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PRESIDENTIAL ADDRESS:
TWO METHODS IN SEARCH OF A SUBSTANCE*

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In tune with the overall theme of these meetings, this Presidential Address is an exercise in the uses of controversy. I am perturbed about present developments in American sociology which seem to foster the growth of both narrow, routine activities, and of sect-like, esoteric ruminations. While on the surface these two trends are dissimilar, together they are an expression of crisis and fatigue within the discipline and its theoretical underpinnings. I shall eschew statesmanlike weighing of the pros and cons of the issues to be considered and shall attempt instead to express bluntly certain of my misgivings and alarms about these recent trends in our common enterprise; let the chips fall where they may.

Building on other students of science, Diane Crane (1972) has argued that scientific disciplines typically go through various stages of growth accompanied by a series of changes in the characteristics of scientific knowledge and of the scientific community involved in the study of the area. In stage one, important discoveries provide models for future work and attract new enthusiastic scientists. In the next stage, a few highly productive scientists recruit and train students, set priorities for research and maintain informal contacts with one another. All this leads to rapid growth in both membership and publications. But in later stages the seminal ideas become exhausted and the original theories no longer seem sufficient. At this

point a gradual decline in both membership and publication sets in, and those who remain develop increasingly narrow, specialized, though often methodologically highly refined, interests. Unless fresh theoretical leads are produced at this point to inspire new growth, the field gradually declines.

Such stages of growth and decline are, of course, not limited to the sciences. In other spheres of culture, religion and the arts for example, similar phenomena have been observed (cf. Thomas O'Dea, 1966; Max Weber 1963; Alfred Kroeber, 1957). One need only think of the creative effervescence in the communities of Christ's immediate disciples and their direct successors in contrast to quotidian routines and ritualized devotions of the later stages in what had now become the Church of Rome. Or consider the art of Byzantine icon painting where, after the early creative period, the same motives, even techniques, were endlessly repeated so that it takes a specialist to distinguish between paintings executed not just decades but even centuries apart. In religion and the arts, however, innovation is not a necessary condition for flowering and appeal, but in the sciences, when no innovation is forthcoming *rigor mortis* is not far away.

The findings of Crane and others in the sociology of science typically refer not to a whole branch of knowledge but only to sub-fields within such branches. It would therefore be wrong to apply these findings to sociology as a whole, composed as it is of a wide variety of sub-areas each with its own pattern and rhythm of growth. Yet permit me nevertheless roughly, and per-

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haps rashly, to sketch what I consider the present condition of sociology as a whole.

By and large, we are still in the second stage of growth, the stage of lively development, of creative ability, and innovative effervescence. Yet there now appear a number of danger signs suggesting that the fat years of the past may be followed by lean years, by years of normal science with a vengeance, in which not only the mediocre minds but even the minds of the best are hitched to quotidian endeavors and routine activities. This seems portended by the recent insistence among many sociologists on the primacy of precise measurement over substantive issues.

The germ of the idea for this address came to me earlier this year when a friend of mine, the editor of a major sociological journal, explained with some pride that, no matter what the substantive merits of the paper might be, he would refuse to accept contributions using old-fashioned tabular methods rather than modern techniques of regression and path analysis. I gather, for I have respect for his opinion, that he meant that he would not accept articles requiring modern methods of data analysis that do not make use of such techniques. Yet, though his intentions are undoubtedly excellent, I submit that such an orientation is likely to have a dynamic of its own and that, inadvertently perhaps, it will lead to a situation where the methodological tail wags the substantive dog, where as Robert Bierstedt (1974:316) once put it, methods would be considered the independent and substantive issues the dependent variable.

My friend's voice is, of course, not a lonely one. In fact, he expressed what is tacitly assumed or openly asserted by a growing number of our colleagues. Fascinated by new tools of research, such as computers, that have come to be available in the last decades, and spellbound by the apparently irresistible appeal of techniques that allow measures of a precision hitherto unattainable, many of our colleagues are in danger of forgetting that measurements are, after all, but a means toward better analysis and explanation. If concepts and theoretical notions are weak, no measurement, however precise, will advance an explanatory science.

