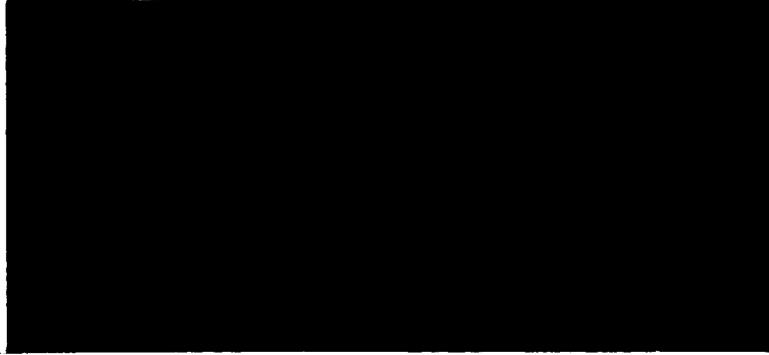# TRANSFORMATIONS
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SOCIOLOGY AS A DISCIPLINE:
QUASI-SCIENCE AND QUASI HUMANITIES

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Sociology as a Discipline:
Quasi-Science and Quasi-Humanities

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Every once in a while it is useful to take stock of where we
stand as a collective enterprise. Most of us go about our daily
business of teaching, writing, and research -- of professional
and civic participation, without reexamining our fundamental
premises-- the views of what we are doing and why we are doing it
that we work out early in our scholarly careers. But occasionally
we ought to take stock, to ask if our original conceptions were
correct, to reassess the options. If our original assessments
were wrong, or partially misguided, we ought to ask how they were
wrong and what steps we might take to change the directions of
research and writing.

In this paper I would like to sketch a view of the
discipline of sociology quite different than the one I was taught
in introductory sociology almost forty years ago, and that is
often still taught in undergraduate and graduate courses in
secular universities. This view, what might be called "the
becoming a science" view of the discipline, was there at the
beginning of sociology in the writings of Comte. In the United
States it intensified after World War II, as operationalism,
logical positivism, and quantitative techniques gave a
distinctive coloration to the science of society. The becoming a
science view continues to influence the world view of many of my

colleagues. It shapes how we teach, how we structure the graduate curriculum, what we reward in scholarship.

In my view "the becoming a science" view is partial and limiting. We are a quasi-science, but we are also quasi-humanistic.1 Because we have denied our ties to the humanities, we have missed many options and we have often mis-specified our intellectual problems. On the other hand, although we are quasi-humanistic, I hold little brief for those who believe that sociology ought to give up its connections to scientific method and explanation, who reject out-of-hand positivism, empirical evidence, and concern for the scope of generalization and the range of its application. There certainly is room for that point of view, and some scholars and departments may well pursue that vision whole heartedly. But I think it would be a mistake for the discipline as a whole to commit itself to a view of itself as solely humanistic. (More about that later).

The "becoming a science" view, especially as it crystallized around mid-century, entailed a set of subsidiary ideas about measurement, the role of theory, statisitics and mathematics, the nature of proof and disproof, and epistemological and ontological assumptions. The "becoming a science" view was in bondage not only to abstract conceptions of science, but to a particular science--physics. Thus, the image of how science was built and how it cumulates was heavily shaped by the architectonics of physics. I believe the unitary and hierachical model of science which we borrowed from physics has led many of us to seriously misunderstand the structural possibilities for

our own discipline. And, pollyanna that I am, it has led us to underestimate the enormous progress we have made in the many arenas of our expertise. If we are to understand ourselves as a quasi-scientific community, it is important that we have a realistic view of the variety of sciences, rather than hold to an inappropriate and unrealistic model for ourselves.

If we are to understand ourself as quasi-scientific quasi-humanistic, we also have to have a sense of how knowledge cumulates and changes in the several humanistic disiciplines. In some sense this is terra incognita, for both sociologists and philosophers have paid much more attention to the processes by which scholars warrant knowlege in the sciences than in the humanities.

Two prefatory comments are in order before I develop my theme: First, although I have read in the philosophy and sociology of science, I claim no expertise as a philosopher of knowledge or as a sociologist of science.2 The arguments presented here are a result of my reflections on the course of sociology during my life time; they are not based on a deep reading of epistemology or a reconsideration of the fundamental object-subject problem which leads some interpretative sociologists to question our status as a science. My own intellectual commitments have been to middle range theory development of an explanatory kind. I am not rejecting those commitments.

Second, although this paper is addressed to the status of sociology as a discipline, in varying degrees the comments could

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be applied to other social sciences, as well. They apply least to anthropology, much of which has been closer to the humanities all along. (Indeed, the problem for cultural anthropology may well be that it has lost its ties to generalizing science.) They apply to political science, especially of the behavioral and modeling persuasions. Unfortunately, political theorists dealing with classical themes until recently have felt too much on the defensive and too walled off from the behavioral mainstream to be of much use in challenging the intellectual limits of the mainstream. The comments apply to economics, although that benighted discipline is largely lost to a dialogue on these issues. Mainstream economics combines an unthinking commitment to Popperian methodology [see Blaug, 1980], a neo-classical model of marginal analysis, and a reification of markets that, taken together, seals it off from these debates. The reification of the market gives them a natural object and marginal analyis is their all encompassing theory. The comments also apply to the less biologically linked aspects of psychology. Since it is difficult enough to get my own colleagues to face the problem I want to address, and since I know less about the other disciplines, I will not comment in any detail on the application of my reflections to sister social sciences.

