Of Time and Space: The Contemporary Relevance of the Chicago School*

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Abstract

This essay argues that sociology's major current problems are intellectual. It traces these problems to the exhaustion of the current "variables paradigm" and considers the Chicago School's "contextualist paradigm" as an alternative. Examples of new methodologies founded on contextual thinking are considered.

Anniversaries are often valedictions. A centennial sometimes shows an association to be moribund, just as a diamond jubilee may reveal a queen's irrelevance and a golden anniversary finds many a marriage dead. By contrast, living social relations celebrate themselves daily. Anniversaries merely punctuate their excitement.

What then are we to make of this centennial year of sociology at the University of Chicago? Is it simply a time for eulogy? After all, Chicago dominance of sociology is half a century gone. And while the Chicago tradition renewed itself after the war in Goffman, Becker, Janowitz, and their like, many of Chicago's most distinguished alumni since its dominant years belong more to the mainstream than to the Chicago tradition proper: methodologists like Stouffer and Duncan, demographers like Hauser and Keyfitz, macrosociologists like Bendix and Wilensky. Nonetheless, at the heart of the Chicago tradition lie insights central to the advancement of contemporary sociology. Therefore, I do not today eulogize the Chicago tradition. One eulogizes only the dead.1

* This article sparked a lot of commentary. Surprisingly, helpful comments came not only from people I knew well, but also from relative strangers. I have therefore had more help with this article than with virtually anything else I have written. The following all contributed substantial comments: Rebecca Adams, Joan Aldous, Margo Anderson, James Coleman, Claude Fischer, Jeffrey Goldfarb, David Maines, Donald Levine, Douglas Mitchell, John Modell, John Padgett, Moishe Postone, and Charles Tilly. I would like to dedicate this essay to the memory of Morris Janowitz, who taught me and many others about the Chicago School. Address correspondence to Andrew Abbott, Department of Sociology, 1126 East 59th St., University of Chicago, Chicago, IL 60637.

Sociology’s Predicament

Obviously if I think the Chicago School has an answer, I must also think there is a question. Or at least a predicament. Sociology’s glaring problems — the closing of the Washington University department and the recent close call at Yale — are perhaps less important than the more subtle ones, on which I would like to spend a few minutes. One of these subtle problems is sociology’s failure to consistently attract graduate students of abilities comparable to those attracted by anthropology, political science, and economics. However much we may doubt standardized testing and GPAs, the differences are too great and too consistent to be ignored (D’Antonio 1992; Huber 1992). Another indicator is sociology’s replacement as a policy advisor to governments, a role that has been almost completely assumed by economics. We may belittle economists within the security of our meetings, but they alone have the ear of the prince (e.g., Rhoads 1978).

Yet another depressing indicator is our fission into disconnected segments. The American Sociological Association’s annual meeting assembles groups that share little in intellectual style, methodological practices, or substantive concerns (See Ad Hoc Committee 1989). To be sure this is in some ways a sign of vitality; we are open about accepting certain kinds of differences. But our little factions show that, in fact, most of us are quite reluctant to accept new ideas; those who have them are cordially invited to pitch their tents elsewhere, sometimes within the ASA, sometimes outside it. Thus, the prospect of rational choice theory entering sociology sends historical sociologists to the barricades. Postmodernism causes the same reaction elsewhere. Even the fortunes of feminism show the same segmentary complacency. In anthropology, history, and political science battles rage over feminist theory. But sociology has absorbed the largest concentration of women in the social sciences without the slightest alteration of its intellectual structure and style in response to feminist ideas.2

Perhaps most depressing, sociology has lost much of its excitement. We still attract undergraduates with books like Elijah Anderson’s A Place on the Corner (1978) or David Halle’s American Working Man (1984). But we read such books only to teach them and certainly discourage our students from writing them. We are too busy being scientific. Yet even our science has a tired feeling. We subscribe to journals but don’t read them. Competition for space so regiments our methods and styles that even authors themselves sometimes seem bored by their material; they simply go through the proper motions of asterisked coefficients, low $R^2$, and suitably judicious claims of theoretical advance. Who today would publish Dudley Duncan’s analysis of synthetic cohorts in The American Occupational Structure, an analysis he admits with a large rhetorical wink to be so much fiddling around? (Blau & Duncan 1967) What mainstream journal today would publish Erving Goffman’s (1956) theory of embarrassment or Egon Bittner’s (1967) observations of police on skid row, or Talcott Parsons’s polysyllabic exegeses of American life (e.g., Parsons 1939)?
Perhaps, as theorists often tell us, things are better in their camp. Theory and methods, after all, have very little to do with each other in the discipline today. Studies of joint ASA section membership show very clearly the isolation of the theory section from the empirical mainstream sections, and reference lists witness the loud silence that greets each in the land of the other. But the book-based literature of the theorists hardly improves upon the article-based literature of empiricism. It simply affects profundiosity rather than positivism, judging its authors rather by the righteousness of their philosophical assumptions than by the currency of their statistics. And certainly one cannot accuse the theorists of relevance to the empirical mainstream, indeed to empirical reality at all. Empirical reality seems beneath their notice unless it involves the whole history of nations or civilizations.

In short, sociology has degenerated into formulas — empirical, theoretical, historical. We are no longer excited enough to take risks, to float unorthodox ideas, to poach on each others’ turf. We have given up writing about the real world, hiding in stylized worlds of survey variables, historical forces, and theoretical abstractions. How many of us in this room, I wonder, can claim to have spent since college even one full year in some social situation that is not academic?

I do not decry sociology’s exhaustion because of my own aging. Forty-three is a little early to be moaning that “things aren’t what they used to be.” Furthermore, it is among people of my own age that I detect the sense of exhaustion, the going through of intellectual motions. Unlike most of my colleagues, however, I believe that sociology’s problem is first and foremost intellectual. The external political threats, the difficulties attracting students, the fragmentation, these all reflect a weakness that is fundamentally a weakness of ideas. It has been a long time since we sociologists saw an idea that got us really excited, an idea that could transform our intellectual practice, an idea that could make us actually want to read the journals. I think that the Chicago School stands for precisely such an idea: that Thomas, Park, Burgess, and their students had a theoretical insight that leads out of our current difficulties. I wish to spend the remainder of my time this morning reminding you of that idea, considering its relation to current theoretical and methodological practices, and sketching the first efforts of scholars to renew it.

THE CHICAGO INSIGHT

It has for some time been unfashionable to speak of the Chicago School as having had any theoretical ideas at all. The introductory textbooks portray an empiricist Chicago that never scaled the theoretical heights with Marx, Weber, and Durkheim and that never escaped from melioristic social welfarism. At best, it is felt, the Chicago School began the practice of large-scale research in sociology and contributed fundamental empirical work in areas like community study and criminology. (See, e.g. the portrayal of Chicago in Ritzer 1988 or even in more friendly works like Bulmer 1984).
But Chicago seems atheoretical only because we, most of us, are sworn vassals of the paradigm that displaced Chicago's approach, what I shall here call the "variables" paradigm. Within that paradigm, and within its interpretation of the European classics, to be theoretical is to make assertions about the relation of abstractions like "gender," "capitalism," "education," and "bureaucracy." I am sure that most of you think that such assertions are in fact the essence of serious sociology, whether you are historical sociologists or status attainers or sociologists of gender. Within such a worldview, the Chicago School — which never believed in abstractions like "gender" and "bureaucracy" — is by definition atheoretical. But perhaps sociology's current difficulties will lead you to suspend judgment until I have made my argument about Chicago's theoretical position.

In a single sentence, the Chicago school thought — and thinks — that one cannot understand social life without understanding the arrangements of particular social actors in particular social times and places. Another way of stating this is to say that Chicago felt that no social fact makes any sense abstracted from its context in social (and often geographic) space and social time. Social facts are located. This means a focus on social relations and spatial ecology in synchronic analysis, as it means a similar focus on process in diachronic analysis. Every social fact is situated, surrounded by other contextual facts and brought into being by a process relating it to past contexts.5

An immediate corollary is that not only do variables not exist in reality, they are misleading even as a nominalist convention. For the idea of a variable is the idea of a scale that has the same causal meaning whatever its context: the idea, for example, that "education" can have "an effect" on "occupation" irrespective of the other qualities of an individual, whether those qualities be other past experiences, other personal characteristics, or friends, acquaintances, and connections. Within variable-based thinking, one allows for a few "interactions" to modify this single causal meaning contextually, but the fundamental image of variables' independence is enshrined in the phrase "net of other variables" and in the aim to discover this net effect, whether through experimental or statistical manipulation. The Chicago view was that the concept of net effect was social scientific nonsense. Nothing that ever occurs in the social world occurs "net of other variables." All social facts are located in contexts. So why bother to pretend that they are not?6

To say that this view has few current adherents is to understate. Most contemporary sociology does not take the location or relationships of a social fact as central. Time appears, of course, but merely as the tick of a clock. People and events are not located in it, variables are. Plots and processes do not run through it, causal arrows do. The same holds, even more strongly, for space. Most sociological articles presume unrelated individuals, whether workers, firms, or associations. These individuals are "units of analysis," not actors in social relations. Yet throughout the Chicago writings we find diagrams of typical histories of cases — Thrasher's (1927:70) diagram of gang careers, for example — and we find map after map after map, dotted with brothels,
schizophrenics, residential hotels, businesses or whatever else was of interest. Throughout the Chicago writings, we find time and place.