The fallacy of misplaced precision con-

sists in believing that one can compensate for theoretical weakness by methodological strength. Concern with precision in measurement before theoretical clarification of what is worth measuring and what is not, and before one clearly knows what one is measuring, is a roadblock to progress in sociological analysis. Too many enthusiastic researchers seem to be in the same situation as Saint Augustine when he wrote, on the concept of time, "For so it is Oh Lord, My God, I measure it but what it is I measure I do not know" (Saint Augustine 1953:35).¹

No doubt, modern methods of research have immeasurably advanced sociological inquiry. Only sociological Luddites would argue that computers be smashed and path diagrams outlawed. What I am concerned with is not the uses but rather the abuses of these instruments of research. They serve us well in certain areas of inquiry, but they can become Frankenstein monsters when they are applied indiscriminately and, above all, when their availability dictates the problem choices of the investigator so that trivial problems are treated with the utmost refinement.

The sheer availability of new methods encourages their use and seems to release the user from the obligation to decide whether his problem or findings are worthy of attention. By way of illustrating this let me quote from the summary of a recent paper by Oksanen and Spencer (1975) in one of the official journals of our Association, *The American Sociologist*: "A rather large degree of explanatory power has been achieved by our regression model, in terms of overall goodness of fit and in terms of significant variables. It is of considerable interest to learn that high school performance is an invariably significant indicator of 'success' in the [college] courses examined." Abraham Kaplan's (1964:28) delightful formulation of the Law of the Instrument comes to mind here: "Give a

¹In fairness to St. Augustine, modern physics tends to agree with his position. "Time is a primitive element in the logical structure of physics," state W. H. Cannon and O. G. Jensen (1975), "consequently physics does not explicitly define time but rather specifies operational procedures for its measurement in units of seconds."

small boy a hammer, and he will find that everything he encounters needs pounding.”

The fact is that, though in principle these new methods and technologies could help us achieve greater theoretical sophistication, they are used as “magic helpers,” as a shortcut to, or even replacement for, theoretical analysis rather than as a means for furthering it. An insistence on the use of these refined methods, no matter what, makes it fall prey to Kaplan’s law.

It would be easy, and perhaps entertaining, to go on quoting similar instances, but each of us can easily supply other examples. Let me instead return to the serious problems now faced by our discipline, many of which have been created, or at least accentuated, by the revolution in methodology and research technology.

Our new methodological tools may well be adaptable to deal with a great variety of topics and problems, and I hope they are. However, the data needed for path or regression analyses are much harder to come by in some areas than in others, and in many of them it would take a great deal of sophistication to discover and handle useable indicators. Consequently; under the pressure to publish to avoid perishing, or to gain promotion, or simply to obtain the narcissistic gratification that comes from seeing one’s name in print, it is more attractive to do what is quick and easy. This is so in every scholarly field and even in the healing arts. In psychiatry, for example, it leads to prescribing drugs instead of psychotherapy, often not as a result of deliberate choice between alternative diagnoses and prognoses, but simply because drug therapy is easy to administer and promises quick results, superficial though they may be. In the world of scholarships, moreover, not only the choice of technique but even the choice of the problem tends to be determined by what is quick and easy rather than by theoretical considerations or an evaluation of the importance of the questions that are raised. Moreover, the uses of a sophisticated technological and methodological apparatus gives assurance, but often deceptive assurance, to the researcher.

Sociology is not advanced enough solely to rely on precisely measured variables. Qualitative observations on a small universe can provide theoretical leads that may at a later

stage become amenable to more refined statistical treatment. To refrain from using descriptive data because they may lend themselves only to tabular presentation will not only diminish our theoretical powers but will retard the refinement of statistical analysis as well.

Training the new generation of sociologists not to bother with problems about which data are hard to come by, and to concentrate on areas in which data can be easily gathered, will result, in the worst of cases, in the piling up of useless information and, in the best of cases, in a kind of tunnel vision in which some problems are explored exhaustively while others are not even perceived.

There is at least some evidence that we tend to produce young sociologists with superior research skills but with a trained incapacity to think in theoretically innovative ways. Much of our present way of training as well as our system of rewards for scientific contributions encourages our students to eschew the risks of theoretical work and to search instead for the security that comes with proceeding along a well traveled course, chartered though it may be by ever more refined instruments of navigation. J. E. McGrath and I. Altman (1966) have shown this in instructive detail for small-group research, but it applies in other areas as well.

Careers, especially those of people with modest ambitions, can be more easily advanced through quantity rather than quality of publication. This leads to an emphasis on methodological rigor, not on theoretical substance. One way to publish rapidly is to apply “the [same] procedure, task, or piece of equipment over and over, introducing new variables or slight modification of old variables, and thereby generate a host of studies rather quickly” (J. E. McGrath and I. Altman, 1966:87). The formulation of theories, moreover, is time consuming, and may not lend itself easily to publication in journals increasingly geared to publishing empirical research, and to reject “soft” theoretical papers. There exist, then, a number of factors in our present systems of training and of rewards that exercise pressures on incoming generations of sociologists to refine their methods at the expense of developing innovative lines.