In the next section I discuss the making of sociology as a collective project. The becoming a science model was chosen not only because of its intellectual attractiveness, but because it also facilitated our legitmation and status achievement in the larger academy. The becoming a science model carried with it a

limited image of the scientific project. In the second section I briefly discuss alternative models of science and their implications for cumulation in sociology.

I then turn to a discussion of problem formulation in sociology. I want to argue that many of our problems come externally from politics and civilization as lived, not from an internal disciplinary concern with puzzles about fundamental and universal aspects of human life. If that is correct, our processes of problem selection and formulation have much in common with the processes and issues of historic concern in the humanities. The third section presents a discussion of sociology as a humanistic discipline, but, alas, a poor one. That we are partially scienctific and partially humanistic has implications not only for the nature of problem formulation and reformulation, but for methodology and the cumulation of knowledge as well. The paper concludes with a discussion of the programmatic implications of my alternative view.

My central thesis is that we are at best a quasi-science; that built into our subject matter are real limits to how scientific we can become. Moreover, because our core preocupations and many of our specific research interests come from civilization as lived, not from an unfolding view of universal and fundamental properties, we are constantly rejuvenated by the traditions and issues posed by our changing civilization. Not only are we quasi-humanistic, but we are a moral-political enterprise. For many sociologists, certainly not all, the motives for participating in the discipline come from

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commitments to political and social change. Life experiences, both inside and outside the discipline, change and channel those motivations.4

The Collective Mobility Project of Sociology

The creation of an academic discipline, much as the creation of a profession, can be thought of as a collective project. Students of the professions (see M. Sarfatti-Larson, 1977) use the concept of a collective project (or "collective mobility project")to focus upon the implicit goals of loosely coordinated people aimed at enhancing some collectivity to which they belong. The collective projects of disciplines have two major components--intellectual, the forms and kinds of knowledge and values the discipline wants to enhance, and occupational, the ideology, organization, and command of resources, including status, that the collective wants inorder to justify and enhance itself in the academic and larger community.

The two components, intellectual and occupational, should not be thought of as distinct, but as interlocked aspects of the overall collective project. That is, intellectual choices require different kinds and amounts of resources and a social organization of disciplinary resources that are dependent upon societal support.(To give a concrete example, contrast the needs of national and international sampling frames in public opinion research as contrasted with the resources needed and organization of ethnomethodological work.) The provision of societal support is related to the valuation of the products of intellectual

choice and affects the distribution of prestige and status within the profession and to the profession as a whole.

The intellectual project of sociology was to become a (the?) science of society. Our concept of a science had two major components, methodological, the development of objective indicators and standardized measurement of social phenomena, and theoretical, the development of nomothetic laws and theories stating universal relations among precisely defined concepts. I return to the intellectual component later.

How did the scientific project tie to an occupational ideology? The scientific project served to enhance our prestige and ability to gain resources by linking us to the prestige of the natural sciences. They had had great success in gaining entry to the academy, in gaining government support for research, in achieving prestige in the society. It was not always thus, of course, and each of the natural sciences has in its own time conducted a collective mobility project. Daniel Kevles (1978) has described the transformation of physics as a disciplinary project in almost these terms. If we could claim to be a science, with an ability to find strong laws and predict non-obvious relationships, surely society would recognize our value. Even the choice of the term "behavioral sciences," commonly used right after World War II, was part of the attempt to nestle closer to the images of a hard, objective, rigorous discipline. The looser and softer term "social sciences" was both fuzzy and less politically acceptable.

At the same time, becoming a science would separate sociology from its ties to social reform, social work, and the social gospel. Even though empirical sociology had flourished at Chicago partly by studying the under-class (e,g, The Ghetto, The Jackroller, The Hobo, Gold Coast and the Slum, The Polish Peasant), there was a difference between studying the underclass and trying to help it. As Everett Hughes delighted in pointing out, the status of professionals is partly determined by the social class of their clientele. Professionals are also known by the professional company they keep. We needed to dissociate ourselves from reformism and social work. If we were to become a science, we had to refrain from giving advice until we had adequate knowledge. Sociology could not be an applied discipline until a tested body of knowledge existed. It was acceptable for professional schools to give advice, for schools of business to train practitioners and consultants, but the most sociology should do is teach the discipline. It was within our mandate to describe organizational practice, or study the incidence of "nuts, sluts and perverts," as the study of social problems was sometimes called. But description didn't imply designing change.

