# Some Basic Issues in Social System Methodology

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OVERVIEW:

Two kinds of problems plague social science: empirical research methodology, and meta-theorical biases or presuppositions toward the study of society.

RESEARCH METHODOLOGY: mainly STATIC, FEW VARIABLES, LINEAR, NON-SYTEMIC, UNAVAILABLE DATA. A Systems orientation usually implies a methodology based on dynamics, nonlinearity, and a substantial number of variables. This implies a need to develop computerized simulation models rather than lean so heavily on the mathematical techniques of the physical sciences.

META-THEORETICAL: simplistic and/or naive understanding of issues in the philosophy of science and theory construction; use of inappropriate analogies such as that between society organism of between society and organism or between society and the commercial marketplace; or a misplaced attack on a defunct 'positivism' as a disguise for a growing disenchantment with science and an attempt to turn back to older untried and undeveloped philosophical speculations such as phenomenology or hermeneutics or utilitarianism; widespread assumptions of a rationalist model of behavior (as compared to, e.g., normative and collective behavior models), or behavioristic assumptions denying any importan causal role for internal mental states, or overly subjectivistic or individualistic views that leave out the role of social an cultural structures in shapin and constraining individual actions and interactions (as in the simplistic "free enterprise" assumptions of old and new economic theory).

**I.** Since the central topic of this conference is Methodology, I would like to discuss, although all too briefly, some broadly defined problems in contemporary Sociology, especially en the U.S.A. My discussion reflects my background in Sociology and a strong interest in a modern systems orientation. The argument is mainly critical-perhaps overly critical-but this is necessary to the dialectic process of advancement.

I assume that the basics of the systems point of view are well-known: A system is seen as a set of components and forces interacting among themselves and with an environment to produce some kinds of characteristic holistic behavior which is often self-regulatin and/or adaptive. This implies the need to go beyond the analytic methodology of most previous science and focus on a synthesizing holistic methodology studying emergent system properties and behaviors.

First of all, there are the basic research limitations that we all know about but often try to ignore:

our studies are usually static;
our measurements and mathematics usually
assume linearity en the data, and
parameters in equations never change over time;
we must confine ourselves to very few variables;
much of the necesarry data in the social sciences
are hidden from public view.

Except for the last, these limitations are due partly to the limited research funding of the social sciences. It is much more expensive to fund large-scale, complex and dynamic research. The problem here lies partly with the priorities of governments and corporate research funding groups: until social research is seen to be as important as that of particle physics, space exploration, or military research, we will have to expect most studies in our fields to be static, linear, and overly simplified.

However, if our research comes to be dynamic, nonlinear, and involve large numbers of variables, we face-of course-new methodological problems in how to deal with the complexity. We find ourselves in a situation similar to that of other fields of science an engineering in which the system under study is too complex to be described by a solvable set of equations. For example, fluid dynamics in such areas as meteorology and weather determination of the orbits of satellites or planets subject to small perturbations due to other bodies. The only way such systems are being studied with any chance of success is by way of large-scale, complex simulation models run on modern computers. The social sciences are way behind in this kind of research.

Another recent avenue being explored today in a number of fields is modern dynamical system theory, referred to by some as "Chaos theory" (A recent semi-popular history and review is the book by James Gleick, **Chaos:Marking A New Science.** N.Y., Viking, 1987). This is an exciting new approach to complex systems, but it is not clear at this point whether in will become more successful than the "catastrophe theory" of a decade ago. It studies the trajecto ries of complex systems whose behaviors appear at first glance to be chaotic or random, or which stabilize in periodic cycles, or evolve continuously in subtle ways. Careful empirical and mathematical studies seem to show, however, that is some structure and universal properties in such seemingly chaotic behavior, and thes has given hope that we can come to understand such systems better, and perhaps predict or control them to some extent. Some believe this approach to be relevant to the social sciences as well as the physical and biological. Though not entirely accpted by maistream science, there are some signs of wider recognition. For example, astronomers at MIT recentaly announced that their computer simulation of the orbit of the planet Pluto millions of years into the future shows that the solar system is absolutely stable.

Even if such new techniques as these are not directly applicable to much social science research, it is important to develop computer modelling and simulation techniques as holding the most promise of a more systemic or holistic, and thus realistic, approach to he study of society. But such modelling should not depend too much on attempts to quantify and express social behavior in terms of mathematical equations alone. For example, models of decision -making- which underlies much behavior and the various products of behavior (such as economic goods or investments), need to be developed on the basis of the "psycho-logic" underlying most actor' decision; not complex mathematical decision functions which assume rationality and goal - maximization, but a more "satisficing" approach: if conditions A and B occur, and not C or D, then probably action X should be taden when the first opportunity arises. This means that actors' hierarchies of goals and values need to be made explicit in the modelling, instead of continuing to rely on an out-moded simplistic assumption of maximizing "rational man". Such computer modelling might well exploit also some the research in artificial intelligence, although, again, using great care to avoid an overly 'logical' or rational model of even economic decision behavior. (A quich glance at typical decision behavior in the stock or commodity markets should dispel any illusions of pure "rationality.") It should also be understood that, if such a dynamic model is used for prediction (and assuming it to be a valid model), it can be used only for the short run and not for any relatively long term, since the system will surely change - in its structures, its parameters, or the normative and value base underlying decisions. It will no doubt be some time before we can incorporate such anticipated changes into our computer models - if ever.

II. At the metatheoretical level there are a number of serious problems that are only beginning to be addressed. These include: the failure of sociological research to be very cumulative, partly because we have not paid as much attention to the development of methods of theory construction and theory validation as we have to empirical research methodology; the so-called micro-macro gap, and the problem of identifying the main ontological focus of sociology at the group level; the supposedly overly 'positivistic' nature of sociology, (though the distinction between 'positivism' and science in general is often not clear); the continued use of inappropriate analogies; and sometimes, the intrusion of normative or ideological bias into our research. I can comment here on only some of these problems.