This is not inherent in methods per se, but it is, let me emphasize again at the risk of

repetition, a temptation for lesser minds. And here as elsewhere inflation has set in. However, it is important to note that even the better minds, those who have been able to use the new methods innovatively, are *nolens volens* geared to deal with problems, important as they may be, for which these methods promise quick results. Even in the serious work that is being done with the help of the new statistical techniques there lurks the danger of one-sided emphasis.

Stratification studies of recent years will illustrate this point. This field has benefited a great deal from modern path analytical methods whose power is perhaps shown at its best in Blau and Duncan's *The American Occupational Structure* (1967). Path analysis allows these authors systematically to trace the impact of such factors as father's occupation, father's educational attainments, and son's education and first job on the son's placement in the occupational hierarchy. It allows for the first time the assessment in precise detail of the ways in which occupational status in a modern industrial society is based on a combination of achieved and ascribed characteristics. It permits, in fact, the assessment of the contributions of social inheritance and individual effort in the attainment of socioeconomic status.

Yet, to use an important distinction by John Goldthorpe (1972), this research contributes to the understanding of the *distributive*, not to the *relational* aspects of social class. The focus is predominantly on the impact on individual careers of differences in parental resources, access to educational institutions and the like, or they center attention upon individual characteristics of people variously placed in the class structure. There is no concern here with the ways in which differential class power and social advantage operate in predictable and routine ways, through specifiable social interactions between classes or interest groups, to give shape to determinate social structures and to create differential life chances. The first and only entry under path analysis in the 1966-1970 Index to *The American Sociological Review* (vols. 31-35) refers to a paper by Hodge and Treiman (1968) tracing the effects of the social participation of parents on that of their offspring. There were only two papers analyzing problems in social stratification with the aid of path analytical

methods in the 1973 (vol. 38) volume of *The American Sociological Review*, and both (Kelley, 1973, and Jackman and Jackman, 1973) deal with the *distributional* aspects of social stratification or with the characteristics of individuals in the class hierarchy.

The 1974 volume of *The American Sociological Review* published three papers on stratification using path analysis, two of which (James N. Porter, 1974; Alexander and Eckland, 1974) deal again with distributional aspects. The *Rose Monograph Series* published by the A.S.A. had issued twelve titles up to the end of 1974. Of these, four, that is one-third, deal with problems of stratification and use highly sophisticated research methods. Their titles speak for themselves: "Socio-economic Background and Educational Performance" (Robert M. Hauser, 1972); "Attitudes and Facilitation in the Attainment of Status" (Ruth M. Gasson, Archibald O. Haller and William Sewell, 1972); "Looking Ahead: Self-conceptions, Race and Family as Determinants of Adolescent Orientation to Achievement" (Chad Gordon, 1972); and "Ambition and Attainment: A Study of Four Samples of American Boys" (Alan C. Kerckhoff, 1974). It would appear as if authors and editors of the series are fixated on the problems of making it.

Yet a class system is not only a distributive system, in which individuals are assigned to their respective niches in terms of background and training, nor is its analytical significance exhausted by individual characteristics of people who make their way within it; it is also a system that is shaped by the interaction between various classes and interest groups differentially located within the social structure. It is a system, moreover, in which command and coercion play major parts. Classes and other socioeconomic groups use their resources so as effectively to maintain or advance their positions and to maximize the distribution of material and social benefits to their advantage. Exclusive concern with the distributive aspects of stratification directs attention away from the socio-political mechanisms through which members of different strata monopolize chances by reducing the chances of others. Max Weber (Gerth and Mills, 1947), building on Karl Marx, saw this with exemplary clarity when he stated that, "It is the most elemental economic fact that the way in which

disposition over material property is distributed among a plurality of people . . . in itself creates specific life chances. According to the law of marginal utility this mode of distribution excludes the non-owners from competing for highly valued goods. . . . It increases . . . the power [of the propertied] in price wars with those who, being propertyless, have nothing to offer but their labor. . . .”

