in G. Ritzer, ed., Metatheorizing. Sage, 1992: ~~203-217.~~ 27-52

TRADITIONS AND COMPETITION

Preface to a Postpositivist Approach to Knowledge Cumulation

JEFFREY C. ALEXANDER
University of California, Los Angeles
PAUL COLOMY
University of Denver

INTRODUCTION

SOCIOLOGY ONCE ASPIRED TO BE a cumulative science. Its practitioners once sought to develop and continuously expand verified knowledge about social patterns, social processes, and their underlying causal dynamics. A generation ago, sociologists shared a fervent belief that such cumulation of scientific knowledge required only that scholars "work like hell" testing hypotheses and theories (Cressey, quoted in Laub 1983; Zetterberg 1955). The result of these labor-intensive efforts was a plethora of paradigms, models, concepts, and empirical investigations concerning virtually every imaginable facet of the social world. Like the natural sciences it emulated, sociology seemed to be making indisputable progress (Stinchcombe 1968).

Today, for a large and growing number of sociologists (e.g., S. Turner 1988), this vision of progress seems to have been a mirage. The contrast between the earlier generation's ardent faith in the possibility of

AUTHORS' NOTE: An earlier version of this chapter was presented at the Section on Theoretical Sociology, "Metatheorizing in Sociology I," at the annual meeting of the American Sociological Association, Washington, DC, August 11, 1990. This chapter is an initial and abbreviated statement of a larger project that outlines a postpositivist model of knowledge cumulation and decline in the social sciences (Alexander and Colomy unpublished).

scientific growth and the current cohort's profound uncertainty about the ultimate product of their social science labors is stark and dramatic. Skepticism has supplanted faith, and words like malaise, pessimism, disintegration, and disillusionment increasingly color discourse about contemporary sociology (J. Turner 1989a; B. Turner 1989; Collins 1986).

To account for this change is certainly important, and we hope that one by-product of this discussion is the outline of an explanation that adds something to those already offered (e.g., Wiley 1979, 1985; Collins 1986; J. Turner 1989a; S. Turner and J. Turner 1990). This is not, however, our primary concern. This chapter is not an explanation but a response to the demoralization of sociology's orthodox scientific creed. An effective response, we argue, requires an alternative framework for understanding the nature of social science. The growth and decline of social scientific knowledge must be assessed in terms of new and more nuanced criteria than the earlier orthodoxy allowed.

Toward this end, we present the rudiments of a postpositivist model that identifies and explains advances and declines in sociological knowledge. Resting upon an alternative conception of the relationship between theory and fact, the model develops a counterintuitive assumption: it hypothesizes that sociological traditions are the critical units of analysis for assessing the cumulation of social scientific knowledge. Building upon this tradition-bound framework, we outline several distinct patterns of social scientific growth, using important classical and contemporary cases of theoretical and empirical shifts to illustrate the viability of our approach.

EXISTING THEORIES OF KNOWLEDGE CUMULATION IN THE SOCIAL SCIENCES

At present, sociology is being pulled in opposite directions by two competing theories of knowledge cumulation and decline: one a continuing version of the "hard," quasi-natural science orthodoxy, the other a reformulation of the "soft" approach to sociology as a literary and humanistic enterprise. In an important sense this debate revolves around the issue of boundaries between social science and other disciplines.

In an intriguing set of papers Gieryn (1983) and Gieryn, Bevins, and Zehr (1985) argue that scientists engage in "boundary-work" to

establish and reaffirm a positive public image for science. They do so by invidiously contrasting science with "nonscientific" intellectual activities. Gieryn shows that boundary-work is used strategically to legitimate a scientific discipline's professional claims to authority and its requests for tangible resources. The line demarcating science from nonscience, his studies demonstrate, is highly contingent and markedly responsive to changing historical circumstances.

Gieryn's imagery of a moving line between science and nonscience speaks directly to our concerns about the current condition of sociology. It should be emphasized, however, that these shifting boundaries have cognitive as well as ideological consequences, and we accord the former more attention. Furthermore, the pertinent boundaries are often multiple rather than singular, and proponents frequently frame their arguments in terms of social science's boundaries with two or more disciplines. Finally, intergenre boundary work may be positive as well as negative. Gieryn is primarily concerned with the negative boundary work involved in distinguishing science from nonscience. But it is also instructive, and this is particularly true in the case of the social sciences, to consider the positive boundary work manifest in attempts to forge powerful links between one set of intellectual activities and another.

Positivism (Toulmin 1953; J. Turner forthcoming) is the philosophical basis for the quasi-natural science view of sociology. Until very recently, positivism not only supplied the dominant theory of how knowledge cumulates and declines in sociology, but it also directly informed virtually all social science practice. Attempting to forge a strong identification with the natural sciences, its proponents asserted that if a boundary between the social and the hard sciences existed at all, it was minuscule. Sociologists were urged to embrace the methodological apparatus and procedures of the more mature sciences and to investigate "social facts" (Durkheim [1894] 1938) with the same dispassionate objectivity that hard scientists purportedly brought to their study of physical ones.

The Frenchman who invented the term *sociology*, August Comte, argued forcefully for the construction of a negative boundary between the science of society and speculative philosophy. Sociology was to be as devoid of metaphysical commitments as were the sciences of nature. This "positive science," as Comte called it, would consist entirely of propositions, laws, and causal statements; interpretations and value judgments would not intrude. As many have noted (e.g., Fuchs and Turner 1986) these efforts to wed the fledgling discipline

of sociology to the more prestigious natural sciences represented, in part, a readily transparent maneuver to wrestle legitimacy, status, and material resources from both the established scientific community and the wider public. But "ideal" interests were at stake as well, and in the long run these proved (contra Mullins 1973) even more consequential.