Of course, there are sociologists, some descended from Chicago, some not, who take location in social time and space seriously. The historical sociologists, although rather causalist in their methodological writing, in practice take temporal location quite seriously. Sociologists of occupations still retain Everett Hughes's Chicago emphasis on temporal process, as students of social movements retain Robert Park's. And of course the microsociologies descended from or confronted by symbolic interactionism — old Chicago's most specific lineal descendant — retain a strong temporal emphasis as well.\(^7\)

As for spatial location, it is the students of communities and networks who have kept that interest alive. To the former it came directly from Chicago, for community was one of Chicago's central concerns. The network theorists, by contrast, derive their interest in location from formal roots. The major approaches to network analysis are associated with mathematical sociologists: clique analysis with James Coleman and structural equivalence with Harrison White. Although both have been faculty members at Chicago, their network ideas owe little to that association.\(^8\)

But in the main, the idea that social facts are located facts, facts situated in social time and place, is a strange one in contemporary sociology. Yet the Chicago work is as readable today — by you or me or by undergraduates — as it was in the 1930s. And throughout it we find an unrelenting emphasis on the location of social facts in contexts of time and space, an emphasis that we have forgotten. Roland Barthes once said, "It is precisely because I forget that I read." (Barthes 1974:10). I would like, then, to reread some of the Chicago conceptualizations of temporal and social contexts. For some of you my discussion will simply remind you of things you may not have thought of for a while. For others — all too many I am afraid — it may be your first acquaintance with sociology's first period of disciplinary greatness.

The Chicago School

The various historians of the Chicago School — peculiarly enough they are often Europeans who have recognized the School's importance before we did — delineate it in various ways. But fundamentally, the Chicago School is a body of work produced by students and faculty of the department between the First World War and the mid 1930s. The School's publications began with Thomas and Znaniecki's The Polish Peasant in Europe and America in 1918. They ended with a wave of dissertation books published in the early 1930s. The active departmental faculty of this period were Albion Small, Robert E. Park, Ernest W. Burgess, and Ellsworth Faris. Chicago work of this period also shows the clear influence of W. I. Thomas (on the faculty from 1894 until he was fired in a celebrated morals case in 1918) and George Herbert Mead of the Chicago philosophy department. I should also mention the dominant faculty of
Chicago's succeeding period: William F. Ogburn, who arrived from Columbia in 1927, and several 1920s graduate students who became central faculty members in the 1930s — Louis Wirth, Everett Hughes, Herbert Blumer, and Samuel Stouffer.9

But at the core of the Chicago School were the dissertations and monographs, usually produced under Park or Burgess, on social structures and processes in Chicago and beyond: Nels Anderson (1923) on the hobo, Paul Cressey (1932) on taxi-dance halls, Harvey Zorbaugh (1929) on the Near North Side, Edwin Thrasher (1927) on gangs, Ruth Shonle Cavan (1928) on suicide, Ernest Hiller (1928) on strikes, Lyford Edwards (1927) on revolutions, Walter Reckless on brothels, Louis Wirth (1928) on the Jewish ghetto, Clifford Shaw (1930 [1966]) on delinquency, E. Franklin Frazier (1932), Ernest Mowrer (1927) and others on the family. It is a long list indeed.

Now these Chicago writers did not simply argue that social facts have contexts in social time and space and leave it at that. They distinguished what one might call degrees of contextuality.10 Think, first, about temporal processes. One fundamental concept of the School was that of 'natural history.' A natural history was a temporal pattern that followed a relatively predictable course. It could be diverted or shaped by environing facts, but its general sequence could be understood as a whole, beyond the contingent details. The clearest example of this concept among the classic works was Edwards's (1927) analysis of revolutions, a work that Theda Skocpol, in a book that won the Sorokin Award only a decade ago, thought it necessary to treat with grudging respect (Skocpol 1979:37-38). For Edwards, revolutions unfolded according to an internal logic. They might be diverted or reshaped. They might fail. But the general logic was regular.

By contrast, Thrasher's (1927) analysis saw developing gangs as considerably more open to contextual influence. The availability of resources, the force of competitors, the physical structure of the environment, all these could shape the career of a gang, a contextual shaping that Thrasher clearly thought more strong than what Edwards saw in revolutions. We might call temporal processes with this greater contextual dependence "careers," to distinguish them from natural histories.

Finally, in a broad range of Chicago work, contextuality was so important that one could no longer focus on a single process. Instead one must study a whole network of intertwined processes. The most celebrated example of this was Zorbaugh's The Gold Coast and the Slum (1929). At first glance — I remember this was my reaction as a graduate student — the book seems like a well-written but somewhat aimless history of Chicago's Near North Side. But in hindsight one can see that the book was the clearest expression of the Chicago School's concept of an "interactional field." The Near North Side was defined by its contextualities; for Zorbaugh, that is, the community's boundaries were the boundaries of the mutual constraints that defined what went on there — some of them geographic constraints, some of them social, some of them economic. The motion of every group within such a community was so dependent on
that of the others that there was no point writing about any one of them alone. One could only write about an interactional field as a whole.

In temporal processes, then, the Chicago writers saw three degrees of contextuality: natural histories with relatively little contextuality, careers with considerably more, and interactional fields with so much that individual processes were braided inextricably. Note that all of these concepts defined temporal processes. That is, there was no possibility of taking these social facts out of their temporal contexts. What was at issue was rather, given a temporal process, how independent it was of social contexts.

One can ask precisely the inverse question. Given a set of spatial or social structures, how independent could they be of temporal context? Here the Chicagoans made similar distinctions. The equivalent to the natural history was the natural area. In defining natural areas, Park spoke of the conversion of “a mere geographical expression into a neighborhood, that is to say, a locality with sentiments, traditions, and a history of its own... the life of every locality moves on with a certain momentum of its own, more or less independent of the larger circle of life and interests about it” (Park 1925:6). Here, as in the natural history, is an entity relatively independent of its surround, but defining everything within it in terms of location. Thus, in Anderson’s The Hobo (1923:15), there is a little map of the hobo “main stem” on West Madison Street. The social dynamics of the street itself and indeed of the local hobo community are bound up in the arrangement of the stores along the street.

As the example of Anderson makes clear, there was actually a deep implicit relation between the natural area and the enviroring city. Hobohemia was created by a certain economic demand (for migrant occasional labor) and by forces of transportation, economy, and social structure within the city. But these forces were constant in the short run, and so it made sense to see Hobohemia as a consistent natural area, whose relation to the environment, although important, was not contingent but temporarily fixed. Thus, the natural areas were social structures, contextually socially determined, but temporarily free of a need for analysis of their temporal context.

Writ large, this kind of thinking led to the ecological analysis of social problems in terms of “disorganization.” Various natural areas had various degrees of organization and disorganization; social problems were believed to be directly associated with the latter. This was to be a fundamental strand of Chicago work, on mental illness (Faris & Dunham 1939), on divorce (Mowrer 1927), and above all in crime and delinquency research (Shaw & McKay 1942).11

It is of some historical importance that the natural-areas model was relatively conformable with variable-based techniques. As the Chicago School declined, location in a natural area simply became another variable describing the individual. But the theoretical content of the concept was lost in the transfer, so that by the time of Robinson’s celebrated attack on ecological correlation and the ecological fallacy (1950) there remained only a shadow reality of the
original conception of ecological forces, implicit within the idea of group-level variables.¹²

Often, Chicagoans considered natural areas expressly in relation to each other, making the pattern of spatial effects depend on various environing factors. Inevitably, this involved a move towards temporality. At the heart of the Chicago conceptual armamentarium — given in Park and Burgess's 1921 text — were concepts like "contact," "conflict," "assimilation," and "accommodation" that described the temporal patterns of reciprocal determination of groups by other environing groups. The situation here was analogous to that of the careers concept in the Chicago approach to temporal context. Just as there were more important social contexts and contingencies in careers than in natural histories, so also did the temporal environment play a greater role in these "area-careers" than in natural areas.

Wirth's (1928) discussion of the Chicago Jewish ghettos was of this "area-career" type. The relation of the ghetto to the larger forces of city expansion, to changing patronage of Jewish businesses, and to the temporal patterns of generational succession and of successive waves of immigration all shaped the experience of daily Jewish life. Wirth centered his analysis around the Jewish community, just as in his analysis of gang careers Thrasher (1927) retained a focus on the individual gangs. But each writer considered more extensive contingencies than in natural histories or natural areas; the development of successive Jewish communities was the group's career in a complex social and geographical space.

In the limit, the range of temporal contexts for a social structure became so great that again it required leaving the individual case behind and discussing the interactional field as a whole. Zorbaugh's The Gold Coast and the Slum again provides the clearest example, for the idea of an interactional field involves not only a range of social contexts, but a range of temporal ones as well. The story of the Near North Side involves not only long-term processes like changes in economic structure and in the composition of the immigrant population, but also shorter-term ones like local succession in neighborhoods, and even more rapid ones like the turnover of residents in rooming houses. Spatially, the interactional field involves not only the large-scale differentiation from and interdependence of this whole area on the city, but also shorter-range phenomena like the intermingling of church congregations produced by parishioner mobility and the economic interdependencies of the various subsections of the Near North Side itself.

