroduced by practicing politicians, it is without intellectual guidance from social theory and research.

CONCLUSION

The promise that Talcott Parsons held out in 1937 of a voluntaristic theory of action has remained unrealized, and since then social theory and social research have moved along diverging paths. Yet this divergence is largely a result of a failure to analyze the structure that social theory based on a theory of action must have. When the sources of this divergence are exposed, as I have done here, the directions for productive theory construction and for research that can contribute to theory (and

²⁷ For work toward the development of such a model see Coleman (1986).

thus increase the usefulness of the sociological enterprise for society) become evident. I have attempted in the final two sections of the paper to indicate what those directions are.

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