In the century-long development of sociology (Eisenstadt and Curelaru 1976; Shils 1970; S. Turner and J. Turner 1990), this perspective was refined in various ways, and contrasting versions were elaborated. Nonetheless, a broad "positivist persuasion" (Alexander 1982a) continued to provide for sociology a unifying, if rarely articulated creed. That persuasion rested upon a series of postulates that continue to form the basis for its adherents today. First, it presumes that a radical break exists between empirical observations and nonempirical statements. Thus theory is a qualitatively different entity than fact. Second, positivism argues that more highly generalized intellectual issues have no fundamental significance for the practice of an empirically oriented discipline. In its most contemporary rendition, this argument holds that "metatheoretical" discussions and debates dissipate intellectual energies that could be employed more productively in "real" scientific work (see J. Turner 1985, 1989b,[1] in contrast with Ritzer 1988, 1990a, 1990b; Fuhrman and Snizek 1990). Third, the positivist persuasion holds that the elimination of nonempirical referents is a distinguishing feature of the natural sciences and, therefore, that a truly scientific sociology must follow suit if it is to assume an equally scientific stature (Stinchcombe 1968). Fourth, questions of a general theoretical nature, it is argued, can be adequately addressed only in relation to empirical observation. Several additional points follow. With regard to the formulation of social theories, the positivist persuasion argues that the process should be one of induction and generalization from observation, or specification through hypothetico-deduction. Critical empirical tests and falsification are enshrined as the final arbiter in theoretical disputes. Finally, it is held that there is no logical basis for generalized, ongoing, and structured types of scientific disagreement.

The revolutionary development of the natural sciences gave tremendous impetus to the positivist persuasion in social science. At an earlier period in the history of human thought, explanations of nature were deeply embedded in metaphysical and speculative themes. Before physics, there was natural philosophy; before astronomy, there was cosmology. If, as Barnes and Becker (1952) once asked, thinking

about nature could make the transition to rationality and positive empiricism, why not thinking about society? Indeed, Durkheim's ([1894] 1938) influential methodological program was premised on the belief that this transition had already been made. In the 20th century, the growing power, prestige, and self-confidence of the natural sciences pushed social science even further in this direction. With the development in the postwar period of sophisticated methodological techniques borrowed directly from the natural sciences, this positivist dream seemed as if it were becoming a reality (e.g., Blalock 1976).

In recent years, however, developments in the history and philosophy of natural science (e.g., Toulmin 1953, 1972; Kuhn 1970) have thrown increasing doubt on the positivist persuasion. These broad intellectual developments have made positive ties between the sciences of nature and the human studies more difficult to sustain in consistent and unambiguous ways. Although there are important differences of emphasis within this antipositivist movement, there is a widely shared understanding that the match between scientific theories and external reality is much more problematic than the positivist persuasion envisioned; indeed, antipositivists hold that theories necessarily involve conjecture and highly contestable interpretations. These investigations have underscored the independent contributions that nonempirical and generalized elements make to the most respected scientific work. Not surprisingly, these trenchant criticisms of positivism have had tremendous ramifications for disciplinary communities (like sociology) that had used the hard sciences as a cognitive and legitimating exemplar (cf. Gouldner 1970). If positivism does not fully explain how knowledge grows in the sciences of nature, then how can it account for the growth of knowledge in sciences that hardly approximate their rigor, precision, and impersonal controls? If positivism does not adequately explain the cumulation of knowledge in the natural sciences, how can its precepts continue to be dutifully accepted as dictums for social science practice?

In sociology, positivism still has articulate and passionate defenders (e.g., J. Turner 1985, 1990, forthcoming; Collins 1975, 1988, forthcoming), and it continues to function as an orienting strategy for contemporary sociological work. Even its defenders, however, are well aware that the discipline's stance toward orthodox positivism has changed fundamentally, that what once could be readily assumed about the nature of sociological inquiry has recently become an object of skepticism, if not downright derision (J. Turner 1989a; Giddens and Turner 1988).

It is within this context of growing skepticism about positivism that reflections about the current "malaise" of sociology should be understood. The discipline's apparent transition from a single to a multiple paradigm science (Ritzer 1975) exacerbated the relativism and self-doubt that accompanied the loss of positivist self-confidence. The proliferation of apparently disconnected subfields (J. Turner 1989a; Dogan and Pahre 1989; Collins 1986) and the ostensible split between theoretical work and a multifarious array of substantive areas (J. Turner 1989b, 1990; B. Turner 1989) contrast sharply with the positivist alliance, shaky though it was, of functionalist theory and quantitative methods that characterized an earlier day.

In contrast with positivism, the other leading perspective about knowledge cumulation in the social science assumes a negative stance toward natural sciences and a positive relation to what it refers to as the "human sciences." Although this position has been available throughout the 20th century, outside of the exceptional German case it never posed a serious intellectual threat to the proponents of a positivist sociology. That in recent years it has acquired increasing stature and a wider audience must be understood in the context of positivism's decline and sociology's fragmentation.

Against Comte, the German philosopher Wilhelm Dilthey (1976) argued that between the human studies and natural science there stands an unbridgeable gulf. In a more constructive vein, Dilthey sought to build strong links between the social sciences and the arts and literary interpretation. According to what he called the hermeneutic position, social science consists of interpretations and descriptive models; if and when causal statements are attempted, they can emerge only from within the subjective world of the social scientist's own experience.

In this view, social science is a fundamentally different kind of activity from its counterpart in natural science. Its objects of investigation—"social facts"—are either states of mind or conditions that are interpenetrated with them. In order to construct the very objects of a social science, therefore, investigators must draw on their own life experience and on their personal understandings of other human beings. This places a premium not only on observation and measurement but on imagination and speculative thought experiments. Once the objects of social science are conceptually constructed, moreover, it is not easy to verify or falsify the social science theories that generalize from them in a definitive way. Because the personal experiences and evaluative standards of investigators are bound to differ, the

embeddedness of social science in value judgments, different personal sensibilities, and political ideology is impossible to avoid.

The human studies position raises serious and unavoidable questions about the possibility of cumulating knowledge about the social world (Friedrichs 1970). Advocates of a hermeneutics approach argue that *understanding* rather than *explanation* should be the major goal of social inquiry. In its weak form (Giddens 1984), hermeneutics allows generalizations, although cautioning that they will be of a fundamentally more tentative character than those in the natural sciences. In its strong version, hermeneutics declares that the possibility of a universal, objective, and generalizing science is completely illusory and that the human studies should be restricted either to critical analysis from a moral perspective (Gouldner 1970; Haan, Bellah, Rabinow, and Sullivan 1983) or to descriptive accounts of unique or "idiographic" events (Winch 1958).

This alternative to mainstream positivism throws the cumulation of social scientific knowledge into doubt. Because research and theorizing are heavily dependent on the interpretive skills of the individual investigator, the dynamics of social studies are viewed as largely idiosyncratic and essentially unstructured. One logical conclusion is to celebrate subjectivity and relativism (Hollinger 1985) and to abandon the search for general principles that are applicable to a wide range of phenomena in favor of the pursuit of thick description (Geertz 1973) and moral interpretation (Friedrichs 1970; Haan et al. 1983). We will argue against this course.