Unlike the natural-areas concept, the concept of interactional fields did not make an easy transition to the variables world. Nonetheless, in some subfields, particularly historical sociology, it is alive and well. Two recent winners of the Sorokin award have in fact been built on this concept: Wallerstein's The Modern World System (1974) and my own book, The System of Professions (1988). To take the case that I know better, my argument about professions was essentially that professions themselves are like ethnic groups pushing around in Chicago and that the work that professions do is equivalent to the physical and social
TABLE 1: Context in the Chicago School.

<table>
<thead>
<tr>
<th>Degrees of Contextuality: Space</th>
<th>None</th>
<th>Some</th>
<th>Much</th>
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<tbody>
<tr>
<td>Degrees of Contextuality: Time</td>
<td></td>
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</tr>
<tr>
<td>None</td>
<td></td>
<td></td>
<td>Natural Area (Park)</td>
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<td>Some</td>
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<td></td>
<td>Area “Career” (Wirth)</td>
</tr>
<tr>
<td>Much</td>
<td>Natural History (Edwards)</td>
<td>Career (Thrasher)</td>
<td>Interactional Field (Zorbaugh)</td>
</tr>
</tbody>
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geography of the city itself. The history of professions is then a history of turf wars. One cannot write the history of any individual profession because that profession is too dependent on what other professions around it are doing. One can only describe the field of interprofessional conflict — the rules, the stratagems, the tricks, the by-plays of interaction. Into this field from time to time come larger forces: changes in professional work occasioned by technological and organizational developments that are equivalent to changes in the transportation and economic patterns of the city. These induce yet further cascading changes in the interactional field. And there are rules for the field, much like the political rules of the city. And so on. And so on. Although the filiation may seem distant, my book is at heart an exposition of an old Chicago concept.

We may think then of a 3 by 3 table in which the row dimension describes increasing degrees of temporal contextuality and the column dimension describes increasing degrees of social contextuality. I have essentially made the argument that the Chicago writers nearly always worked in the last (most contextual) row or column, and that these two intersect, in the 3,3 cell, in the concept of interactional field, which presumes multiple levels of social and temporal context. In the concept of interactional field, we must, like Zorbaugh, Wallerstein, or myself, move away from the level of individual cases and begin to describe the rules and regularities of interaction throughout the field. Contextual contingency is so complex that one cannot study the individual case directly and one cannot make predictions of any but the most general sort.

I shall later discuss some other cells of this little table. For the moment I merely wish to emphasize that implicit in this whole focus on context and contingency was a coherent vision of social structure. The Chicago writers believed social structure to be a set of temporary stabilities in a process of flux
and reciprocal determination. The social world was made up of actors mutually determining each other in ways sometimes deliberate and sometimes quite unforeseen. But the cornerstone of the Chicago vision was location, for location in social time and space channeled the play of reciprocal determination. All social facts were located in particular physical places and in particular social structures. They were also located within the temporal logic of one or more processes of succession, assimilation, conflict, and so on. This meant that the Chicago vision was of a social structure embedded in time, a structure in process.

I read this summary with no little irony. Didn’t the historical sociologists tell us that the general importance of Marxism in 1970’s sociology was that it gave sociologists a way to think about change and process? (Abbott 1991a). Did not the Weber of rationalization and other processes replace the Parsonian Weber in the same period and with much the same effect? Why is it that contemporary sociology so cleanly forgot what the Chicago School had to say? Why were they not our “classical sociology”? 13

In part, it happened because the Chicago work lies between the contemporary and the classic. When we read a classic, we ignore the old ideologies and the odd phrases in order to focus on that part of the text that addresses the perennial, the permanent, the enduring. Thus we make an important social theorist of Durkheim despite his silly neocorporatism, of Weber despite his penchant for pigeonholing, of Marx despite his preoccupation with a factory system now long gone. But the warm prose of the Chicago School fools one into reading it as contemporary work. 14 Then all at once, one is embarrassed by Park’s earnest theory of racial accommodation, by Cresssey’s description of a “tendency to promiscuity” (1932:xiii), by Thrasher’s analysis of “wanderlust” (1927:c.10). And so one problem is that the Chicago School seems neither old enough to be classic nor yet young enough to be contemporary.

Another problem has been simple snobbery. Chicago writing lacks the Latinate literacy and high tone of the Europeans. Our sociological theorists, like Henry James and many others before them, find the raw insight of American thought too much for their tastes. They prefer European sophistication, having managed somehow to forget the impact of pragmatism — the American philosophy that shaped Chicago sociology firsthand — on European theorists like Habermas (Habermas 1971).

Whatever the reason for ignoring Chicago, the central Chicago concept of locatedness has a peculiar importance for us today. For it can help us reconcile theoretical and empirical work. Most sociological theory, whether continental or American, contemporary or classic, keeps social facts in their contexts. It concerns process, relationship, action, and interaction. But most of our current empirical work concerns decontextualized facts with only a tenuous connection to process, relationship, and action. A general empirical approach founded on action in context could articulate much more effectively with our general theoretical traditions than do our present methodologies.

This gap between theory and empirical work must be of great concern to every contemporary sociologist. Of course, each subarea has its own theorists
and theories, as Merton (1948) urged in his celebrated remarks about middle-range theory, and these area theorists, as one might call them, are indeed much more closely tied to empirical work than are the generalists. But they work with a vocabulary of ideas about causality, about effects, about contexts, that is implicitly a general theory whether they will or no. And that general theory in fact derives from the decontextualizing methodological paradigm of modern sociology. Let me now consider that paradigm in more detail.

Notes on the Relation of Sociological Methods and Theory

The notion of removing social facts from their contexts did not begin with the decline of old Chicago in the 1930s. In one sense any social count does this, and social statistics after all date from the seventeenth century. Since the coming of the Hollerith machine in the 1880s, statisticians had been able to cross-classify social statistics extensively, a task helped by the invention of correlation in the same period. Cross-classification and correlation were used mainly for description, to say that people of type x were also likely to be y and z. This kind of typologizing is, in a mild way, contextual thinking, since it lumps values of variables into their “contexts” of other variables. But it is already a major step towards decontextualization, because it constructs types from individual variables, rather than analyzing them as gestals or emergents.15

The first major exponent of formal statistical methods in sociology was Franklin Giddings, the dominant figure in the Columbia sociology department in the first third of this century and the supervisor of such distinguished students as Howard Odum, William Ogburn, and Stuart Chapin. Giddings’s *Inductive Sociology* (1901) makes causal understanding the goal of sociology and conceives of causality as a sufficient combination of necessary causes. But after this stirringly Millian introduction, the book moves on to give, essentially, long lists of variables related to various social phenomena, complete with hints on measurement. Sociological laws are patterned after the law of gravitation or the ideal gas law, and so are essentially summaries of empirical correlations. In terms of my 3 by 3 table, Giddings was interested in the 1-1 cell, the cell with minimal contextuality in either space or time. It is also clear that, deep down, his idea of theory was simple empirical generalization. Causality, however interesting in principle, seemed to require a level of empirical generalization still far off.

The chief contrast to Giddings’s methods with their variables and correlations was provided by what were called “social surveys,” which were broad-based field researches on communities, institutions, and social problems, usually conducted by social workers or their ancillaries in the charity organization movement. These were focused studies, generally using multiple methodologies, but always retaining facts in their immediate contexts because the surveyors wanted to figure out why and how particular social problems occurred in order to change them. There were interview studies of workers in
particular industries, crime reports interlarding official statistics with
descriptions and case studies, and whole community studies involving teams
of researchers. *Middletown*, for example, is one of the last great products of the
social survey movement (Lynd & Lynd 1929). The survey movement had no
theoretical aims. It never aimed to discover, for example, the relation of
abstractions like "work" and "commitment to family" — the kind of "law"
that was central to Giddings's project — but rather studied (to continue the
example) the relation of a certain group of workers to their families within a
certain community and a certain industrial context.16

At the empirical level, then, the Chicago School was in many ways a hybrid
of the social survey tradition and the Giddingsian "scientific" sociology
(Giddings detested surveys [Bulmer 1984]) Although Park and Burgess spent
considerable time differentiating their "scientifically guided" work from the
meliorism of the survey movement [Bulmer 1964:c.5, Turner and Turner
1990:25ff], to the reader of today, the work of the Chicago School reads more
like the social survey literature than it does like the work of the Giddings
school.17

Yet the Chicago school had theoretical ambition. This differentiated its
procedures from those of the survey movement and gave the School's writings
a coherence that the products of that movement lack. As I have argued, Chicago
theory focused on the locatedness of social facts and the importance of
contextual contingencies.18 This theoretical commitment entailed the Chicago
mixture of methods, for if the effects of causes were so shaped by enwironing
factors that no causes had uniform effects, specific theories must be theories
about constellations of forces, not theories of individual causes. The fastest way
to discover such constellations of forces was by case study, since the sheer
combinatorics made studying the matter at the aggregate level difficult. And
more generally only the eclectic combination of ethnography, statistics, life
history, and organizational history could do full justice to the multiple layers
of spatial and temporal contexts for social facts.19

By contrast, Giddings's notion of causality — sufficient combinations of
necessary causes themselves conceived as abstractions — drives one to directly
investigate the varying properties of independent units. Only if causality
depends fundamentally on context does it make sense to pursue the Chicago
style of research. For the 1,1 cell of minimal contextuality, there is no need to
waste time situating facts. It seems to me, then, that the Chicago School
represented a distinct intellectual advance on Giddings's program, since it
addressed the complexity of social life but did so within a frankly scientific
framework. It replaced a particularly simple version of the variables approach
with a much more nuanced contextualist approach.