Avoiding social action had another benefit: it made us acceptable to the administrators and boards of trustees who paid the bills. In Advocacy and Objectivity: A Crisis in the Professionalization of American Social Science, 1865-1905, Mary Furner (1975) shows how reformist economists and social scientists had to pull in their horns and how disciplines shifted the boundaries of the acceptable. By 1950 avoidance of social

action had become an occupational ideology. Of course, claims of objectivity and value neutrality also served to emulate the standards of science and helped to create an internal consensus about the tasks of the discipline.

The benefits of becoming a basic science and separating from social work and social reform (even though a concern with social conditions and social problems was part of most sociologists' motivation for seeking out sociology) were fairly explicit in discussions about sociology. What was less explicitly discussed was our relations to the humanities.

Academic disciplines, like professions, claim expertise over arenas of knowledge. As Andrew Abbott (1988) forcefully argues, there is a contest, implicit and explicit, for control of the arena of the production and application of knowledge. The social sciences grew out of the humanistic disciplines, especially philosophy, beginning to separate and become institutionalized after the Civil War in America. Of course, there was (and is) no one model that underlies the humanities disciplines, so a contrast of sociology with them is necessarily selective. By the time of the great depression, the contrast was quite sharp. The contrast was both intellectual and occupational. One thing the humanities disciplines did share was that they were not rising disciplines. Classics and philosophy, once rising disciplines in relation to the study of Christian theology, had been displaced as queens of the academy by the natural sciences and there was little to be gained by attempting to emulate them. Unlike Great Britain, for instance,

where the ramparts of civil service professionalism were dominated by those who had studied "the Greats" and science was still finding it hard to break into elite institutions, in the United States the humanities presented a pallid, dare I say effete, image. They represented the past, the conserving of past class and cultural dominance, not the new, progressing forces.

Intellectually, the humanities were rejected for a variety of reasons. Philosophy had normative overtones and was an armchair discipline. Only philosophy of science was of much use to social scientists. Classics was a descriptive account of past civilizations. Since we were to stand outside of our own cultural and civilizational biases, the axial concerns and alternatives of our own civilization were of little use to us. Literature was textual and aesthetic. There may be truths in a poem or novel, but they were not our kinds of truth. The truths of literature could not be stated in propositional and variable language. They provided individualized insights, not generalizations.

History, then as now, stood in a different relation to the social sciences. By the end of World War II social history had emerged and economic interpretations of political institutions were quite common. Still most social scientists rejected history as an ally or a model. Much of history used political biography as a major part of its lens on the world. The focus on individuals and the action of individual leaders as a means of reading social change was methodologically rejected by the generalizing social sciences. Historians read their empirical observations from residual archives, not from systematic samples

of populations. Most importantly, the focus on explaining
singular events, on describing and explaining periods in local
context, was quite opposed to the generalizing ethos of the
emerging social sciences. Although the grand theorists of
cyclical transformation, such as Spengler, Toynbee, and Mumford
were referred to in courses on social change, there was no middle
ground. Comparative historical sociology had few if any
exemplars. Some comparative sociology did exist, in the work of
students of revolution (Brinton, a historian, Petty, Edwards) and
in writings on the sociology of religion. But, by and large,
history as a discipline was in another world.

Playing the science card worked. As a collective mobility
project the social sciences in general and sociology in
particular moved from a peripheral position in the academy to, if
not a central position, at least a fairly well established one.
We became established in almost every university. Except for a
few holdouts, where as matters of convenience we are lodged with
anthropology, we were granted status as separate departments .
Student demand has been strong enough that in most universities
we are the fourth or fifth largest social science department--
behind history, economics, and psychology,-- sometimes larger
sometimes smaller than political science (depending upon the
class structure of the university; political science as a
training ground for lawyers is larger in elite universities),--
usually ahead of anthropology and geography. Government
agencies and foundations recognized the value of social science
in investigating basic and applied phenomena. The National

Science Foundation coded us in with other social sciences. Large amounts of money have been spent for the collection of data on de-politicized social concerns (e.f., the election process, demographic phenomena, social epidemiology and medical sociology). Where history and philosophy had to wait until the establishment of the National Endowment for the Humanities in the early 1970s to receive Federal funds for projects and fellowships, we began to receive Federal funding in the 1930s (for rural sociology), had a substantial growth in funding in the 1950s, and remain much better funded than the humanities today. .

Another measure of our success can be seen in the opening up of the elite positions of the academy to sociologists. It is no longer rare to find sociologists as Deans, Provosts and even Presidents of universities and colleges. And it is the elite as well as the non-elite colleges that are selecting sociologists as senior administrators. Sociologists such as Harrison White, William Sewell, Ronald Freedman, and Charles Tilly have been elected to the National Academy of Sciences. Sociologists such as Tilly, Converse, Bell, Merton, Shils, Bellah, Gusfield, and Smelser participate in the councils of the peak learned societies, such as the American Philosophical Society, the American Association for the Advancement of Science, the American Council of Learned Societies, and the American Institute of Arts and Letters.