A POSTPOSITIVIST APPROACH TO KNOWLEDGE CUMULATION

Although the hermeneutic, human studies approach supplies a fundamental corrective to positivist orthodoxy, it embraces a framework about which we believe the social sciences must be extremely wary. Philosophically (Alexander 1990a, forthcoming; Toulmin 1972), this path leads to an extremely vulnerable form of relativism; socially (Alexander 1990b), it can lead to a dangerous and enervating distrust of reason itself.[2] An alternative paradigm that moves beyond both positivism and its antipositivist extreme is necessary if sociology is to avoid the difficulties associated with either of these positions.

In contrast to both of the approaches outlined above, we propose a substantially different model to examine both progressive and regressive developments in sociological knowledge. This perspective is a reaction to both the powerful critique leveled at orthodox positivism by philosophers and historians of science (e.g., Kuhn 1970; Lakatos 1968, 1970; Toulmin 1953) and to the severe limitations the human studies approach would impose on efforts to generate cumulative social knowledge. Unlike human studies, it suggests that social scientific knowledge can grow and, over the long run, certainly has grown. At the same time, its characterization of how knowledge advances and declines is quite different from conventional positivism. Our postpositivist alternative rests on four basic assumptions.

The first holds that sociological work is profitably analyzed as falling along a scientific continuum ranging from abstract, general, and metaphysical elements on the one end to the concrete, empirical, and factual on the other (Toulmin 1953; Alexander 1982a). Other elements of scientific discourse, including ideologies, models, concepts, laws, propositions, methodological assumptions, and observational statements, fall between these endpoints. Even though its overall form may be characterized more by one element than another, every social scientific statement contains implicit or explicit commitments about the nature of every other element on the scientific continuum. The nature and types of social scientific debate are limited by the distinctive character of these elements. The discussion and controversies that mobilize the profession focus on particular elements and emphasize certain kinds of discourse over others.

Second, these basic elements with which sociology is built cannot be formulated in an infinite variety of ways. Although social scientists usually accept one formulation or another without hard and definitive evidence in a natural scientific sense, they do not accept a position without argument and vigorous efforts at intellectual persuasion. Such efforts are rational in the sense that they refer to generalized criteria that themselves must ultimately be justified through open and uncoerced debate (Habermas 1984). Indeed, it is our contention that important social scientific debates largely consist of arguments over the criteria for evaluation that are immanent in different levels of discourse (e.g., criteria about presuppositions, ideologies, models, and methods).

Third, in the history of sociological thought the options available at each discursive level have been sharply limited. In terms of what they presuppose about human nature, for example, students of society

have usually been preoccupied with the degree to which actors act in either an instrumentally rational fashion or with reference to moral rules or emotional need (Parsons 1937; Ekeh 1974; Alexander 1987a; Stinchcombe 1986). The options for ideological discourse are more historically bounded (Gouldner 1970), but in the modern era at least a continuous argument between relatively coherent conservative, liberal, and radical arguments can be observed. As for models of society (cf. Eisenstadt and Curelaru 1976), the axes of dispute have concerned the relative randomness or coherence of systems, on the one hand, and the relative dynamic versus equilibriating tendencies of systems, on the other. The conflict between interpretive and causal approaches has preoccupied general methodological disputes.

Fourth, although in principle there is no intrinsic relationship between the different elements arrayed across the scientific continuum, there is a clear tendency for certain kinds of commitments to hang together. Thus there are no empirical or logically compelling theoretical reasons for an interpretive methodology to be combined with the commitment to a nonrational or normative understanding of action. Yet, *structural* considerations of theoretical logic must not be confused with the *contingent* issue of historical and empirical probability. In the history of social thought, the commitments made at different scientific levels have not been randomly interrelated. To use a Weberian phrase, there has often appeared to be an "elective affinity" between some theoretical commitments and others (Eisenstadt and Curelaru 1976; Gouldner 1970). Conflict models of society, for example, tend to be more attractive to radical than conservative thinkers, and rationalistic presuppositions are more characteristic of liberals than conservatives. But an even more powerful contingent factor must be considered. Whatever the purely logical possibilities for intrinsic (as compared with elective) affinity between options at different levels, practicing social scientists usually *believe* that certain imperative linkages do exist. The reason is that social science practice unfolds within powerfully stated theoretical traditions, and every tradition stipulates the relationship between theoretical elements in a sharply defined way (Tiryakian 1979, 1986; Shils 1970; Wiley 1979; Seidman 1983).

In our view, the various forms of sociology are carried forward by traditions, which are typically called "schools."[3] We would define sociology, indeed, as a multilevel rational discourse about society and its constituent units, with the patterns and directions of that discourse being conditioned by the discipline's leading traditions. The elements

of this definition form a paradox but not a contradiction. Traditions, of course, are patterns of perception and behavior that are followed not, in the first instance, because of their intrinsic rationality, not because they have "proven their worth," but because they are inherited from the past. The traditional status of social scientific schools confers upon them prestige and authority, which is reinforced because they are typically upheld by organizational power and supported with material resources (Mullins 1973; Fuchs and Turner 1986). These considerations do not, however, mitigate the rational aspirations of social science, its sharply delimited structure of debate, and its often extraordinary ability to approximate and understand social reality.

Like other traditions, the rational movements of social science are founded by intellectually charismatic figures, whose followers believe that their powerful attraction stems from their awe-inspiring scientific prowess. At the beginning of a discipline, such great intellectual figures are regarded as classical founders (Alexander 1987b); at later points, they are accorded quasi-classical status and are treated simply as the founders of powerful disciplinary traditions. This organizational fact shows in yet another way why social science practice cannot be understood simply as the confrontation between scientist and social reality. Social reality is never confronted in itself. Because perception is mediated by the discursive commitments of traditions, social scientific formulations are channelled within relatively standardized, paradigmatic forms. The matrix social scientists inhabit need not be drawn from a single tradition or be wholly of a piece, but inhabit it they must, aware of it or not.