This new notion of emphasizing context had a distinguished intellectual
lineage, evident in Park and Burgess's text. The great European sources were
Ratzenhofer and Simmel. Surprisingly, the chief American interpreter of the
former was the oft-maligned Albion Small; Chicago students had thus been
hearing "ecological" theory long before the arrival of Simmel's student Robert
Park, and they had heard it from historicist sources, much as we today hear similar arguments from historical sociologists like Charles Ragin (1987). To these European sources were added the unique contributions of W.I. Thomas and, via Ellsworth Faris, George Herbert Mead, who between them provided a social psychology less mechanical and abstract than that of either Durkheim's socialized dope or Tarde's (and Giddings's) knowledgeable imitator. Thomas and Znaniecki's *Polish Peasant* study showed how the complexities of the social environment worked themselves out in individual lives and then rebounded through the individuals to reshape the environment and its institutions. The relation of individual and society was thus itself recontextualized as one of mutual contexts for each other.

In my view, then, the Chicago School made a decisive advance by joining the scientizing and the surveying traditions via the central idea of contextuality. Subsequent history shows, however, that the Chicago synthesis failed rather rapidly. A number of forces drove this failure. Some were institutional. The Rockefeller Memorial money dried up in 1932. Park left Chicago for Fisk University. The University's new president, Robert Maynard Hutchins, was unenthusiastic about social science. But other forces were intellectual. These are the more important.

For one thing, by the 1930s Herbert Blumer was laying the foundations of symbolic interactionism by emphasizing the symbolic, intersubjective side of the Chicago approach and by appropriating Mead's social psychology (some have said in vain, see Harvey 1987:161ff). In the process, he attacked earlier, more eclectic Chicago work, like the *Polish Peasant*, for being unscientific because its categories were not generated directly enough from the data. Blumer thus accepted the scientific aims of sociology, but helped further a split within it by conflating on the one hand objectivism, quantitative study, and variable-based approaches and on the other subjectivism, qualitative study, and case-based approaches. (This unification of previously cross-cutting dichotomies into an overarching opposition worked itself out in the department in the personal opposition between Blumer and Ogburn.) Blumer also missed the point about context, thinking that the central problem with variable-based approaches was their failure to capture the *subjective* ambiguities of the situation, rather than their denial of contextual determination in causality in general, of which the subjectivity problem was merely a part.

But the more important force in the decline of Chicago was the rise in the 1930s of opinion polling and market research. This was a much-improved version of the variables paradigm of Giddings. The extreme wing was led by Paul Lazarsfeld. Lazarsfeld favored purely operationalist social science, deliberately ignoring causal processes and theory, for which he had little hope. (Turner & Turner 1990:105ff,114ff. Coleman [1990] says Lazarsfeld "had a difficult time understanding sociological theory" [89]) His archetypical project was market research on consumer attitudes. Its ultimate aim was not to figure out why consumers thought what they thought, but simply to find which product they preferred (See Lazarsfeld and Rosenberg 1955).
Decontextualization was central to such a project partly because consumer tastes would change before an adequate contextualized theory of current tastes could be developed and partly because marketers could not hold "other things equal" in the survey and purchase situations. The necessary decontextualization of particular social attributes was then accomplished through the rapidly advancing discipline of sampling, which not only separated individuals from their social context of friends, acquaintances, and so on, but also deliberately ignored an individual variable's context of other variables in the name of achieving "more complete" knowledge of the variable space. Sampling not only tamed contextual effects to mere interactions, it also thereby produced data sets in which the levels of contextual causation were deliberately minimized. This would later enable a whole generation of sociologists to act as if interaction were a methodological nuisance rather than the way social reality happens.24

Like Lazarsfeld, Stouffer, the leader of the moderate survey researchers, believed that only modern survey analysis could produce the building blocks of a discipline (e.g., Stouffer 1950). Stouffer had underscored the contrast between survey research and the Chicago School in his celebrated dissertation, which tested the speed and efficiency of four student colleagues as judges of attitudes when pitted against a set of coding protocols. That the protocols produced the same answers in a fraction of the time Stouffer took as conclusive demonstration that protocols were the preferable approach to social research. But of course by posing the problem as "what is the fastest way to code a variable" he had already designed his answer. The great issue in the 1930s debate between case studies and survey methods was not over whether surveys could find variables faster; that was obviously true. Rather it was over whether the concept of variables — the concept of taking social facts out of their contexts — was a sensible one in the first place. About this, Stouffer's elegant experiment had nothing to say. For Stouffer was deeply committed to the idea of variables, as was clear from his later statement that general theory "does not provide us with interrelated propositions which can be put in the form if \( x_1 \), given \( x_2 \) and \( x_3 \), then there is a strong probability of \( x_4 \)." (1950:359). The aim of theory was to provide deductive sources for testable statements about variables, and if a theory did not provide those deductions, it was too vague to be useful. Variables were the reality.25

The practical importance of survey research's operationalism lay in its amenability to the equally operational body of statistical procedures invented by Ronald Fisher and his colleagues in the 1920s. (See Anderson 1989 for a discussion of the increasing dominance of these procedures.) Lazarsfeld himself preferred his esoteric "latent structures approach." But for the less adroit, there were the highly portable Fisherian methods, designed for specific operational aims — in Fisher's case, to decide whether or not fertilizer did any good on a particular field. (Stouffer had studied with Pearson and Fisher in the early 1930s [Bulmer 1984].) There was no pretense that the Fisherian factorial design could aid causal theory, although conversely it was easier to test hypotheses if one
had an effective causal theory. (See Kempthorne 1976 for an overview.) The causal emphasis was to come later (Bernert 1983). In the meantime, the main fight between the would-be Fisherian and the conservatives was over the issue of contextual causation. Fisher and most other biometricians had handled contextual effects through experimental design. Even so, Fisherian statistics suffered a brilliant contextualist attack from Jerzy Neyman, who blasted Francis Yates in the mid 1930s by showing that the whole procedure depended on assuming that contextual (interaction) effects were well-behaved (Traxler 1976). Surprisingly, nothing ever came of Neyman’s remarks. (Neyman himself had after all done fundamental work on sampling.) Indeed, brilliant but fruitless attacks on the Fisherian methods for their atheoreticality have continued for decades, the latest coming from Berkeley’s inimitable David Freedman (1987).

The makers of the new variables revolution saw that by removing social facts from their immediate contexts one could make them accessible to the power of the new inferential statistics. Correlational methods, regression, factor analysis, all the panoply of hypothesis-testing methods became applicable once one made the conceptual leap that “values of variables” were comparable across a wide variety of contexts. Even a hardened contextualist must admit that the combination of survey methods and statistics was radical and exciting. Like the contextualist, interactionist insight that preceded it, it produced an extraordinary flowering of sociology, books like Lazarsfeld, Berelson, and Gaudet’s The People’s Choice (1944), Stouffer et al.’s The American Soldier (1949), Berelson, Lazarsfeld, and McPhee’s Voting (1954), Lipset’s Political Man (1960), Coleman’s The Adolescent Society (1961), Blau and Duncan’s The American Occupational Structure (1967) — a very distinguished list.26

If you take the time to reread these books, you will find them to be raw, fresh, exciting. On rereading Blau and Duncan — which 25 years ago won the award for which I address you now — one feels the authors’ sense of acute excitement. Here is Duncan eager to try the ultimate wrinkle of the variables paradigm, the structural equation approach developed, ironically enough, by a Chicago colleague of Robert Park’s — the great biologist Sewall Wright. The book is written, to the great surprise of a contemporary reader, in narrative format. In each chapter, the authors walk the reader through what they do as they do it, rather than presenting it in some proper “scientific” or logical order. The authors are therefore present and visible. Their acute disappointment at the shortcomings of their data speaks out again and again. They frankly confess their strong statistical and methodological assumptions as well as the ways those assumptions violate reasonable views of the social process. Their apology is simply that perhaps radical assumptions will produce exciting results. And because one is swept up in their excitement, even the unsympathetic reader grants them the assumptions, just to see what will turn up. Like the contextualist paradigm, then, the variables paradigm produced enticing and exciting works.27

By the 1960s, the variables paradigm had built a whole edifice of social analysis. Thanks to Lazarsfeld and others, it had its major anchor in the
commercial survey literature — market research and polling. But it had taken over empirical sociology as well. The paradigm reached its high water mark in 1964 when Bernard Berelson and Gary Steiner published their summary of (I quote) "what the behavioral sciences now know about the behavior of human beings" (3). Most of the truths in the book, which, for those of you who don’t know or don’t remember, was entitled Human Behavior: An Inventory of the Findings, are in fact bivariate associations, sometimes with one or two controls. There are relatively few references to the reasons why things happen.28

This was soon to change. During the 1960s, sociologists began to accept a probabilistic image of causality that had always been implicit in the structural equation approach. The image of probable causality was borrowed from the physical sciences by Blalock (1964) and other writers to justify their work. Unlike market researchers and biologists, sociologists using the variables model began to think that their main effects stood for causal forces. Their articles implied that variables like gender, bureaucracy, and income could "do things" in the social world.29

The takeover of causal imagery was facilitated by generational turnover. By the 1960s the generation trained under the pre-variables paradigm began to disappear. New students learned the methods not as an adjunct to more general analysis or as a quick solution for empirical problems set by the interactionist paradigm. Rather, in true Kuhnian fashion, they learned from the method a set of assumptions about social reality that fundamentally shaped their vision of the social world. The paradigm gradually became self-enclosed because its methods negated any social fact they could not comprehend. By the 1970s many sociologists imagined the social world as a kind of general linear reality (Abbott 1988a). The closed nature of the paradigm dominated even theorists in subfields, whom Merton had hoped would prevent such closure. Even a writer like John Meyer, in proclaiming the new institutionalism in organizational studies, insisted on setting it forth in bivariate, decontextualized (other things being equal) hypotheses. (See Meyer & Rowan 1977; Scott & Meyer 1983).