A strong critique of the non-cumulative nature of sociological knowledge has recently been made by Lee Freese (**Theoretical Methods in Sociology**, 1980, University of Pittsburgh Press). We have both a jumble or unrelated data and a hodgepodge of theories, and a central cause is the lack of shared standards for constructing and validating theories that match the level of care and precision with which we gather and analyze data. Our important concepts are often poorly defined and justified in terms of a larger theory, which in turn is very loosely expressed and articulated. Freese aregue that, first, there is little consensus on how rigorous a calculus or language is required to express a social theory. Though rigorous empirical reasearch methods are highly prized in the field, attempts to develop rigorous theory are not appreciated, but seen as turgid and lacking imagination. Many seem to prefer the language of poetry instead. Axiomatic theories are thusrequired so that we can at least see their logical propersties and relations.

Secondly, there is not much agreement on decision rules for accepting or rejecting theoretical claims. The rules for accepting observation statements need to be distinguished from the rules for accepting theories.

Third, granted what that we now appreciate the fact that, contrary to logical positivists, there is no completely clear line between observation statements ahd theoretical statements, yet there are some differences which we need to clarify. Most sociological knowledge at present is made up of a chronology of events at particular times. Hence it is mostly an empirical or historical endeavor. Science is inherently generalizing, and theoretical statements point to properties that cut across natural groupings, cultures, and historical periods. Knowledge cannot be accumulated on the basis of empirical generaliztions alone. If we can develop a cumulative systematic theory, it can substitute for the "meticulous and massive development of data." If a theory is good enough we do not need an endless collection of data to give it substantial support. We do not have to accept the view of Freese that "Facts need theories more than theories need facts" in order to appreciate his main argument. Although theory without facts is blind, it is also the case that there is no immaculate perception; data can seldom speak for themselves.

It becomes clear that this issue is a continuation of the centuries old struggle between Rationalism and Empiricism, which we thought we had integrated into a unified Science long ago. We still argue, however, with the legacy of a moribund logical positivism with its empiricist over-weighting. Now a reaction has set in, and many current theoretical orientations in social science have turned back to older philosophical speculation, such as hermeneutics or phenomenology, with rather sparse concern for systematics observation and empirical research. This will probably continue unless and until we clarify the kinds of metatheoretical issues being discussed here.

Much of social science has been hindered, I believe, by the too uncritical use of inappropriate analogies: for example, the analogy of society as like an organism, and society as like a marketplace for the (usually rational) exchange of valued social relationships. The first of these let to structural-functionalism and its problems of teleology, the assumed adaptiveness or benificence of persisting structures, and difficulty in dealing with social conflict and change. A better analogy would be that society is like a species - not an organism. As such, it accumulates structures or internal changes, at the time that new and competing or conflicting variations arise that may promote better, or worse, adaptation.

In the second case - the exchange analogy - one main problem, already addressed above, is the widely criticized assumption of "rational man." This is also, of course, a central assumption of neoclassical economic theory, which other social scientists are now beginning to try to correct. You are probably familiar with the recent formation of an international Socio-Economic group of social scientists devoted to correcting the simplistic or non-existent sociocultural base of "economic man" underlying economics and Neo-Utilitarianism. One of the leaders of this movement, the sociologist Amatai Etzioni, has just published a book, **The moral Dimension: Towards a New Economics**. As the title suggests, the book attempts to augment rational economic motives with normative and value motives, which often run counter to strictly "rational" motives of self-interest or efficiency. The concept of "rationality" itself has not been sufficiently explicated, and is often used to refer to some absolute or inherent characteristic of a process or behavior. However, a little thought suggests that the term is strictly relative to some goal or end-in-view. Thus a process or begavior can be said to be "rational" only in terms of some clearly specified goal, just as the cybernetic or control engineer points out that it is not possible to define a control or self-regulating system unless we specify the goal point toward which the system is regulated. Even the pioneering work of Max Weber on "rationality" is often vague in this respect.

Another meta-theoretical problem area being recognized more and more today is the so-called "micro-macro" gap: "the problem of developing theories and research that explain how just social structures help shape actors' behaviors, and vice versa. (See for example the recent book of Jeffry Alexander, et al., **The Micro-Macro Gap**.). In fact, we still do not seem to have come to a common undestanding of the ontological status of "society" or the "group" as an entity to study in its own right, and the central focus of the study of society. Some still take the "individual" alone as "real" and consequently argue for a reduction of social theory and research to "individualistics" concepts. Thus, the philosopher May Brodbeck argued some yeras ago for "methodological individualism" on the ground that only "individuals" and not "societiest" or "groups" can be directly observed. This, however, can be seen as an epistemological errer. We can directly observe, in fact, only the biological organism, and not the human "person", which is the main concern of most social (and psychological) science. And the human person - the product of a long process of group socialization - cannot, of course, be observed directly. Once again we must conclude that, until we have resolved some major meta-theoretical problems, we cannot be sure of our research methodologies let alone our theories of society.

Finally, in closing let me just mention the unfortunate tendency today for many scholars - who are perhaps disillusiond with the slow progress of social science - to turn back to older areas of speculative philosophy (e.g. phenomenology, hermeneutics, or some aspects of "critical theory"). Although a variety of orientations in social science is all to the good, many are turning in this direction as a substitute for science rather than an augumentation of it. The net result is often a callous disregard for any empirical grounding of their terminology and speculations.

I thank you very much for the invitation to speak to you. Although my talk has been mainly negative, it is intended to provide food for thought and to encourage constructive correction of problem areas in social science.

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