Intellectually, the success of the collective project shows in the patterns of diffusion of ideas, methods and theories. A few years ago a survey of political scientists found that the AJS

and ASR were two of the most widely read journals in political science (in the top ten). Almost every profession now borrows sociological theory and methods for investigating their patterns of client usage, institutional structure, and service adequacy. Modern historical study is almost unthinkeable without a heavy infusion of sociological conceptualization and analysis, acknowledged or not. Indeed, the infusion of sociological and more recently anthropological concerns in history has created a sense of embattlement among practitioners of the older narrative and political styles. (Himmelfarb, 1987)

In spite of this success, we are not so well established that our intellectual accomplishments overwhelm the society at large, academic administrators or colleagues in other disciplines. When bad times come to universities, sociology looks somewhat expendable. (We are lucky to have geography around. When hard times come, if a university does not have a geography department to axe, it looks to sociology.) Research funders and reviewers sometime wonder what they are getting from the enterprise and we do not receive strong support from natural scientists when Congress is being lobbied. (Indeed, it often seems as if Congressmen have to convince the natural scientists to accept money for us.) Scholars in other disciplines feel little compunction about evaluating and commenting on sociological productions. A measure of turf domination might well be the extent to which members of other disciplines believe they can directly evaluate sociological work during tenure decisions, rather than relying upon expert opinion from within the

discipline. It is clear that sociologists don't evaluate mathematicians and that even art historians are more autonomous than we.

I want to argue that we are a different kind of science than we thought we could be, and by not articulating with a resurgent set of humanities disciplines, we are less of a scholarly enterprise than we ought to be.

Becoming a Science and the Cumulation of Knowledge

That we have become widely accepted in the academy is clear. That sociological concepts and methods are widely used both in and outside of academia is clear. But becoming a science implies more than using or aping scientific methods; it implies more than developing conceptual distinctions and categories that highlight the human landscape. Becoming a science implies some kind of criterion of progress, of evidence that error is eliminated and truth is approximated, that theories that make more sense of the facts will supplant weaker theories.3

When we reflect upon our status as a discipline many of us rejoice in the richness of our concepts and the vast number of topics that have been rendered amenable to systematic observation and analysis. Moreover, there are a large number of approaches and methods that may be applied to any given topic--historical, hermeneutic, case analysis, sample surveys, and on. However, it is this very profusion of topics, theoretical ideas, presuppositions and methodological approaches that leads us to be a "low consensus discipline" (See the debate between Lowell

Hargens and Stephen Cole, Gary Simon and Jonathan Cole, 1988, as well as references cited therein.)

That we have such a profusion of approaches and topics and that we are a low consensus discipline is sometimes taken as a sign that we are a proto-science, not quite there yet. We await the great systematizers (Parsons?) or simplifiers (Homans?) who will establish a common language and overarching frame for the analysis of social life. Of course, I cannot disprove that that is a possibility for the future. It may happen. But the history of the last forty years points in quite the opposite direction, the multiplication of specializations and approaches.

Not only are there more specializations but they do not relate in a clear architectonics. Current attempts to map micro-meso-macro linkages is an attempt to unify different levels of analysis, and is thus a move toward an integrated architectonic for the discipline. But looking for such linkages is by itself without theoretical content. If there are many different kinds of linkages and many different kinds of analyses at each level, a unified theory will not emerge.

Later I am going to argue that one source of the profusion of topics and approaches is based in our situation as a quasi-humanities: our problems come from civilization as lived, from our deep enmeshment in our own civilization's concerns and traditions. But here I want to argue that scientific progress can occur without unification and that the image of a science as a unified entity may well be inappropriate for sociology.

One approach to unification has been through global mathematical models. Although mathematical sociology is alive and well, modern modellers, by and large, take on small networks of relations. The global encompassing model of a Stuart Dodd has passed from the scene. Although Robert Hamblin's sociological application of S.S. Stephen's psychometric function has been quite ambitious, it has not been widely emulated or adopted.

Anthropologist Roy D'Andrade (in Fiske and Shweder, 1985) suggests one reason mathematical unification is unlikely. He argues that there are three major groups of sciences--what he labels physical science, natural science, and semiotic science. Physical sciences--physics, chemistry, parts of earth sciences and astronomy are characterized in terms of a set of mathematically described and interrelated set of concepts. Mathematicization was important both for solving internal problems and for creating an aura of wonder in the society at large. These disciplines resemble Hempel's covering law model of sciences, in which the phenomena under study are captured in universal statements that hold in all places and time. The natural sciences-- much of biology and the social sciences , are desribed as systems of interconnected mechanisms. D'Andrade notes, for instance, that molecular genetics has made enormous progress with hardly a mathematical equation. Models of systems are created and mechanisms for switching components are isolated. The models are more context dependent. "The description of DNA is thus, not the description of a law, but the description of a complex contingent mechanism." (p. 21) The texture of these kinds

of sciences is lumpy, made up of various components, each
described in static and dynamic terms. D'Andrade argues that
social systems can be described in these terms as well. The
analysis of capitalism can proceed as if it were a natural
system. But he also argues that in the case of much of social
science, our systems are constructed within a context of meaning.
(He notes that much of modern social psychology, which pretends
to be studying universal elements, is actually a kind of modern
ethnography, elucidating constrained and socially constructed
behavior within the contemporary world.) Understanding changes
in the context of meaning requires semiotic approaches. For
D'Andrade understanding social systems and social behavior
requires both natural science and semiotic approaches.
(D'Andrade's list of the semiotic sciences is stronger on the
hard ones, linquistics, and a little weak on the soft ones,
textual hermeneutics. He is silent on the relation of the
semiotic disciplines to the humanistic ones.)