It was this final move to "causal analysis" that created the real abyss between theory and methods. Men like Stouffer had had one foot in the old and one foot in the new. Stouffer was trained in a department dominated by ethnography, but himself moved away from it. He believed deeply in Giddingsian generalizations and dreamed that one could create deductive theories with decisively testable differences in empirical implications (Stouffer 1950). And Parsons, with Stouffer at his side at Harvard, believed much the same thing, although he wanted the major effort put into generating the theories and he meant by theory something far more abstract that did Stouffer. In the sequel, Parsons’s theories never specified any empirical implications, but for the moment the two men supported the temporary rapprochement of theory and methods that got sociology through its great flowering in the late 1950s and early 1960s. A similar, but closer, more synergistic relationship obtained between Merton and Lazarsfeld at Columbia.20
TABLE 2: Contextualist Methodological Examples

<table>
<thead>
<tr>
<th>Degrees of Contextuality: Space</th>
<th>None</th>
<th>Some</th>
<th>Much</th>
</tr>
</thead>
<tbody>
<tr>
<td>None</td>
<td>Mainstream Methodologies</td>
<td>Network Analysis (White)</td>
<td></td>
</tr>
<tr>
<td>Some</td>
<td></td>
<td>Robust Action (Padgett) Network Games (Abell)</td>
<td></td>
</tr>
<tr>
<td>Much</td>
<td>Sequence Analysis (Abbott) Narrative Formalism (Abell, Heis)</td>
<td>Encoding</td>
<td></td>
</tr>
</tbody>
</table>

But causalism went beyond that. Once empirical sociologists actually began to think that main effects represented something more than an analytic convenience they lost touch with the theoretical foundations of the discipline in real social action. The variables paradigm had begun with Ogburn, Stouffer, Duncan, and Lazarsfeld’s fortunate yoking of the new statistics to sociological thinking via the idea of variables. Causalism restricted this bridge to one-way traffic.

As a result, the variables paradigm has never really renewed itself. Today, it is old and tired. The intellectual exhaustion I described at the outset of my talk is in fact the exhaustion of the variables paradigm. After years of distinguished and important work, it has lost its capacity to excite us. In part it lost its excitement through self-absorbed technicality ("formal and empty ingenuity" said Mills 1959:75). In part it lost touch with real people, not only as readers but also as objects of analysis. In part, as I have said, it lost touch with social theory, which was, and continues to be, mainly about social action and interaction.

But perhaps most important of all, the fun is over. Today we never see the big wink from Duncan as he tries yet another far-fetched calculation. We never see a big wink from anybody. 31 Today, the sudden rush of new results that inevitably flows from new methods — the overall picture of political life that the new surveys made possible in Political Man, for example — is over as well. We can never be surprised that way again. Now we are simply filling in the details. The idea of variables was a great idea. But its day as an exciting source of knowledge is done.
The Future of Sociological Methodology

Now you might expect that I am about to say that if only we would read the Chicago School, and hearken to its message, the clouds would lift, sociology would become exciting, policy-makers would beat a path to our door, and so on. But you know, and I know, that that is nonsense. The flowerings of both the Chicago School and the variables paradigm depended — I’m being a good Chicagoan here — on a conjunction of things. It was not just good theory that was involved. In both cases, a large body of method and analytic technique lay ready at hand. Chicagoans built directly on the long-standing social survey tradition. The variables paradigm went nowhere until it began to use the analytic machinery of the Fisherians. Therefore, absorbing the contextualist message of the Chicago classics can do us no good unless there are methods at hand for putting that message into active empirical practice. The good news is that those methods are waiting for us.

Let me begin with desiderata. A methodology for contextualist sociology must put into some kind of empirical practice the basic concepts of the Chicago School that I outlined above. We require ways of discovering natural histories—long consistent patterns of events. We require ways of parsing careers—complex sequences with substantial environmental determination. We require ways of describing interactional fields. We require also ways of investigating complex spatial interdependence, and of making this spatial interdependence more and more temporally structured, till again we arrive at the description and measurement of interactional fields.

Note that I make no statements about the kinds of data required. A central reason for returning to contextualism is that it would bring together kinds of sociology long separated. As I said before, historical sociologists often do discuss natural histories and careers and interactional fields. Many of those in the interactionist tradition do discuss spatial interdependence. And our theorists do couch their theories in terms of action, often structured in time and space. In what follows, then, I shall focus mainly on positivist, formal methods for contextualism. For they are what the discipline needs most and what, fortunately, seem about to become widely available.32

In these closing minutes, then, I would like to discuss some work that begins to develop such methods. I do not mean that these new methods are in fact the particular techniques that will revolutionize sociology, but rather that they illustrate the kind of thinking that will produce the techniques that will revolutionize sociology. Once again, I proceed by considering the heavily contextual cells of my table.

I begin with studies of social context, for these have seen more work than temporal contexts, principally in what has become known as network analysis. Network analysis has substantial empirical results to its credit. Thus, Laumann and Knake’s Organizational State (1987) uses network analysis to uncover a complex and shifting structure within national politics that is completely unrecoverable through standard variable-based techniques. Location is a central
theme here, as it has been throughout Laumann's work. The empirical achievements of network analysis reflect an equally powerful set of theoretical foundations, developed largely by Harrison White and his students. Together, the empirical and theoretical sides of network thinking present a more closely unified approach to social reality than anything in the variables paradigm. And, of course, network thinking has raised the question of contextual determination in ways highly embarrassing to that dominant paradigm, as in White's book *Chains of Opportunity* — another winner of this prize — which has shaped studies of labor markets among groups from civil servants to psychiatrists and state police. (See Chase 1991 for a review on "vacancy chain" systems, and the sources cited above, note 8, for general sources on network analysis.)

Some daring investigators are pushing network analysis towards temporal complexity (the 2,3 cell of Table 2). Peter Abell (1990a) has begun to combine his formal analysis of narrative similarity with rational choice models of action. The result is a conception of "games in networks." Another example is Padgett and Ansell's (1993) analysis of networks of connections over the century-long buildup of the Medici political party in Florence. With methodologies developed from White's block modeling, Padgett shows in elegant detail how the kinship, economic, and political networks are slowly, often accidentally, constructed that ultimately give Cosimo d' Medici the possibility of "robust action," action largely independent of controls or coalitions under him (precisely the kind of action none of Laumann and Knopke's organizations can undertake). As in so much of contemporary sociological theory, Padgett and Ansell's direct focus is on action in structure. (White himself has just completed a book on this subject [White 1992].)

The same sort of methodological development is gradually coming to studies of temporal patterns and contextual determination. A number of theorists have been at work on formal models — particularly Thomas Fararo and John Skvoretz (e.g., 1984). But here, too, we see the development of practical empirical methodologies. (For a review, see Abbott 1992b)

The first of these are addressed directly at the classical problem of comparison and categorization of narratives across units: the 3,1 cell of Table 2, where temporal context is strong, but units are basically independent. This is the practical question of whether Park was right in emphasizing natural histories, careers, and other sorts of temporal patterns. My own work in what is called sequence analysis focuses here. I have used scaling techniques to analyze narratives of professionalization among American doctors, showing clearly that local power took precedence over local knowledge (Abbott 1991c). I have also used more esoteric techniques — sequence comparison algorithms from computer science — to analyze careers of musicians (Abbott & Hrycak 1990) and histories of welfare states (Abbott & DeViney 1992).

Others — chiefly David Heise and Peter Abell — have focused on more complex, interactive narratives involving several interdependent actors. Heise's methods (see Corsaro & Heise 1990; Heise 1989, 1991) arose out of his attempt
to code complex, ethnographic narratives in consistent ways. These methods produce logical structures of complex stories that can be compared across instances of one type (arguing with a policeman, say) or between types (arguing with a policeman compared with, say, academic seminars.) An elegant example is Griffin’s (1993) analysis of lynching. Abell (1987, 1990b, 1993) has taken a different approach (emphasizing mathematics rather than logic) to a somewhat similar coding of complex narratives. Abell’s studies of consumer cooperatives show how surprising similarities of outcome emerge under narrative (and causal) conditions that seem strikingly different. Other writers (e.g., Padgett 1981) have tried still other techniques.33

In their critics’ eyes, all of these studies, those emphasizing temporal context as well as those emphasizing spatial context, suffer from a crucial flaw. They lack “causal analysis.” They do not tell us “what are the crucial variables.” But that is their whole point. Causality as it has emerged in sociology since 1965 is so much reification. Social life is no more, in fact, than recurrent patterns of action in recurrent structures. These narrative methods, each in its own way, converge on the direct analysis of patterns of social activity in temporal and social context that were at the heart of the Chicago School. They tell us what are the crucial actual patterns, not what are the crucial variables.34

Like Park and Burgess, like Ogburn and Stouffer, workers in these new contextualist paradigms have found much of their methodology outside sociology itself. Harrison White and Peter Abell have put their physics Ph.D.s to good use in formalizing contextual structures. I have borrowed principally from biology and computer science. (Indeed, Ragin’s [1987] Qualitative Comparative Analysis — a variables-framework version of contextualism — comes originally from electrical engineering.)