One need not accept Marx's dichotomous scheme of class analysis in order to agree that classes are linked in asymmetrical relationships. The notion of a class of owners of means of production is dialectically bound to the notion of a class of non-owners. Just as in the classical Indian caste system, as Louis Dumont (1970) has shown, the purity of the Brahmins is inseparable from the impurity of the Untouchables, so the central characteristics of the class systems is not that there are propertied and the propertyless but that they are mutually interdependent. Randall Collins, arguing against a narrowly defined sociology of poverty, puts the matter well when he writes, “Why some people are poor is only one aspect of the same question as to why some people are rich: a generalized explanation of the distribution of wealth is called for if one is to have a testable explanation of either particular” (Collins, 1975:17).

A system of stratification consists in relationships between groups or categories of men and women which sustain, or alter their respective access to life chances. It is one thing to investigate the ways in which, for example, people manage to attain the status position of medical practitioners in American society, it is quite another to analyze the institutions that help the American Medical Association to monopolize the market for health care by restricting access. What needs analysis is not merely the ladder to medical success but those institutional factors that contribute to the maintenance of a system of medical service that effectively minimizes the life chances of the poor (Kelman, 1974).

Analysis of the distributional aspects of stratification systems can dispense with considerations of social and political power; concern with the relational aspects, however, directs attention to the power contentions that make for the relationships which establish differential class privileges, and create patterned conflicts between unequally

benefited contenders. When no question is asked about who benefits from existing social and political arrangements, stratification research, no matter how sophisticated its methodological tools, presents a “bowdlerized” version of social reality. When the causes and consequences of differential location in the class structure remain unanalyzed, the whole area of research so brilliantly opened up by Robert K. Merton's (1968: chapters 6 and 7) seminal anomie paradigm remains unexplored.

I am not arguing, let this be clearly understood, that concern with the structures of power and exploitation is necessarily better than preoccupation with the pathways to individual mobility. There is surely a need for both types of studies. I believe, however, that the methodological tools that are available help focus on the latter. It must be added—lest I be accused of technological determinism—that such restrictions are also rooted in the prevailing American ideology of individual achievement. But taken together, the ideology combined with the use of statistical methods in limited areas, prevents the growth of our discipline and curtails our ability to strive for a full accounting and explanation of the major societal forces that shape our common destiny and determine our life chances. If the computer and the new methodological tools we possess now are not yet adequate for handling some of the issues I have raised, then let us at least press forward with theoretical explorations even if they should later have to be refined or modified by more precise empirical research. Let us not continue on a path about which one may say with the poet Roy Campbell (1955:198): “They use the snaffle and the curb all right. But where is the bloody horse.”

Another symptom of the decline of a discipline, as Diana Crane (1972) indicates, is exclusive insistence on one particular dimension of reality and one particular mode of analysis by cliques or sects who fail to communicate with the larger body, or with one another. Under such conditions, a community of scholars will gradually dissolve through splitting up into a variety of camps of ever more restricted esoteric and specialized sects, jealously fighting each other and proclaiming that they alone possess the keys to the kingdom, while others are not just in

error, but in sin. Under such conditions the only dialogue between antagonistic camps is a dialogue of the deaf. Such tendencies have also become apparent in the last few years of the history of our discipline. This brings me to my second topic of examination, the assessment of ethnomethodology.

If I understand correctly, ethnomethodology aims at a descriptive reconstruction of the cognitive map in people's minds which enables them to make sense of their everyday activities and encounters. It is a method that endeavors to penetrate to the deeper layers of the categorical and perceptual apparatus that is used in the construction of diverse realities. The method also aims at a rigorous description of ordinary linguistic usage and speech acts. As such it seems aggressively and programmatically devoid of theoretical content of sociological relevance. Limiting itself by a self-denying ordinance to the concrete observation of communicative codes, subjective categorizations, and conversational gestures, it underplays the behavioral aspects of goal directed social interaction. It focuses instead on descriptions of definitions of the situation, meaning structures, conversational exchanges and the mutual modifications of images of self in such interchanges. Ignoring institutional factors in general, and the centrality of power in social interaction in particular, it is restricted to the descriptive tracing of the ways in which both individual actors and students of their activities account for their actions.

Ethnomethodologists put particular stress on the contextuality of accounts and meanings, their imbeddedness in the interactive context, their "situated" nature. Given the constitutive situatedness of any act, it is asserted that no objective generalizing approach is possible in the social sciences which by their very nature can only provide ideographic description. In some versions of ethnomethodology, inter-subjectivity is consciously neglected so that one ends up with a view of individual actors as monads without windows enclosed in their private and unshareable universes of meaning.