If there was a master social system that could be analyzed
as a natural system, our work would be vastly simplified.
Something approaching a unified social science would be possible.
Strange bedfellows they may be, but world systems theory and
micro-economic analysis present themselves as explanatory
approaches to the master social system. But there have been many
natural social systems in that different societies have existed
but with little contact or continuity with each other. Moreover,
even the modern world is coupled, but not tightly coupled. And
there were in the past even more loosely coupled systems. The

components change. The loose coupling of the system is one source
of the profusion of topics and approaches in sociology--
institutional domains, e.g., religion, family, law, politics,
sciene, art, cannot be reduced to a single analytic model. (cf.
Friedland and Alford, 1987)

If one of the tasks of sociology as science is to describe
and explain the many loosely coupled components of the social
world, the profusion of sub-disciplines and topics should not be
surprising. However, that does not mean that progress does not
occur, but the progress is in accumulating knowledge within the
local contexts of sub-disciplines and invisible colleges.

Moreover, the texture of knowledge, the form of
generalization, and the linkage of empirical statement to
concept and theory will vary depending upon the problem set, the
methodological commitments, the style of data collection and the
form of conceptualization in each particular arena.
Ethnomethodologists have made progress in understanding how
intersubjectivity is achieved. They develop inference rules and
concepts from deep descriptions and analysis of small units of
interaction. Social demography has specified the components of
the income differentials between the races. The components are
described in econometric equations that yield quantitative
empirical generalizations. Both have made important advances in
knowledge. But the form of the realizations of these advances is
vastly different.

The implication of this argument is that sociology as a
science has made substantial progress. In almost every area of

sociology we can make statements based on evidence and theory that we could not have made thirty or forty years ago. We can say why statements made at the earlier time are wrong or partial.

At the same time, the way in which growth and problem shifts occur in sociology is bewildering. We multiply concepts and specializations and take up and drop problems. At first glance, we seem to switch problems fadishly. One source of the multiplication and change of problem should be seen as purely positive: sociologists use observational techniques and theory to illuminate areas of behavior previously reserved to other disciplines or not discussed at all. For example, in the last two decades the emotions, the production of culture, the mortality rates of organizational forms, and the routine grounds of everyday life have all lent themselves to sociological dissection. But part of the discontinuity in problem selection comes from the fact that we are a weak and unsophisticated humanistic discipline. I want to argue that in contrast to the physical and biological sciences, the social sciences, and especially sociology, are heavily dependent upon society as lived as a source of their problems. Willynilly we are caught in civilizational issues. Unfortunately, because we are a poor humanistic discipline, we lose sight of the roots of our problems and concerns.

Problem Selection in a Quasi-Science, Quasi-Humanities

The limits of sociology as a science stem not from methodology, nor from its particular architectionics. They stem from a misconception of the theoretical and substantive task of sociology; from a misconception of the sources of major substantive problems and how these relate to the structure of the discipline. In a nutshell, my argument is that the attempt to model our discipline on physics led us to search for universal laws; however, our real metier is the contextually qualified generalization about important social processes. (See Converse in Fiske and Schweder, 1985 for a parallel emphasis on contextually qualified generalizations.) Importance stems not only from the role of the process or concept in explaining social reality, which would be an internal, theoretical basis for establishing importance, but from the concern of scholars with civilizational values and outcomes.

As I have already argued, because physics is (was) the exemplar of the natural sciences, sociologists often modeled their view of what the discipline should be on their views of its structure. The search was for a few fundamental concepts or elements related to each other in law like generalizations, through fundamental forces or mechanisms. The concepts had to be very general. Systems theory, structural-functional theory, conflict theory, and role theory were some of the more general and abstract formulations proposed as candidates. Or we have searched for the most elementary forms of the linkage between behavioral conditioning and status. Unfortunately, in almost

every case the pursuit of theoretical elaboration at the most
general level resulted in vacuous, banal, and boring
generalizations, often contradicted by other generalizations.
Each of these very general theoretical frameworks might lead to
illuminating insights and rich data sets when applied by a
social scientist with a feel for a particular historical- social
context. Or, because of detailed elaboration, some facet of
elementary behavior or of system functioning would come into
view. Nevertheless, when pushed too far toward generality, the
result often has seemed vacuous. Moreover, the application of
these theoretical perspectives to particular cases has depended
upon sensitivity, nuance and craft, not rigorous logical
deduction.

If a science is to be universalizing it needs elements that
reasonably apply in all societies. While such concepts as
"action" or "status" or "role" or "reward" or "differentiation"
may apply universally, they take on meaning only when filled with
cultural and civilizational content.