While some of these borrowed techniques require serious mathematical training, the large set of problems involving classification of temporal or spatial patterns do not. As it happens, work on classification of complex patterns makes up a substantial fraction of the current computer science literature. These dynamic programming methods for pattern-matching have widespread application in biology, computer science, cognitive psychology, and related fields. They are without question the most general set of techniques currently under development in data analysis of any kind. Since they can be applied to patterns of several dimensions, they will ultimately be applicable to comparing networks just as I have used them to compare narratives. (For a current overview, see the sources cited in Abbott 1993).35

Imagine if we could tell a policy maker not just, “Well, if you put x amount of money on that problem the problem will grow .15 less than it otherwise would have next year.” But suppose we could say, “Well, if you put x amount of money on the problem, then a and b might happen, and if a happens then perhaps c, but if both a and b, then c is quite unlikely, and, since c is necessary to d where you are trying to go, then you can’t solve your problem by this approach, unless you can avoid b.” Imagine if that were not just a thought experiment that we all can and do in interpreting regression results, but were
a direct result of standard methods applied to data on policy experiences. That would be policy science indeed. Well, the methods are available to produce it. Right now. They simply await our imagination.

Conclusion

Sixty years ago, sociology had an intellectual revolution, the variables revolution. As so often happens, the revolution was misinterpreted by many of the participants. Stouffer thought variables were a shortcut to sociological truths that he still understood within the contextualist, interactionist paradigm (Turner & Turner 1990). Ogburn thought variables were a way of making sociology scientific, not a change of paradigm within an already established field.

Today we stand at the end of that journey. The variables paradigm is old and tired. How lucky for us then that not only do we have the great interactionist theoretical heritage to rediscover, but that, at the same time, a number of sociologists have begun to develop methods for studying action in context, methods that in turn can make use of the vast array of pattern-recognition techniques. Because of this conjuncture, we can now create a “positivism” attuned to the questions posed by our own theoretical heritage. We now have the empirical power to return social facts to their temporal and spatial contexts. We can look directly at social action by particular social actors in particular social times and places.36

In my view, then, we are not at all in crisis. Quite the contrary. Sociology stands before a great new flowering. New methods are available for borrowing. Problems for analysis are more pressing and more exciting than ever. Above all, we possess a goodly heritage of both theoretical and empirical work in the contextualist, interactionist tradition, bequeathed us by the Chicago School. That work provides a foundation and an example for where sociology ought to go. In this centennial year, then, there is no need to write a valediction of the Chicago tradition. One eulogizes only the dead.

Notes

1. To explain the disparity between the phrase “this centennial year” (i.e., 1992) and the current date (1997), let me briefly review the history of this article. The ASA’s Distinguished Publication award carries as its only material reward the right to deliver the Sorokin Lecture to whichever regional association is currently at the top of a rotating queue. On 10 April 1992, I read this article as my Sorokin Lecture to colleagues in the Southern Sociological Society. Despite the initial provenance, however, it seemed appropriate given the theme of the essay to publish it in the American Journal of Sociology. For reasons I do not know because I was too angry (then and since) to read the comments, the AJS turned the paper down flat. So I held onto it for use in a book. When I came to rewrite it for the book, however, it seemed on rereading so much a piece of platform oratory that I thought it a shame to revise it into the more sober language of a book. After all, one of its main points was that sociology
had gotten too sober. I resolved to try Social Forces, with the happy result that the article is before you, unrevised except for the addition of some scholarly machinery. I have not changed the dates or updated the references. I have not, indeed, removed any of the dramatic and polemical tone of the original; this is still a paper meant to be heard as well as read. The initial location explains the dozen or so references to other winners of the Sorokin award.

2. On historical sociology see Abbott 1991. The ASA Section on Comparative Historical Sociology sponsored a 1991 session on "Rational Choice Theory and Historical Sociology" that was little short of a witch hunt. On feminism, it is instructive to consult George Ritzer's (1988) contemporary theory text, which has a separate section on feminism written not by Ritzer by two invited women. The pattern of segmentalism rather than incorporation is painfully clear. See also Stacey and Thorne (1985). Some, however, believe this fission to be general in the social sciences (e.g., Levine 1981).

3. This separation is pretty clearly shown by Cappell and Gutterbock (1986, 1992). I am less confident of Ennis's (1992) more ambiguous results because of the high stress value for the two-dimensional scaling.

4. Thus Giddens sets his criterion in a characteristic sentence (1984:xxvii): "In formulating structuration theory I wish to escape from the dualism associated with objectivism and subjectivism" (as opposed to "in formulating structuration theory I wish to answer the following question."). Alexander's celebrated tetralogy (beginning with Alexander 1982) well illustrates the theory-without-empirical-referent school.

5. I mean more here than that social facts are always embodied, always found in particular instances, although that assertion is the foundation for my further meaning. Because social facts are always embodied, they are also always situated in relation to other social facts and other social bodies. It is this location that is intellectually central, not the mere fact of embodiment.

6. I have elaborated this point extensively (Abbott 1988a, 1990, 1992a), as has Peter Abell, an apostate from the variables camp. Abell (1987) is an excellent discussion of these issues.

7. On historical sociology, see Abbott (1991). In the literature on occupations, it is the concept of professionalization that has kept the idea of process alive. See Freidson (1986) or Abbott (1988b). On social movements, see the excellent review of McAdam, McCarthy, and Zald (1988). For symbolic interactionism, see Rock (1979) and Lewis and Smith (1980), as well as the more recent work of Maines (e.g., 1993).

8. Spatial theories are also reviving today in criminology, following the tradition of Shaw and McKay at Chicago. Of the network theorists, the classic works are Coleman (1961), Coleman, Katz, and Menzel (1966) on the one hand, and Lorrain and White (1971), White, Boorman, and Breiger (1976) and Boorman and White (1976) on the other. For collections, see Marsden and Lin (1982), Wellman and Berkowitz (1988), and Breiger (1990). At least one writer combines the Chicago theme of urban study and networks, Fischer (1982) in his study of personal networks. Geography itself is also making a comeback in sociology, see, e.g., Hochberg 1984, Hochberg and Miller, forthcoming. I should also note a recent Sorokin award winner that takes a strongly contextualist line on the city: Logan and Molotch's Urban Fortunes (1987).

9. Standard works on the Chicago School, some of which call the very phrase "Chicago School" into question, are Faris (1967), Bulmer (1984), Kurtz (1984), Harvey (1987), Smith (1988), and Deegan (1988). (Bulmer, Harvey, and Smith are Europeans.) The dates of major faculty appointments through 1940 (those of at least ten years' duration) are Small (1892-1925), Thomas (1895-1918), Park (1914-34), Burgess (1916-52), Faris (1920-39), Wirth (1926-52), Ogburn (1927-52), Blumer (1928-52), Hauser (1947-77), W. L. Warner (1935-59), Stouffer (1935-44), Hughes (1938-61), and E.A. Shils (1940-47, 1958-95). As these figures make
clear, the department turned over very suddenly in 1952 with the retirement of Ogburn and Burgess, the departure of Blumer for Berkeley, and the death of Wirth. Only Hughes and Hauser remained, of the tenured sociologists. (Warner was an anthropologist with a dual appointment.) The arrivals of Goodman (1950), Duncan (1951), Bogue (1954), Blau (1954), Katz (1955), Rossi (1956 — but leaving in 1967), Coleman (1957 — but leaving in 1959), J.A. Davis (1958), D. MacRae (1959), and H. C. White (1960) indicate the transformation of the department, largely through Ph.D.s from Columbia (Blau, Katz, Rossi, Coleman). After Hughes in 1938, A. Strauss (1954-58) was the lone “Chicago School” hire until Janowitz arrived in 1962 and reignited the Chicago ethos through his own writings and the “Heritage of Sociology” series. Nonetheless, a group of distinguished graduate students revitalized the tradition under the leadership of Hughes and Burgess in the immediate postwar period — A. Strauss (Ph.D. 1945), A. Rose (1946), M. Janowitz (1947), R. Turner, B. Meltzer, and T. Shibutani (1948), M. Dalton (1949), H. Becker (1951), E. Freidson (1952), K. Lang and E. Goffman (1953), G. Lang, R. Habenstein and J.A. Roth (1954), F. Davis (1958). Many of these writers, whom Fine (1992) quite rightly calls the “second Chicago School,” self-consciously maintained the tradition (mostly outside Chicago).

10. “Context” has two senses, of which one is more important to my argument than the other. The strict sense, which is my concern here, denotes those things that environ and thereby define a thing of interest. The loose sense simply denotes detail. The acute reader will note that these correspond nicely to the two judgments of the scientific worth of contextual information. If decontextualization is merely the removal of excess detail, then it’s a fine thing, scientifically. On the other hand, if it is the removal of defining locational information, it is a scientific disaster. I thank Donald Levine for demanding this clarification.