Although traditionalism implies habitual behavior, it need not imply stasis or lack of change. In social science, this openness to change is intensified by the universalism of institutionalized standards that mandate impersonal rationality and push against the particularism of a traditionalist response (Merton [1942] 1973). Social science traditions define themselves by staking out theoretical cores that are highly resistant to change, but there are substantial areas surrounding these nuclei that are subject to continuous variation (Lakatos 1968, 1970; Kuhn 1970). In ideal-typical terms, changes in the peripheral areas of traditions can be conceived as proceeding along three lines: *elaboration, proliferation,* and *revision* (Wagner 1984; Wagner and Berger 1985; Berger, Wagner, and Zelditch 1989; Alexander 1979; Colomy 1986, 1990). Although these lines of development present themselves as loyally carrying out traditional commitments, they differ

in the creativeness with which they pursue this task. Because elaborative and proliferative sociological work proceeds from the assumption that the original tradition is internally consistent and relatively complete, they aim primarily at refinement and expansion of scope. In revisionist work, by contrast, there is a greater sense of the vulnerabilities of the established tradition; in the guise of loyal specification, an often implicit effort is made to address these strains and to offer formulations that can resolve them (Alexander 1979; Colomy 1986, 1990).

Elaboration, proliferation, and revision are lines of specification that recur periodically in a tradition's history, not only in the period of routinization that immediately follows the charismatic founding but in the wake of the powerful reformulations that must emerge if a tradition is to remain intact. The latter possibility points to a fourth ideal-typical form of theoretical change. Insofar as cores themselves undergo substantial shifts—without abandoning their association with the overarching tradition—there occurs a theoretical activity that can be called *reconstruction* (Alexander and Colomy 1990b). Reconstruction differs from elaboration, proliferation, and revision in that differences with the founder of the tradition are clearly acknowledged and openings to other traditions are explicitly made. Reconstruction can revive a theoretical tradition, even while it creates the opportunity for the kind of development out of which new traditions are born (e.g., Habermas 1979).

The most far-reaching form of scientific change carries the reconstructive impulse farther still and brings us back full circle to the intellectually charismatic founders of sociological traditions. *Tradition-creation* involves generating new schools organized around historically distinctive cores. The essence of tradition-creation is the synthesis of elements drawn from several existing and often competing intellectual paradigms, with the aim of generating the theoretical core of a new school. Marx's reconfiguration of elements from Hegelianism, the Enlightenment, French socialism, and British political economy represents the best documented (Alexander 1982b) instance of this form of scientific change.

One should be careful not to see these ideas—elaboration, proliferation, revision, reconstruction, and tradition-creation—as presenting either a necessary historical sequence or a scale of theoretical significance. As for sequence, with one important exception to be noted below, different types of change weave in and out of both the history of sociology and the historical course of each particular tradition. As

for significance, most of the greatest minds in social science never made the transition from reconstruction to tradition-creation. Many who attempted to make the transition, moreover, were much the worse for it. The works of Von Wiese are long forgotten; the writings of Gramsci, Lukacs, Mannheim, and Mauss continue to be intently pursued.

Traditions can also be destroyed (*tradition-deconstruction*). This does not happen because core and peripheral commitments are falsified in the narrow sense. It occurs because these commitments have become delegitimated in the eyes of the scientific community. Delegitimation leads to the withdrawal of trust from core commitments. Only after core commitments are abandoned can fundamental falsification be understood as having occurred. Even in this situation, however, traditions do not so much disappear as become latent; the possibility always remains (cf. Eisenstadt and Curelaru 1976) that they may be picked up again.

Elaboration, proliferation, revision, reconstruction, tradition-creation, and tradition-deconstruction describe the closeness of fit between subsequent theoretical and empirical work and an original tradition. It is important to emphasize that they do not describe the degree of real scientific advance. Elaboration, for example, may be thin or thick, to redeploy Geertz's (1973) ethnographic standard. Traditions may be enriched and elevated by the processes of theoretical change we have identified, but they may also be impoverished and simplified, robbed of their sophistication, and denuded of some of their most powerful intellectual sustenance. If social science change can be progressive, therefore, it can be regressive as well.

Over the long run, the dynamics of traditions within a disciplinary community (cf. Shils 1970)—the shifting fortunes of its theoretical positions—are not determined by the theoretical effectiveness and sophistication of the respective positions, nor by their objective empirical scope. Shifts in a discipline's "scientific sensibility" (Alexander 1986), usually precipitated by significant social and global developments (e.g., the anti-Vietnam-War movement, the Civil Rights struggle, and the push for democracy in Eastern Europe), put different questions on the floor; they place a premium on the creation of different modes of discourse. Indeed, it is often only after highly generalized and discursive commitments (e.g., Gouldner 1970) are made to a new approach that increased theoretical sophistication and empirical scope emerge. It is in this sense that one can speak less of social scientific "development" than of social scientific "movements."

Disciplines (contra Merton 1968) should not be understood as being organized primarily by specialties defined by their empirical objects of investigation (i.e., into middle-range subfields like deviance, political sociology, and stratification). The deep structure of a discipline (Toulmin 1972) consists of the networks and literatures that are produced by the contact between empirical objects, ongoing traditions, and new disciplinary movements.

TRADITIONS AND COMPETITION

Competition plays a critically significant role in the cumulation and decline of social scientific knowledge. Indeed, according to our model, social science does not grow simply because of the compulsion to understand empirical reality, nor can its growth be measured merely in relation to the expansion of empirical knowledge or conceptual scope. The primary motor of social scientific change is conflict and competition between and within traditions. The primary reference points for measuring scientific growth are established by the relations between traditions and by signposts internal to a given tradition itself. Instead of speaking about theoretical or empirical progress per se, one must speak of relative explanatory and theoretical success, vis-à-vis one's own tradition or competing ones (B. Turner 1989).

Every ideal-typical pattern of knowledge cumulation and decline is driven by competition. Implicitly or explicitly, every scientific statement claims to be more incisive or compelling on some point(s) than previous work. Accordingly, potential contributions are always partially assessed by comparison to earlier efforts.

Competition occurs in both discursive genres, and it occurs between and within traditions. At the level of generalized discourse, competition proceeds through disputes centered about a tradition's residual categories, its analytic and empirical breadth, its theoretical acumen in interpreting the classics, its avowed or implied ideological stance, its resonance with the epoch's reigning issues and social movements, its logical coherence (or lack thereof) as expressed through its conceptual schemes, and its utility for empirical investigation. At the level of research programs, competition is organized around rival attempts to explain empirical structures and processes

regarded as significant by the discipline. In either case, a tradition advances when it issues statements deemed superior relative to comparable work produced by other schools.