There is another way to make the point. Assume that my
argument about the nature of cumulation in a low consensus
discipline is correct. If we were only a science, problems and
solutions to problems would occur largely within the community of
scholars. New problems would emerge largely in response to the
progress and anomalies found in the debate within sub-disciplines
and invisible colleges. Or new problems and methods would emerge
at the intersection of sub-disciplines and invisble colleges, as
progress and change in one cross-fertilized others. I take it to

be the case that problem formulation, problem solution, and problem succession are largely internal matters in the physical and biological sciences. We consider societal intrusion into theory choice and problem choice an aberation in the natural sciences, even as we recognize that applied concerns shape the agenda of the natural sciences and as we recognize ethical limits to the conduct of experiments. Although the applied and civilizational embeddedness of physics, astronomy, and biology shape work to some extent, the basic elements of these disciplines and the core problems evolve with little political, moral and civilizational resonance.

Some topics of sociology and the social sciences may evolve in response to purely internal puzzle and problem solution. Formal interactionism and structuralism, material culture evolutionism and areas of socio-linguistics and micro behavior in sociology may have some of this autonomous and internal quality. But more often than not, the choice of problems, the definition of what is important, even the terms of analysis are freighted with political, moral and civilizational overtones. Although the scholar attempts to distance herself from the more purely ideological and self serving definitions of the phenomena, the problems take on meaning precisely because they resonate with this larger social context. Indeed, when the work becomes too isolated from the concerns of the larger society and its civilizational context it begins to resemble a kind of technicism--problems without purpose.

An example from an area that borders on my own work in organizations may be useful. In the mid 1960s there developed a great interest in the formal structure of complex organizations. Originally stemming from civilizational concerns about the growth of the administrative state and the increasing dominance of managers and administrators, a number of sociologists and social scientists began to systematically investigate the interrelation of organizational complexity, role differentiation, size, rules, delegation, and levels of authority. Early authors would be Berle and Means and Burnham, followed by Bendix, and in a more quantitative vein, Anderson and Warkov. This work was given great impetus by the group around Peter Blau in the United States and the so-called Aston group (Derek Pugh, David Hickson, and others) in Great Britain. Key articles were published in leading journals, and a flood tide of studies followed, including replications in several other countries. Finally, the whole enterprise ran out of gas. Later articles were published in the less prestigious journals. And today, in 1988, the topic has almost disappeared from the agenda. Although textbooks still summarize the studies, they are rarely cited in scholarly journals.

In one sense, this example illustrates the processes of normal science, as research findings accumulate and the research terrain is exhausted. But I think the more important lesson to be drawn is that in the process of developing statistical indicators, scholars lost sight of the relation of their specific empirical studies to the larger issues of authority and control.

Nowhere does the difference between sociology and the physical sciences show more clearly than in the way that major paradigmatic shifts occur. Although the claim can easily be overstated, shifts in the natural sciences occur because of events internal to the field of study. Theories don't predict, new measuring instruments reveal data not easily accounted for in the accepted paradigm, new formulations explain unaccounted for observations. Paradigm shifts in sociology may occur for these reasons, but the new paradigm is often a restatement, an intensification of older answers. For example, modern institutional theory, so ably developed by John Meyer, is an intensification and restatement of Weber's project on rationalization. Moreover, the press for reformulation may occur because of moral and political currents in the larger society; because events in the larger society and the moral and political evaluation of them lead one to reflect on the adequacy of current formulations.4 In both cases the reformulation reflects processes found in the humanities, more than it does those found in the sciences.

A clear example is the ascendancy of world systems theory and dependency theory and the decline of convergence theory, a branch of modernization theory. Would anyone really argue that convergence theory was <u>disproved</u>? What we would be more likely to argue is that it miscast questions, or evaded critical issues. It led to one set of foci, rather than another set. And at least one part of the rejection of convergence theory came from moral and political concerns. Liberals wanted to emphasize the

importance of civil liberties even though they recognized that
totalitarian regimes have some dependence upon the passive
consent of the governed. And radicals rejected convergence
theory not because it was wrong, but because it ignored class and
power in preserving industrial capitalism. On the other hand,
world systems theory developed out of a concern for the interplay
of center and periphery in the modern world. It combined the
moral fervor of Lenin with the long view of Braudel. It then
played back into the historical-sociological analysis of western
capitalism.

Not only do our focal concerns get shaped by the moral and
politial currents of the larger society, but our answers (as
noted above) are framed by major traditions of thought. No
physicist rereads Newton or Einstein when searching for a
solution to current problems. The laws or principles they
developed are encapsulated in current formulations. If a current
puzzle leads to deadends, they try to reformulate the puzzle. For
sociologists, on the other hand, the classics represent major
secular statements about civilizational issues.