11. I am taking no position on whether disorganization was a good or a bad concept, the issue that has caused most spilt ink here. (See the various sources cited in Kurtz 1984:55-57, and esp., Alihan 1938). Rather my concern with the ecological studies of social disorganization is that they took space, connection, and context seriously.

12. The targets of Robinson’s article included such prominent Chicagoans as Ogburn, Shaw, and Harold Gosnell of the political science department. As is well known, Robinson’s mathematical argument shows that ecological correlations are generally higher than individual ones for artifactual reasons related to clustering on the variables. Many have interpreted Robinson’s article as enforcing the individual level of analysis, the decontextualization characteristic of emerging survey analysis. However, Robinson (1950) himself acknowledged that his argument did not affect those concerned with true “area-level” measurements; rather, as he said “even out-and-out ecologists, in studying delinquency, for example, rely primarily upon data describing individuals, not areas” (352). (His example was Clifford Shaw.) Robinson thus simply discounted the idea that the rates were indicators of a group level property of disorganization, taking them simply as individual behaviors. This was, of course, a theoretical decision of his, not Shaw’s. It is worth noting, in this regard, that like the survey tradition generally, Robinson simply ignored the problems of contagion and diffusion, assuming independent individuals as units of analysis, contrary to the theoretical tradition of ecology.

13. The standard answer to this question is that Chicago failed to become classic theory because it had no theory and that its empirical work is of mainly antiquarian interest. I have disposed of the first argument above. As for the second, not only is the Chicago work exciting reading today (as I am about to argue), but the Chicago data archives are in fact a rich mine for researchers willing to reanalyze them with current methods. It is nonetheless striking that C. Wright Mills’s (1959) denunciation of the 1950s sociological consensus (between “grand theory” and “abstracted empiricism”) makes no mention of Chicago sociology, even though one principal theme of his attack is decontextualization.
14. "In these pages," Everett Hughes writes at the opening of his 1928 book on the Chicago real estate board, "we do not observe a dinosaur's bones, for which we must imagine the flesh, but the struggling flesh itself of a living and young institution in the City of Chicago." Imagine that being published by a scholarly press today!


16. On the Survey movement, see the discussion in Turner and Turner (1990) and more generally the various essays in Bulmer (1991).

17. Although the Chicago School thus seemed to combine the attention of the survey movement to particular details with the scientific ambitions of the Giddings school, there were few direct links between them. Burgess had participated in a survey (Bulmer 1964:73; Harvey 1987:87) and Park taught a course on surveys, but both thought Chicago research practice to be far more systematic. (But see Deegan [1988] who sees a much stronger and direct link with the survey tradition.) On the "scientific sociology" side, Park and Burgess knew Giddings's work — it appears in their text (Park & Burgess 1921) — but were quite conscious that he lay in a different tradition (e.g., Park's introduction to the text.) Yet Chicago writing certainly reads like the surveys. For example, one may readily compare Ogburn's 1912 Columbia dissertation on child labor laws with Crystal Eastman's 1910 Work Accidents and the Law, one volume of the great Pittsburgh Survey, and with Everett Hughes's 1928 dissertation on the Chicago Real Estate Board. All three study the rise of regulatory institutions. But where Ogburn focuses on average ages of permitted entry across states, considering the increasing uniformity of these ages as the years pass, Eastman and Hughes study actual events in particular contexts. Particular actors are readily identifiable, as are patterns and constellations of forces surrounding particular events. One can follow processes within particular cases (in Eastman's case, in literally gory detail). None of this is possible in Ogburn.

18. Contrary to the usual comments about Chicago, ecology was and is a perfectly respectable genre of scientific theory. Our current notion of theory is so shaped by the variables paradigm that we define theory by conformality to that paradigm. Most sociologists think that that is theory which rigorously describes the relation between two variable properties of individual units. As a glance at theories of catalysis or evolution reveals, however, sometimes it is necessary, because of the impact of context on cause, to theorize systems directly in terms of the relations within them. Chicago-style ecology belongs to this branch of theorizing.

19. The eclectic combination was Bulmer's "Chicago Manifold" (Bulmer 1984). Note that the argument about combinatorics implies that the relation of case study and ecology is more an elective affinity than a necessity, something that was forgotten in the great methodological debates of the 1930s. Those tended to reduce a whole series of dichotomies to the single contrast of quantitative with qualitative work. See Burgess (1927) for an anguished plea for open-mindedness, ignored in the subsequent debate (covered in Bulmer 1984 and to some extent in Turner and Turner 1990).

20. None of Ratzenhofer's works is translated, to my knowledge. (Small translated portions, which appear in his own work and in the American Journal of Sociology [e.g., 10:177 ff, 1904]). The main source in English is parts 4 and 5 of Small's General Sociology: An Exposition of the Main Development in Sociological Theory from Spencer to Ratzenhofer (1905). For Small, Ratzenhofer's central insights were the ideas a) that not society but the social process was the subject of inquiry and b) that the social process consisted of a continuing interplay of
conflicting interests. Small's contextualist message is especially strong in *The Meaning of Social Science* (1910). For example, in speaking of social causes, Small says "The part that one of these factors plays at a given moment is a function of the operation of all the other factors at the same time." (1910:20). Sources on Simmel are so well-known that there is no need to cite them. Donald Levine has emphasized to me that Simmel's interest in interaction was highly abstracting, and therefore somewhat decontextualizing (loose sense) and that it was Park who insisted on bringing social forms into complex, contextual interrelation.

21. Chicago also, as Martin Bulmer (1984) has shown so well, pioneered the large-scale, externally-funded research enterprise in sociology, beginning with the *Polish Peasant* and continuing through the salad years of Laura Spelman Rockefeller Memorial support. There can be little question that part of Chicago's eminence stemmed from this support and from the failure of the dogmatic Giddings (Bulmer 1984:142) to command similar support. For related material on (and varying interpretations of) the decline of Chicago, see Bulmer (1984) and Turner and Turner (1990). In studying this decline, it is wise to separate the political decline of the department in the discipline (shown in the founding of the *American Sociological Review* and the "coup" of 1936 — a coup in which many young Chicago graduates participated) from the decline of the ideas now labeled as "Chicago School." These are in some ways quite different stories. Joan Aldous made the interesting suggestion to me that one reason for the decline of the intellectual message of Chicago was that people took it too literally, as if concentric zones were the message rather than the importance of context and location.

In making contextuality the central focus of the Chicago School, I am departing from the tradition that has emphasized the role of the subjective, of values, of intersubjectivity generally, in Chicago writing on social life (e.g., Harvey 1987, to some extent, and all the historians of symbolic interaction, starting with Blumer himself, [see, e.g., Rock 1979]). I am arguing that the important aspect of intersubjectivity is not so much its subjective character as its relational character. For another rejection of the Blumerian interpretation of Chicago, see Fisher and Strauss (1978). I am also seeing the Chicago focus on process — which many have noted before — as logically correlative with the Chicago focus on place, both physical and social. In doing this, I am of course reading selectively. It is not my aim to study the School in all its complexity. That has been well done by others. Rather I aim to take the School's central idea and return it to the foreground of active sociological consciousness. This is what I mean by reading the Chicago School writings as classic texts.

22. For discussions of Blumer's developing views, see Harvey 1987:136ff, Turner & Turner 1990:67ff, as well as Bulmer 1984:c.10. Blumer's own views are set forth in his celebrated critique of *The Polish Peasant* (Blumer 1946). His attack on the concept of variables was early (1931) and reiterated (e.g., 1956). But because of the other concepts that he often conflated with "variables" (like "rigor" at times), Blumer contributed heavily to the unnecessary polarization of the time. There is a large and contentious literature on the roots of symbolic interaction. See sources cited in note 7. There is also a current effort to reclaim Blumer as a macrosociologist (Maines 1989)! See also note 32 below for more on subjectivity.

23. It is deeply revealing that Lazarsfeld openly stated that the act of purchasing a good was the very archetype of human action (Lazarsfeld & Rosenberg 1955:389-390).

24. To my reading, the main text of this period — Lazarsfeld and Rosenberg's (1955) *Language of Social Research* — moved to a fully decontextualized position. Relation between cases makes a brief appearance in a couple of early Festinger papers and one sociometry paper (by Goodacre). Relation between variables (context within the variables paradigm) commands only one paper (by Barton). Otherwise, the variables paradigm is full blown in dozens of papers, with independent cases, independent variables, and narrative conceived as trends or panel changes in variables. The methodological Other for Lazarsfeld and Rosenberg is institutional research and ethnography, of which they say "in none of these
cases could we find systematic analyses of the methodological problems involved[!]" (1955:5). Insofar as Lazarsfeld and Stouffer were interested in context it was simply a matter of ecological variables, not taking the particular location of particular cases as primary. Stouffer's soldiers, for example, were as influenced by brigade, battalion, and company effects as by personal ones. (And Lazarsfeld was equally interested in "global" (i.e., emergent) variables [Coleman 1990:87]. But the ecological variables always worked through individual units, not as whole structures. I thank John Modell for demanding this clarification.