At any given time, the field on which traditions compete is organized hierarchically.[4] Traditions are invidiously compared, and a small subset are accorded high levels of prestige. Such recognition is contingent on intermittent displays of scholarly virtuosity. Advantages accruing to those affiliated with prestigious traditions (e.g., greater publishing opportunities and a larger audience for those publications) unquestionably can facilitate the production of first-rate work. At the same time, however, the more renowned a school, the more likely its products will be subject to rigorous scrutiny. This disciplinary judgment that a tradition is especially illustrious encourages competing schools to frame their discussions as critical alternatives to the reigning approach. Proponents of less esteemed paradigms are constrained to demonstrate their tradition's relative merit by highlighting its theoretical and empirical strengths vis-à-vis more hegemonic paradigms. Thus a recent discussion of the Chicago school's "second generation" (i.e., the contributions of many of the sociologists who received their Ph.D.s from the University of Chicago's Department of Sociology between 1945 and 1960) indicates that Chicago sociology's generalized discourse as well as its research programs in role theory, deviance, social problems, the professions, formal organizations, and collective behavior and social movements were presented as critical responses to the then prominent functionalist tradition (Colomy and Brown forthcoming).

When a tradition is challenged, and especially when the challenge is regarded as legitimate and meritorious, its proponents are obliged to respond. For a variety of reasons, however, an insular strategy may be embraced with advocates presenting only occasional, perfunctory rebuttals or dismissing virtually all outside criticism as uninformed and unwarranted. In the short run, an insular strategy can sustain stability and some intellectual progress, primarily through elaboration and proliferation. In the long term, however, isolationism tends to delegitimate a tradition in the eyes of the disciplinary community and leads to its eventual eclipse.

Competition spurs incomplete or incipient traditions to devise more comprehensive formulations. Research programs that have not yet devised a complementary body of generalized discourse are highly vulnerable to metatheoretical critiques explicating the implicit and often restrictive assumptions upon which the research is premised. Thus

despite the impressive empirical advances generated by the status attainment program, its failure to develop an explicit metatheoretical rationale led some to discredit it as atheoretical (e.g., Buroway 1977; Coser 1975) and others to suggest that the generalized discursive questions raised about the program have precipitated a crisis in status attainment research (Colclough and Horan 1983).[5] The most effective response to such charges, of course, is to articulate and defend the analytic grounds of the research program. Likewise, critics frequently assail incipient traditions that emphasize generalized discourse to the apparent neglect of empirical research. In this context, Giddens's analytically innovative and sophisticated structuration theory has been indicted for its failure to devise a compelling research program (Gregson 1989; Muller 1990). Again, the most viable rejoinder is to demonstrate the tradition's empirical fruitfulness by launching research programs in several specialty areas. This retort is most persuasive, moreover, if the new research proves superior to extant programs affiliated with long-standing traditions. For instance, Saks (1983) rebukes neo-Marxists and neo-Weberians for their continuing dependence on the patrimony of discursive attacks leveled against earlier functionalist and interactionist treatments of the professions rather than devising a viable research program of their own. For more established traditions, competitors' critiques and alternative explanatory models constitute a conceptual and research agenda that the focal school can address through elaboration, proliferation, revision, and/or reconstruction of both its genres. Recent revisions of the functionalist research program on social change, for example, are self-consciously presented as rejoinders to the charges leveled by the theory's critics (Colomy 1986; Alexander and Colomy 1990a).

Because established traditions constantly change and new schools frequently emerge, the boundaries linking and separating paradigms are regularly subject to reassessment. Typically cast as discussions of the similarities and differences between competing schools and usually pitched at the level of generalized discourse,[6] this intertradition boundary work—whether between symbolic interaction and ethnomethodology (Zimmerman and Wieder 1970; Gallant and Kleinman 1983), neofunctionalism and structuration theory (Muller 1990), feminism and Parsonian theory (Johnson 1988, 1989), Marxist and Weberian theory (Antonio and Glassman 1985; Wiley 1987), or postmodernist and critical theory (Habermas 1981, 1987; Kellner 1989, 1990)—can clarify and reaffirm existing divisions, introduce alterations in the intellectual core of one or more traditions, and/or,

by highlighting previously unrecognized commonalities, lay the groundwork for syntheses of variable scope between approaches once regarded as largely irreconcilable.

As the preceding remarks imply, traditions are not hermetically sealed and competition between them can produce some convergence in both generalized discourse and research programs. When members of antagonistic schools address a similar problem and draw on some of the same intellectual resources to resolve it, their theorizing and research frequently reveal agreements alongside continuing differences. Highlighting common themes in the work of scholars affiliated with several rival approaches, Ritzer (1990c), for example, detects a diffuse, cross-tradition movement toward a synthetic position on the micro-macro issue. Commonalities can also emerge through expropriation, which occurs when proponents of a given approach openly appropriate an idea developed by competitors and employ it, usually with significant modifications, to extend their home traditions. Collins (1985, 1988), for instance, adopts the neofunctionalist notion of multidimensionality to advance a more inclusive version of conflict theory.

Over time, competition engenders significant changes on the disciplinary field. Established and highly regarded traditions are discredited and sometimes disappear, lowly ranked schools gain prominence, and new paradigms flourish. The alterations reflect schools' varying ability to fashion persuasive responses to both the critiques issued by rival traditions and shifts in disciplinary sensibility stemming from encompassing global and societal transformations. The difficulties in responding satisfactorily to these recurring challenges are enormous, and it is not surprising that most traditions experience periods of crisis or that many expire shortly after they are initiated. To persist, traditions must change and those that last for more than a generation are almost always substantially revised and reconstructed. Antonio (1990) suggests that Marxism has been periodically declared intellectually bankrupt only to renew and reconstitute itself and reappear phoenix-like on the disciplinary scene. We would add only that Antonio's characterization is applicable to every enduring social scientific tradition.

The discussion thus far has proceeded as if each tradition was an intellectually consensual community. By definition there is considerable consensus among a school's adherents, but this does not prevent serious disagreements from arising. Indeed, most schools contain two or more tradition segments[7] that although affiliated with the same encompassing framework and pledging scientific fealty to the

same classic progenitor(s) nevertheless make disparate commitments at one or more levels of the scientific continuum. Although personal considerations undoubtedly play an important role and the availability of resources such as stable employment, students, funding, and publishing outlets exert a powerful conditioning effect, the fault lines along which tradition segments arise and the intellectual grounds used to support them are most readily understood as fundamental disagreements about the school's generalized discourse and research programs. For example, Meltzer and Petras (1970; see also Meltzer, Petras, and Reynolds 1975; Buban 1986; Reynolds 1990) maintain that though the (old) Iowa and Chicago schools of symbolic interactionism shared many assumptions about social action and order and acknowledged Mead as the founding figure of their school, their conflicting assumptions about methodology, determinism, and the nature of the self prompted the formation of distinct tradition segments.