As distinct from path analysis and similar methods, ethnomethodology has not found ready acceptance within our discipline, in fact it has never sought such acceptance. It has consciously limited its appeal to devoted followers united in the knowledge that they

possess a special kind of insight denied to outsiders.

Ethnomethodology claims access to types of knowledge not accessible to the sociological *vulgus*. Write Zimmermann and Pollner (1970), for example, on the ethnomethodological reduction, one of the mainstays of the method: "The reduction does not generate research that may be regarded as an extension, refinement, or correction of extant sociological inquiry. . . . The reduction constitutes as its phenomenon an order of affairs that has no identifiable counterpart in contemporary social science." More typically still than the oft-repeated insistence that ethnomethodology has a unique subject matter is the esoteric and particularistic nature of the pronouncements of its practitioners. Consider, for example, a paper by David Sudnow (1972) entitled "Temporal Parameters of Interpersonal Observation" which turns out to deal with the glances people exchange with one another or direct at the passing scene. It is concerned, as the author elegantly puts it, with "the issue of glance timing importance" (1972:273). "Let us consider," he states, "the situation of 'walking across the street,' where an orientation to be clearly so seen is held by virtue of the noted presence of a rapidly approaching vehicle. Here a familiar traffic situation may be regularly imagined where a mere and single glance is expected, where the sufficiency of the mere and single glance is criterial for bringing off safe passage. . . and where, as a consequence, the concern for a correspondence between the 'details' of what we are doing and what we are seen at a single glance to be doing, may be of paramount concern" (1972:269). When I try to explain to my four-year old grandson that he should always be careful when crossing a street, I say to him, "Always watch for passing cars." I do not think that Sudnow's jargon conveys anything more. Each field, to be sure, must construct its own defined terms, but what is developed here is a restricted code of communications rather than open scientific vocabulary (Bernstein, 1971).

It is much too facile simply to poke fun at a group of people who profess central concern with linguistic aspects of interactive processes and yet seem unable to handle the vernacular. But the fact is that such language diseases have sociological significance in the develop-

ment of particularistic communities of True Believers. To begin with, esoteric language erects barriers against outsiders and confirms to the insiders that they have indeed a hold on some special truth. But there is more, such jargon, as the philosopher Susanne Langer puts it, "is language which is more technical than the ideas it serves to express" (1973:36), so that it can successfully camouflage relatively trivial ideas. Moreover, esoteric jargon may serve to bind the neophyte to his newfound anchorage. People tend to value highly those activities in which they have invested a great deal. Having invested considerable time and energy in mastering an esoteric vocabulary, people are loath, even when some disillusionment has already set in, to admit to themselves that what has cost them so much, might, after all be devoid of genuine value. Hence the particularistic vocabulary is not due to happenstance; it serves significant functions in marking boundaries and holding members.

Yet another characteristic with obvious functional value that ethnomethodologists share with similar close groupings in other scholarly areas, is the characteristic habit to limit their footnote references almost exclusively to members of the in-group or to non-sociologists, while quoting other sociologists mainly in order to show the errors of their way. There is, in addition, a peculiar propensity to refer to as yet unpublished manuscripts, to lecture notes and research notebooks.

It will be recognized that the characteristics I have outlined are those of a sect rather than of a field of specialization. I here define a sect as a group that has separated in protest from a larger body and emphasizes an esoteric and "pure" doctrine that is said to have been abandoned or ignored by the wider body. Sects are typically closed systems, usually led by charismatic leaders and their immediate followers. They attempt to reduce communication with the outside world to a minimum while engaging in highly intense interactions between the True Believers (Coser, 1974). Sects develop a special particularistic language, distinctive norms of relevance, and specialized behavior patterns that effectively set off the believers from the unconverted, serve as a badge of special status, and highlight their members' differentiation

from the larger body of which they once formed a part.²

Yet what is functional for the sect is, by the same token, dysfunctional for those who are not among the elect. Blockage of the flow of communication is among the most serious impediments of scientific developments. A science is utterly dependent on the free exchange of information between its practitioners. Preciseness and economy in information flow makes for growth, and blockages lead to decline (cf. Crane, 1972). But the language of ethnomethodology, as James Coleman (1968; 130) once put it, makes for, "an extraordinarily high ratio of reading time to information transfer." More generally, an esoteric language can only serve to dissociate a body of people who were once united in common pursuits. As in the story of Babel in Genesis, "And the Lord said, 'Behold they are one people, and they have one language; and this is only the beginning of what they will do. . . Come, let us go down, and there confuse their language, that they may not understand one another's speech'."