As sampling developed, random sampling proved problematic. For the clustering of cases in the variable space (and in terms of accessibility to sampling) meant that random sampling provided little information about unusual groups. The answer to this was stratified sampling, in which information about the real clustering of cases in the variable space was used to bypass that clustering in order to sample unusual groups beyond their presence in the population, later resetting their information to its proper proportion through weights. The clustering information thus was not a thing-to-be-investigated, but rather a problem in survey design. In theory, the aim was to make information on the unusual cases achieve the same relative error as information on the common cases (where large numbers coupled with the law of large numbers guaranteed rapid approach to population parameters). But this involved a philosophical assumption that relations in the underlying space were broadly linear, embodied in the statistical practice of making the behavior of parameters under different sampling strategies into the principal criterion for selecting statistics. Otherwise there would be no point in taking the unusual cases at all seriously; one would rather analyze the space directly in terms of clusters, that is, in terms of interaction. On the history of sampling, see Chang (1976) and Hansen and Madow (1976). The fundamental structure of modern sampling stems from a 1934 paper of Jerzy Neyman.

25. Stouffer's data-driven character shines through the classic story, repeated to me both by Howard Becker and Charles Tilly, that Stouffer stimulated his theoretical thinking by watching the cards pile up in the slots of the IBM card sorters. His research office had to have extra wiring to support three sorters (Terry Clark, personal communication.) Note, with respect to the quote in text, that $x_1, x_2, x_3, and x_4$ are not actions (in which case we would have Stouffer proposing generalized narratives) but rather variable properties of individual units of analysis.

26. It should be noted that there are works on this list that extend beyond the variables paradigm. The Adolescent Society is partly a network book, as were several other major works out of the Columbia Department, such as Katz and Lazarsfeld (1955). Nonetheless, in these books, networks were not seen as whole structures, but rather in terms of their final connections to the respondent. Katz and Lazarsfeld talk about "sequence" of influence, as Berelson, Lazarsfeld, and McPhee (1954) talk about the "process" of voting. But both of these are reconstructed out of information gathered as variables, and hence have lost most processual detail. One can see in these works, however, an attempt to preserve the concepts of particular social and temporal location within the variables framework. It was only after the revolution of causalism (see below) that even this attempt disappeared. The reader should recall that, as with my analysis of Chicago, I am not concerned with the important organizational contributions of Lazarsfeld and the Columbia department. For a note on these, see Glock (1979).

27. I am not the first to read Blau and Duncan with a largely literary eye. See Gusfield (1980). Contrasting it with Tally's Corner makes it seem quite a different book than it seems here, where I am contrasting it with later works in the status attainment tradition.

28. James Coleman has argued (Coleman 1990:91-92, 1992) that sociology became more individualistic because the society around it did. He implicitly locates the methodological change I have noted (towards operationalist and variable-based conceptions) as part of this move towards an individualized, disconnected society. Although the debate is beyond the
scope of this paper, I think, quite to the contrary, that the new individualism of the social
sciences was in fact constitutive of societal individualism. In any case, I am consciously
pursuing an internalist reading of the discipline here and so disregard his (important and
worrisome) argument.

Also, John Modell has correctly pointed out that I have ignored one crucial link between
the Chicago heritage and the new standard sociology, that running through human ecology
to demography, via people like McKenzie and Hawley. Although space forbids addressing
that strand of sociology, it is striking that demography today is thoroughly dominated not
by the concerns of theoretical ecology or even by formal demography, but rather by routine
applications of methods from the Lazarsfeldian heritage that I am discussing here.

29. For the smoking gun of probable causality, see Blalock 1964. The language can be seen
rapidly shifting, over the first few pages of the book, from a concern with actual events to
a removed level of variables. See Abbott 1991b and 1992a for detailed analyses of this type
of language.

30. It is useful to see exactly how this articulation worked. A classic example is Berelson et
al.'s Voting (1954) to which we have Parsons's detailed response (1959). What is striking
about the Berelson et al. book, to the reader used to the language of causality post-1965,
is that each chapter concludes with a long list of bivariate correlations, sometimes with
controls. The book concludes with a massive list of these and comparisons with several
other studies. The feel of the book is thus highly descriptive, in current terms. Where, we
would ask today, is the causal analysis? Moreover, the book feels quite historical in its
attention to particular details (and not just because these are old details. The book is thus
contextual in the loose sense.) To be sure, in formal terms process remains only as a nascent
panel cause diagram (281) and in the 16-fold tables that cross-classify two dichotomies at
two time points. But there is attention to particular political generations, to particular
historical processes, and, of course, there are the three waves of the survey on which the
book is based.

Yet the theoretical lessons learned have in fact little to do with the actual results. There
are two chapters of conclusions. The first — for the disciplines — correctly situates the book
as an attempt to mediate between a psychological and a more historical, structural view of
voting. The second — clearly for the general audience — examines the implications of the
findings for the normative theory of democracy. It is this second conclusion that is the sole
concern of Parsons in commenting on the work. Parsons (1959) gives a long interpretation
of the various "paradoxes" that Berelson et al. had used, in a somewhat ad hoc fashion, to
conclude their book. He weaves these into a theory of the equilibrating functions of the
political system. There is thus a nearly complete disjunctures between the "middle range"
conclusions with which Merton might have been concerned (chapter 13) and the grand
theoretical (Parsonian) ones (chapter 14). This disjunctures allowed Parsons to believe that
the work realized an aim he expressed in 1948: "The ideal is to have theoretical categories
of such a character that the empirical values of the variables concerned are immediate
products of our observational procedures." (1948:158). The Parsons/Stouffer "articulation"
was therefore actually a disarticulation bridged by a leap of faith. It thus becomes clear
how Mills (1959) could attack grand theory and abstracted empiricism as different things
even as their practitioners thought they were different aspects of one thing. Note, however,
that the link between Merton and Lazarsfeld was much closer, and much longer enduring,
than that between Parsons and Stouffer (Coleman 1990:89).
31. Duncan got the last wink on everyone when he published, as his last major work in sociology, Notes on Social Measurement in 1984. Notes blew a raspberry at vast areas of current sociological research and methodology, from LISREL (209-210) to occupational scales (194ff). Unfortunately, it has remained merely a cult classic.

32. In this connection, I have been asked several times why I have not dealt in this article with the topic of culture. This is an important question, for culture is a central topic in sociology today (as it should be) and in particular is associated strongly in many people's minds with both social temporality and social geography, both of which are seen as socially constructed. The obvious reason for ignoring culture is length. A second reason is that I do not think that the idea that meaning (causal or otherwise) arrives in part from context is the same as the idea that meaning is inherently multiple or complex. It is the latter that I take to be the central concern of my questioners, in part from their quizzical reactions to my conception of "narrative positivism" (Abbott 1992b). In that sense, I think culture a central topic, but one I don't dare take on when I am already fighting the battle of contextuality and causality. Like Blumer, some readers of this paper have tended to conflate variables, quantitative study, objectivism and analytical rigor on the one hand and interpretation, ethnography, subjectivism, and narrative on the other. (For a published example, see Richardson 1990.) I am arguing that a common concern for context unites various groups with various positions on those other dichotomies. Nonetheless, there is clearly an article like this one to be written about the disappearance of culture from mainstream empirical sociology.

33. The reader will note that I have left undiscussed the 3-3 cell — heavy on both temporal and social context. This is in part because I feel no one has achieved serious empirical analysis there. But more important, it reflects my judgment that the unification of the temporal line of contextualism (the narrative positivism of Heise, Abell, and myself) with the social contextualism of White-type network analysis requires some profound theoretical work. Most important, I feel that the temporalists have to overcome the teleology implicit in their narrative comparisons; after all, the past isn't "really out there somewhere." I think this problem can be addressed through the notion that past narratives are "encoded" in current social structure. But that is a matter for an article or book of its own, not for a short paragraph here. Some readers may worry that I have also not discussed event history analysis and the model of social reality implicit in it, since those seem quite temporally oriented. I have in fact written an article on that subject (Abbott 1990) and so slight it here.

34. It is striking that empirical social science's original stronghold — market research — is the strongest locus of non-causal analytic techniques like scaling and clustering. Marketers bet millions on scaling analyses (MDS was after all pioneered by the marketing group at Bell Labs under Joseph Kruskal), while sociologists, with the conspicuous exception of Edward Laumann, never publish them. There is of course an extensive literature on causality, of which Marini and Singer (1988) is a useful review. Sociology has most of the same problems with causality that medicine does. Its original models sought individually necessary causes, for its aims were often operational, and, as in medicine, the narrow neck of an individually necessary cause provides absolute operational control. In medicine, this kind of causal thinking works for yellow fever, but not for arthritis or cancer. Such complex diseases demand analyses of complex pathways leading to common outcomes. Looking for causes in such cases boils down, as Marini and Singer 1988:355 note, to finding "insufficient but nonredundant parts of an unnecessary but sufficient condition." That is the reality of "causal analysis" in sociology today.

35. My faith in these new analytic techniques reflects their direct focus on pattern, on relationship, on context and connection. These techniques can look directly at any sort of pattern, including the complex patterns, interactive in time and space, that old Chicago made central to its sociology. On seeing these methods, one can realize that in part old
Chicago died because its theory was too far ahead of its methods. For these are the quantitative methods that Park and Burgess would have seen as ideal for scientific sociological analysis.

36. Most important, our discipline has the strongest theoretical tradition in social science founded on the idea of interaction and contextual determination. That idea is utterly absent from the conceptual apparatus of our competitors the economists. Game theory is their first halting recognition that maybe the action of others is consequential for the actions of the self.

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1178 / Social Forces  75:4, June 1997


Of Time and Space / 1181


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