Virtually every enduring tradition generates competing tradition segments; the longer a school persists the more segments it will create. Perhaps no social scientific tradition can stake a more rightful claim to longevity than Marxism and few if any have produced a larger number of segments, with distinctive renditions branching off at nearly every point along the scientific continuum (e.g., Alexander 1982b, p. 328-70; Bottomore 1975, 1978, 1988; Bottomore and Goode 1978; Anderson 1976, 1983; Antonio 1990; Aronson 1985).

Relations among tradition segments are always competitive, but the competition ranges from the friendly and mutually enriching type to more divisive forms that precipitate rancorous, public breaks and the formation of extremely hostile moieties. Even in the latter case, however, competition between segments can be among the most productive modes of scholarly exchange, resulting in significant contributions to every type of knowledge cumulation. Because all parties are well versed in the tradition's generalized discourse and research programs and are cognizant of the school's analytic and empirical shortcomings, disputants can prepare astute critiques and equally informed replies. In some instances, a segment's proponents may adopt an insular strategy in reaction to another's challenges, but such isolation is even more difficult to sustain among competing segments than it is between rival traditions.

The tendency to structure the disciplinary field hierarchically recurs among tradition segments. Depending on the number of competing segments within a school, there is a tendency to cast one or two

segments in starring roles, while relegating others to much smaller parts. This invidious division places the intellectual burden of proof on the latter camp and in order to demonstrate their relative scientific prowess, insurgent segments can be expected to emphasize how their contributions account for anomalies more established segments purportedly cannot explain.

Founders of traditions that subsequently splinter into competing strands are usually associated with the school's most preeminent segment. So long as the founder continues to produce, it is unlikely that challengers will supplant his or her segment's privileged position.[8] Successful challenging segments more typically appear either after the founder's death or during dramatic shifts in disciplinary sensibility.

The disciplinary community as a whole tends to minimize differences between competing segments, treating a particular approach as a single and more or less coherent whole. Maynard and Clayman (forthcoming) note that despite the diversity within ethnomethodology, commentators usually treat it as a unitary perspective. Apart from the intellectual commonalities and personal relations that may bind adherents of rival segments, this disciplinary perception engenders an externally imposed sense of shared fate that encourages various forms of what might be called tradition teamwork (Goffman 1959) vis-à-vis the larger discipline. That teamwork is manifest in founding scholarly associations, securing official recognition for specialty areas deemed crucial to the tradition, engaging in collaborative publishing ventures, and defending the work of competing segments from criticism by adherents of rival traditions.

Finally, it is important to recognize that although competition generates winners and losers,[9] it is hardly an infallible mechanism for advancing knowledge. Competition between and within schools is as much a sociological process as an epistemological one, and the dynamics that propel it can impede genuine knowledge cumulation. Plainly put, the traditions or tradition segments that win in the social sciences do not always have the best arguments. The dynamics of fashion sometimes figure prominently in the rapid ascent of new traditions. Fashionable schools are not exempt from criticism—if nothing else scholarly communities are flush with critics—but the questions raised about the new approach may have little impact, at least in the short term, on the disciplinary community's assessment. On the other hand, the traditions or tradition segments that lose do not always advance the least defensible arguments. When debate is

the challenge that postpositivist science studies poses to contemporary social science. The critical effect of competition between perspectives is vastly underestimated, as is the role that general discourse plays in stimulating and framing the ongoing work within research programs.

If social science is once again to become a legitimate public activity, this crisis of confidence, which at its roots is no less than a crisis of confidence in reason itself (Alexander forthcoming), must be resolved. Our perspective offers the possibility that there are secure epistemological and, indeed, moral foundations for advances in the social sciences. For such a substantial conception of progress to be maintained, however, positivism must be fundamentally reconstructed and a new model of social scientific growth erected in its place.

NOTES

1. Recently, J. Turner (1990) has tempered his antipathy to metatheory and suggested that it is useful "when the goal is to produce scientific sociology" (p. 50). However, he remains highly critical of other forms of metatheory that in his view do not advance sociology as a science.

2. These broader ramifications are briefly discussed in the concluding section of this chapter and in much greater detail in Alexander and Colomy (unpublished).

3. This section elaborates an argument we have developed elsewhere (Alexander and Colomy 1990b).

4. Furthermore, competition is affected by the unequal distribution of material and symbolic resources across traditions that condition the production and reception of sociological discourse. These issues are examined in Alexander and Colomy (unpublished).

5. Horan (1978) disputes the contention that status attainment research is atheoretical. He contends that the program is premised on a functionalist conception of social structure—one he regards as analytically restrictive and, therefore, vulnerable to criticism on discursive grounds.

6. Intertradition boundary work also occurs at the level of research programs. Handel (1979), for instance, outlines a synthesis of functionalist and interactionist treatments of the structure and dynamics of social roles. However, in this case, as in many other efforts to integrate rival research programs, the proposed synthesis proceeds by specifying complementarities in the generalized theoretical logic of each program. Similarly, attempts to highlight differences between competing research programs—e.g., Collins's (1971) contrast between functionalists, and conflict theorists' studies of education—point to disparities in each program's underlying assumptions. The central point is that generalized discourse figures prominently in most intertradition boundary work, even when the connections or discrepancies between particular research programs are the primary concern.

7. The notion of tradition segments is adapted from Bucher and Strauss's (1961) discussion of professional segments. Their analysis highlights the diversity and conflict

within professions and suggests that competition between segments is an important source of change in professional communities.

8. When a splinter group competes on equal (or nearly equal) terms with the founder, it is often because the insurgent segment is itself organized around an intellectually charismatic challenger.

9. Competition can also produce stalemates. Focusing on rival research programs, Wagner (1984) argues that because competitors disagree about (a) the criteria appropriate for evaluating competing theories, (b) how to apply criteria they agree are appropriate, (c) the relevance of existing data, and (d) the interpretation of the data they agree is relevant, and because comparisons of competing theories tend to degenerate into irresolvable metatheoretical disputes, "competition is not a very efficient form of theory growth" (p. 75). It should be noted that in subsequent work Wagner has formulated a more comprehensive statement, arguing that in the context of contesting theoretical research programs competition can advance social scientific knowledge (Wagner and Berger 1985; Wagner 1984, pp. 104-5).