Even though the sect is still quite young, the splits and fissions that typically beset sectarian developments have already set in. I do not profess to be knowledgeable about the detailed grounds of these developments (see Attewell, 1974 for an excellent mapping and critique), but shall only sketch some of them very roughly. At present, the ethnomethodology of Garfinkel differs significantly from that of Sacks which, in turn, is far removed from the concerns of Blum or the researches of Cicourel. Some versions are, in fact, solipsistic, others attend to intersubjective meanings, some admit the existence of invariant rules and procedures that transcend situations, others deny the possibility of any analysis that is not situation-specific. Some find philosophical anchorage in the German idealistic tradition and its Husserlian offshoots, others make use of British linguistic philosophy and seem to have replaced the guidance of Alfred Schuetz by that of Ludwig Wittgenstein. Some concentrate on the analysis of unique events, others attend to invariant

² For an earlier analysis of sect-like characteristics in sociology cf. Coser (1955).

properties of situated actions. The only thing all of them still seem to hold in common is the rejection of the possibility of an objective study and explanation of society and history, and a celebration of that long-dead warhorse of German idealistic philosophy, the transcendental ego.

Concern with the hypertrophy of wordage among ethnomethodologists and their other sectarian characteristics should, however, not preempt all of our attention. It is axiomatic among sociologists of knowledge that the origin of ideas does not prejudice their validity. It is possible that important and fruitful ideas may indeed develop in sectarian milieus. This has, in fact, often been the case, from the inception of puritanism to the emergence of psychoanalysis in the Viennese sect of Freud's immediate disciples.

Yet, when one turns to the problems that ethnomethodology tries to illuminate one is struck, for the most part, by their embarrassing triviality. We have already encountered Sudnow's "glancing research." Schegloff (1968) has spent productive years studying the ways in which people manage to begin and end their telephone conversations. I am not denying that "Studies of the Routine Grounds of Everyday Activities" (Garfinkel, 1967) may uncover significant and valuable matters, but in my considered judgment what has so far been dug up is mostly dross or interminable methodological disquisitions and polemics. Bittner's (1967) fine studies of the police or Cicourel's (1968) analysis of juvenile justice and a very few other good studies are not enough to justify the enormous ballyhoo surrounding ethnomethodology.

In general, it would seem to me, that we deal here with a massive cop-out, a determined refusal to undertake research that would indicate the extent to which our lives are affected by the socioeconomic context in which they are embedded. It amounts to an orgy of subjectivism, a self-indulgent enterprise in which perpetual methodological analysis and self-analysis leads to infinite regress, where the discovery of the ineffable qualities of the mind of analyst and analysand and their private construction of reality serves to obscure the tangible qualities of the world "out there." By limiting itself to trying to discover what is in the actors' minds, it blocks the way to an investigation of those central

aspects of their lives about which they know very little. By attempting to describe the manifest content of people's experiences, ethnomethodologists neglect that central area of sociological analysis which deals with latent structures. The analysis of ever more refined minutiae of reality construction, and the assertion that one cannot possibly understand larger social structures before all these minutiae have been exhaustively mapped, irresistibly brings to mind Dr. Johnson's pregnant observation that, "You don't have to eat the whole ox to know that the meat is tough."

Path analysis, as has been shown, is a method that found quick acceptance among wide circles in the sociological discipline because it provided technical means for more precise measurements hitherto unavailable; ethnomethodology in contrast, found acceptance only among a small number of practitioners huddled around a charismatic leader and his apostles. The first was widely communicated through the various informational networks, both personal and impersonal, available to sociologists; the second developed particularistic codes of communication that effectively restricted access to all but the insiders. Yet what both have in common is a hypertrophy of method at the expense of substantive theory. The first has been used as an encouragement to neglect important areas of inquiry even while it has brought about greater precision of measurement in other areas, some important, some trivial. The second lends itself at best to atheoretical mappings of cognitive categories, and deliberately eschews concern with most of the matters that sociology has been centrally concerned with ever since Auguste Comte. In both cases, I submit, preoccupation with method largely has led to neglect of significance and substance. And yet, our discipline will be judged in the last analysis on the basis of the substantive enlightenment which it is able to supply about the social structures in which we are enmeshed and which largely condition the course of our lives. If we neglect that major task, if we refuse the challenge to answer these questions, we shall forfeit our birthright and degenerate into congeries of rival sects and specialized researchers who will learn more and more about less and less.

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