What did Weber or Marx really mean? Was the young Marx more
important than the older Marx? In what ways is Weber superior to
Durkheim? These are questions in the tradition of reading sacred
books. In this case they are secular sacred books. They
represent for sociology major and exemplary answers to enduring
issues of social structure and social change. They are to
Sociology what Shakespeare is to English literature and Plato is

to philosophy--exemplar treatments within the canon of enduring themes.5

The answers they provide also have moral and political overtones, they are not just alternative conceptual formulations. It would be possible for a conservative to use a Marxian analysis of capitalism, but reject the political implications. Yet the social implications of using such an analysis leads the conservative to disguise the origins of his analysis.

The reliance on classic treatments is quite apparent in choice of paradigms for macro-issues in sociology. But the classics also play the same role in other areas. G. H. Mead provides one set of answers to the enduring question of the relation between self and society. Peirce is important for providing critical guidance in thinking about symbols, meaning and society. Those questions are not important in every civilization.

In a sense there are lodestone classics for each of the enduring civilizational themes. Returning to the classic themes and the classic answers occurs as our common language usage shifts and as we attempt to refine and rethink our orientation in specific problem areas. As the focal concerns of scholars shift in the context of the larger society, different historic usages and analyses come to the fore. That resembles the procedures in the humanities more than it does those of the sciences.

Sociology as a Humanistic Discipline

To say that sociology is partly humanistic, drawing upon civilizational values and traditional modes of interpreting and

understanding them, does not mean that we are a very _good_ humanities discipline. We are not very self conscious in developing that aspect of our intellectual life. We are quite self conscious in organizing the scientific side of our enterprise. Issues of research design, of theory construction, of modes of analysis, are treated in most graduate programs, in journal articles and in textbooks. Moreover, even the exemplary _current_ humanistic sociologists are more interested in justifying an interpretive, qualitative mode than they are in exploring the implications of their humanistic orientation for the organization of the discipline. They rarely take the humanistic disciplines seriously as models for methods or for the organization of knowledge. Unfortunately, some humanistic sociologists are more interested in sociology as moral suasion and social criticism than they are in the organization and cumulation of knowledge. Social criticism and interpretation is a useful function. But social criticism and interpretation without explicit comparison and concern for generalization leads us towards high level journalism--a kind of idiographic sociology-- or towards romanticism. (See the debate between Norman Denzin and Randall Collins, 1987)

The fact that we are a weak humanistic discipline has several implications. Many of our theories and major pieces of research are sharply delimited by cultural biases that we are ill-equipped to recognize or deal with. Thus, concepts such as stratification or power are treated as universal properties in blissful ignorance of, for example, work by Fallers on African

stratification or Geertz on power among the Balinese that present
a deep challenge to our hasty conceptualizations. We are
tediously ahistorical. In my own major area, the theory of
organizations, we write as if a timeless architectonics holds. We
detach organizations from the surrounding socio-economic system.
Poor Weber! We invoke him but ignore his assumptions. Finally,
as a weak humanities, we ignore our linkages to fundamental
philosophical issues and debates. We talk about normative orders
without examining the history of ethics. We develop theories and
research about distributive justice, equity, and equality with
little attention to the long philosophical debates and
interpretations of the terms. We develop a cognitive sociology
without attending to historic debates about perception, signs,
symbols, and epistemology.

It is true that many philosophers, literary critics and
classicists ignore the results of sociology, but that is their
problem. It leads them to making distinctions without a
difference, to deal with the extremes of moral issues, to ignore
the psychological bases of audience response to literature
without attention to the empirical range of real life
situations.

How different our courses would look, our alliances and
inter-disciplinary contacts would be if we took these issues
seriously. A few sociologists have of course bucked the trends.
Phenomenologists have studied epistemology and have created a
small bridge to linguistic philosophy. Some sociologists, such as
James Coleman (1974, 1986) have recognized the absolute

centrality of philosophy in both clarifying and chosing
fundamental assumptions and in thinking about social policy.
Philip Selznick (1961) has written persuasively on the role of
the normative order for both the study of justice institutions
and, with Gertrude Jaeger (Jaeger and Selznick, 1964) for the
study of high culture. Joseph Gusfield (1981) with his roots in
symbolic interactionism and the literary-dramatistic methods of
Kenneth Burke, pushes hard to reveal the moral drama and
rhetorical styles involved in public policy formation. But these
social scientists who bridge to the humanistic disciplines and
tradition are not widely imitated.

Conclusion: Programmatic Implications of Being A Quasi-Humanistic Discipline

The becoming a science model served us well as a collective
intellectual and mobility project. In recent times, however, we
have seen it challenged. Some parts of that challenge have been
misconceived, since we have held ourselves to an inappropriate
scientific model, that of physics. We have been more successful
as a science than we give ourselves credit for. Still, there are
real limits to the sociological enterprise. We share many
concerns with the humanistic disciplines. However, I believe it
would be an intellectual and collective mobility mistake to
abandon our concern for explanation, empirical evidence, and
scope of generalization. Without these concerns we have little
marginal advantage over social critics or social philosophers.