REFERENCES

Alexander, J. C. 1979. "Paradigm Revision and 'Parsonianism.' " *Canadian Journal of Sociology* 4:343-57.

———. 1982a. *Positivism, Presuppositions, and Current Controversies*. Berkeley: University of California Press.

———. 1982b. *The Antinomies of Classical Thought: Marx and Durkheim*. Berkeley: University of California Press.

———. 1986. "Science, Sense, and Sensibility." *Theory and Society* 15:443-63.

———. 1987a. "Action and Its Environments." Pp. 239-318 in *The Micro-Macro Link*, edited by J. C. Alexander, B. Giesen, R. Münch, and N. J. Smelser. Berkeley: University of California Press.

———. 1987b. "On the Centrality of the Classics." Pp. 11-57 in *Social Theory Today*, edited by A. Giddens and J. Turner. London: Polity Press.

———. 1990a. "Beyond the Epistemological Dilemma: General Theory in a Postpositivist Mode." *Sociological Forum* 5(4:531-544).

———. 1990b. "Between Progress and Apocalypse: Social Theory and the Dream of Reason in the Twentieth Century." Pp. 15-38 in *Rethinking Progress*, edited by J. Alexander and P. Sztompka. London: Unwin Hyman.

———. Forthcoming. "General Theory in the Postpositivist Mode: The 'Epistemological Dilemma' and the Search for Present Reason." In *Postmodernism and General Social Theory*, edited by S. Seidman and D. Wagner. New York: Basil Blackwell.

Alexander, J. C. and P. Colomy, eds. 1990a. *Differentiation Theory and Social Change*. New York: Columbia University Press.

———. 1990b. "Neofunctionalism Today: Reconstructing a Theoretical Tradition." Pp. 33-67 in *Frontiers of Social Theory: The New Syntheses*, edited by G. Ritzer. New York: Columbia University Press.

———. Unpublished. "The Structure and Dynamics of Traditions: Toward a Postpositivist Model of Knowledge Cumulation and Decline in the Social Sciences."

Anderson, P. 1976. *Considerations on Western Marxism*. London: NLB.

———. 1983. *In the Tracks of Historical Materialism*. London: Verso.

Antonio, R. J. 1990. "The Decline of the Grand Narrative of Emancipatory Modernity: Crisis or Renewal in Neo-Marxian Theory." Pp. 88-116 in *Frontiers of Social Theory: The New Syntheses*, edited by G. Ritzer. New York: Columbia University Press.

Antonio, R. J. and R. M. Glassman, eds. 1985. *A Weber-Marx Dialogue*. Lawrence: University Press of Kansas.

Aronson, R. 1985. "Historical Materialism, Answer to Marxism's Crisis." *New Left Review* 152:74-94.

Barnes, H. E. and H. Becker. 1952. *Social Thought from Lore to Science*, 2nd ed. Washington, DC: Harran Press.

Berger, J., D. G. Wagner, and M. Zelditch, Jr. 1989. "Theory Growth, Social Processes and Metatheory." Pp. 19-42 in *Theory Building in Sociology*, edited by J. Turner. Newbury Park, CA: Sage.

Blalock, H. 1976. "Implicit Theories Underlying Macro-Level Data Analysis: Measurement Errors and Omitted Variables." Pp. 76-100 in *New Directions in Sociology*, edited by T. Bottomore and R. Nisbet. Newton, MA: Abbott, David & Charles.

Bottomore, T. 1975. *Marxist Sociology*. New York: Holmes & Meier.

———. 1978. "Marxism and Sociology." Pp. 118-48 in *A History of Sociological Analysis*, edited by T. Bottomore and R. Nisbet. New York: Basic Books.

———, ed. 1988. *Interpretations of Marx*. Oxford, England: Basil Blackwell.

Bottomore, T. and P. Goode, eds. 1978. *Austro-Marxism*. Oxford, England: Clarendon.

Buban, S. L. 1986. "Studying Social Process: The Chicago and Iowa Schools Revisited." *Studies in Symbolic Interaction*, 2(Suppl.):25-38.

Bucher, R. and A. Strauss. 1961. "Professions in Process." *American Journal of Sociology* 66:325-34.

Buroway, M. 1977. "Social Structure, Homogenization, and the Process of Status Attainment in the United States and Great Britain." *American Journal of Sociology* 86:618-29.

Colclough, G. and P. M. Horan. 1983. "The Status Attainment Paradigm: An Application of a Kuhnian Perspective." *The Sociological Quarterly* 24:25-42.

Collins, R. 1971. "Functional and Conflict Theories of Educational Stratification." *American Sociological Review* 36:1002-19.

———. 1975. *Conflict Sociology*. New York: Academic Press.

———. 1985. "Jeffrey Alexander and the Search for Multi Dimensional Theory." *Theory and Society* 14:877-92.

———. 1986. "Is 1980s Sociology in the Doldrums?" *American Journal of Sociology* 91:1336-55.

———. 1988. *Theoretical Sociology*. New York: Harcourt Brace Jovanovich.

———. 1989. "Sociology: Proscience or Antiscience." *American Sociological Review* 54:124-39.

———. Forthcoming. "Sociological Theorizing." In *Postmodern and General Social Theory*, edited by S. Seidman and D. Wagner. New York: Basil Blackwell.

Colomy, P. 1986. "Recent Developments in the Functionalist Approach to Change." *Sociological Focus* 19:139-58.

———. 1990. "Revisions and Progress in Differentiation Theory." Pp. 465-95 in *Differentiation Theory and Social Change*, edited by J. C. Alexander and P. Colomy. New York: Columbia University Press.

Colomy, P. and J. D. Brown. Forthcoming. "The Chicago School and the Specter of Functionalism." In *The Second Chicago School of Sociology*, edited by G. A. Fine. Chicago: University of Chicago Press.

Coser, L. A. 1975. "Presidential Address: Two Methods in Search of a Substance." *American Sociological Review* 40:691-700.

Dilthey, W. 1976. "An Introduction to the Human Studies." Pp. 159-67 in *Dilthey: Selected Writings,* edited by H. P. Richman. Cambridge, England: Cambridge University Press.

Dogan, M. and R. Pahre. 1989. "Fragmentation and Recombination of the Social Sciences." *Studies in Comparative International Development* 24:1-18.