What would we do if we wanted to realize our potential both as a quasi-scientific and quasi humanistic discipline? One possibility would be for some departments to specialize in humanistic sociology. But, although it is a viable alternative for some departments, it misses the point for the discipline as a whole. If I am correct in my diagnosis, the problem is not that we need more sociologists who are appropriately trained in humanistic disciplines. The problem is how to maintain our interest in explanation, in systematic evidence and scope of generalization, at the same time enriching our conceptualization, nesting our analysis in deeper historical and cultural understanding, exploring less common-language definitions of key concepts and social processes. It is not enough to say "be reflexive"--"be widely read."

Any program must take into account the limited time available in doctoral programs, since students are often overburdened with requirments already. Several kinds of programs are possible, at the doctoral and post-doctoral training level, and in research enterprises.

At the doctoral level one route might be for sociology to reconnect to anthropology. Since anthropology has covered some of the ground that I have been talking about, greater liasion of sociologists with anthropologists might be salutary. (I believe that anthropology has much to gain from such a liasion, as well, but I won't detail that now.)

Secondly, doctoral students working on particular substantive problems should be encouraged to explore relevant

courses in the humanities . Cognitive sociologists and
phenomenologists could learn much from epistemology.
Criminologists and sociologists of law might gain from courses in
ethics and jurisprudence. Students in the sociology of the arts
might be counseled to explore aesthetic theory. By appropriate
changes in cognate and minor requirements, by judicious joint
appointments, and by scrutiny of methods requirements, it might
well be possible for even the mainline major postivist
departments to better connect to humanistic studies.

Post-doctoral programs might well be built around the
intersection of sociology and the humanities. Two kinds of
programs could be developed-- problem specific and disciplinary
specific. A problem specific post-doctoral program would focus
upon a specific concept or theoretical intersection--justice,
equality and inequality, language and society. A discipline
specific program would explore the intersect of sociology with
specific humanities disciplines--sociology and literature,
sociology and philosophy, etc. These programs might be short-
course programs--summer institutes, or individually tailored
programs of one or two years at selected institutions.

Finally, ways must be found to encourage senior scholars to
explore collaborative research, to expand their intellectual
horizons. Some senior sociologists such as Coleman, Joseph
Gusfield, and Stephen Lukes already feel at ease in the land of
the humanities. But more must be encouraged to cross the divide.
Support for collaborative seminars, for conferences on specific

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topics, and for interdisciplinary courses might be a good
beginning.

Would we find collaborators in the various humanities
disciplines? There has been enormous ferment and change in the
several humanities. It is clear that the barriers between history
and sociology have been crumbling. Classicists have turned to
sociology (a sociologist, Keith Hopkins, holds the Regius Chair
of Classics at Cambridge); the challenge to the canon in English
literature opens up the field to analysis of social structure and
to the production of culture and audience relations; philosophers
worry about the death of philosophy (Baynes, Bohman and McCarthy,
1986) and some use sociology at the core of their analysis.
(MacIntire, 1984) We should not expect a wholesale rapprochment
with those fields currently distant, but a selective one is
clearly feasible. We have much to learn from them, but they have
much to learn from us as well.

Of course, it may be objected, sociology is a mansion with
many rooms; younger sociologists are already pursuing this
agenda. Certainly it is true that scholars such as Wendy Griswold
(1987), with her training and interests in literature and
sociology, and Guillermina Jasso (1980), with her background and
training in philosophy and mathematics, begin to approach the
intersect. My own perception is that many younger sociologists
are pursuing critical and interpretive sociology, but at the
expense of empirical and explanatory approaches. It is my faith
that much is to be gained by interlocking our evidential and
explanatory concerns with normative, interpretative and

hermeneutic analyses; that a creative tension develops by joining

our scientific and humanistic aspirations and concerns.

Footnotes

1. The disciplines we now label as humanities grew out of the humanistic movement of the Renaissance. It celebrated mankind and the achievements of civilization. These disciplines, the study of language, literature, philosophy and the arts had an interpretative and moral core.

In contrast with the social sciences they have little commitment to replicable knowledge and the empirical testing of alternative explanations. On the other hand, more than the social sciences, they have been concerned with the history and intellectual roots of alternative patterns of thought and creative production.

2. Some of the readings that have been most helpful in thinking about the issues raised in this paper are to be found in Gutting, editor, (1980) Fiske and Shweder (1985), and in Churchland and Hooker, (1985).

3. A part of the debate over Kuhn's notions involve assertions he made about the incommensurability of theories and paradigms, which leads to a relativistic, non-progressive view of science. See Shapere, Stegmuller, and Blaug in Gutting, (1980).

4. An earlier version of this paper included a separate section on the moral and political recruitment base of the discipline. For sake of clarity in an already overburdened argument I have left it out.

5. In a personal communication Phil Converse argues that we have an advantage over the physical sciences in that as ongoing participants in a changing world, we can reexamine our theories

as events bring them in to question. There are many revolutions that can be used to challenge our theories of political revolutions, but only few discoveries of super-nova that challenge astro-physical and cosmological theories. I would add, however, that what we take to be important events and our root interpretations of those events emerge from our own civilization, not from a detached theory.

6. See Arthur Stinchcombe's discussion (1982) on the role of the classics in the education of sociolgists.

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