Durkheim, E. [1894] 1938. *The Rules of Sociological Method.* New York: Free Press.

Eisenstadt, S. N. and M. Curelaru. 1976. *The Forms of Sociology: Paradigms and Crises.* New York: John Wiley.

Ekeh, P. K. 1974. *Social Exchange Theory: The Two Traditions.* Cambridge, MA: Harvard University Press.

Friedrichs, R. W. 1970. *A Sociology of Sociology.* New York: Free Press.

Fuchs, S. and J. H. Turner. 1986. "What Makes a Science Mature? Organization Control in Scientific Production." *Sociological Theory* 4:143-50.

Fuhrman, E. and W. Snizek. 1990. "Neither Proscience nor Antiscience: Metasociology as Dialogue." *Sociological Forum* 5:17-31.

Gallant, M. J. and S. Kleinman. 1983. "Symbolic Interactionism Versus Ethnomethodology." *Symbolic Interaction* 6:1-18.

Geertz, C. 1973. "Thick Description: Toward an Interpretive Theory of Culture." Pp. 3-30 in *The Interpretation of Cultures,* by C. Geertz. New York: Basic Books.

Giddens, A. 1984. *The Constitution of Societies.* Oxford, England: Polity Press.

Giddens, A. and J. Turner. 1988. "Introduction." Pp. 1-10 in *Social Theory Today,* edited by A. Giddens and J. Turner. Oxford, England: Polity Press.

Gieryn, T. F. 1983. "Boundary-Work and the Demarcation of Science from Nonscience: Strains and Interests in Professional Ideologies of Scientists." *American Sociological Review* 48:781-95.

Gieryn, T. F., G. M. Bevins, and S. C. Zehr. 1985. "Professionalization of American Scientists: Public Science in the Creation/Evolution Trials." *American Sociological Review* 50:392-409.

Goffman, E. 1959. *The Presentation of Self.* New York: Doubleday.

Gouldner, A. 1970. *The Coming Crisis of Western Sociology.* New York: Basic Books.

Gregson, N. 1989. "On the (Ir)relevance of Structuration Theory to Empirical Research." Pp. 235-48 in *Social Theory of Modern Societies: Anthony Giddens and His Critics,* edited by D. Held and J. B. Thompson. Cambridge, England: Cambridge University Press.

Haan, N., R. N. Bellah, P. Rabinow, and W. N. Sullivan. 1983. *Social Science as Moral Inquiry.* New York: Columbia University Press.

Habermas, J. 1979. "Toward the Reconstruction of Historical Materialism." In *Communication and the Evolution of Society,* edited by J. Habermas. Boston: Beacon Press.

———. 1981. "Modernity Versus Postmodernity." *New German Critique* 22:3-14.

———. 1984. *Theory of Communicative Action,* vol. 1. Boston: Beacon Press.

———. 1987. *Lectures on the Philosophical Discourse of Modernity.* Cambridge, MA: MIT Press.

Handel, W. 1979. "Normative Expectations and the Emergence of Meaning as Solutions to Problems: Convergence of Structural and Interactionist Views." *American Journal of Sociology* 84:855-81.

Hollinger, R., ed. 1985. *Hermeneutics and Practice.* South Bend, IN: Notre Dame University Press.

Horan, P. M. 1978. "Is Status Attainment Research Atheoretical?" *American Sociological Review* 43:534-41.

Johnson, M. M. 1988. *Strong Mothers, Weak Wives.* Berkeley: University of California Press.

———. 1989. "Feminism and the Theories of Talcott Parsons." Pp. 101-18 in *Feminism and Sociological Theory*, edited by R. Wallace. Newbury Park, CA: Sage.

Kellner, D. 1989. *Critical Theory, Marxism, and Modernity*. Cambridge and Baltimore: Polity Press and John Hopkins University Press.

———. 1990. "The Postmodern Turn." Pp. 255-86 in *Frontiers of Social Theory: The New Syntheses*, edited by G. Ritzer. New York: Columbia University Press.

Kuhn, T. 1970. *The Structure of Scientific Revolutions*, 2nd ed. Chicago: University of Chicago Press.

Lakatos, I. 1968. "Criticism and the Methodology of Scientific Research Programmes." *Proceedings of the Aristotelian Society* 69:149-86.

———. 1970. "Falsification and the Methodology of Scientific Research Programmes." Pp. 91-196 in *Criticism and the Growth of Knowledge*, edited by I. Lakatos and A. Musgrave. New York: Cambridge University Press.

Laub, J. L. 1983. "Interview with Donald R. Cressey." Pp. 131-65 in *Criminology in the Making*, by J. L. Laub. Boston: Northwestern University Press.

Maynard, D. W. and S. E. Clayman. Forthcoming. "The Diversity of Ethnomethodology." *Annual Review of Sociology*.

Meltzer, B. N. and J. W. Petras. 1970. "The Chicago and Iowa Schools of Symbolic Interactionism." Pp. 3-17 in *Human Nature and Collective Behavior*, edited by T. Shibutani. New Brunswick, NJ: Transaction.

Meltzer, B. N., J. W. Petras, and L. T. Reynolds. 1975. *Symbolic Interactionism: Genesis, Varieties, and Criticism*. London: Routledge & Kegan Paul.

Merton, R. K. 1968. "On Sociological Theories of the Middle Range." Pp. 39-72 in *Social Theory and Social Structure* (enlarged ed.). New York: Free Press.

———. [1942] 1973. "The Normative Structure of Science." Pp. 267-78 in *The Sociology of Science*, edited by N. W. Storer. Chicago: University of Chicago Press.

———. 1987. "Three Fragments from a Sociologist's Notebooks." *Annual Review of Sociology* 13:1-28.

Muller, H. 1990. "Neofunctionalism or Structuration Theory." Paper presented at the annual meeting of the International Sociological Association, Madrid, Spain, July 11.

Mullins, N. 1973. *Theories and Theory Groups in Contemporary American Sociology*. New York: Harper & Row.

———. 1983. "Theories and Theory Groups Revisited." *Sociological Theory*. 1:319-37.

Parsons, T. 1937. *The Structure of Social Action*. New York: Free Press.

Reynolds, L. T. 1990. *Interactionism: Exposition and Critique*. Dix Hills, NY: General Hall.

Ritzer, G. 1975. *Sociology: A Multiple Paradigm Science*. Boston: Allyn & Bacon.

———. 1988. "Sociological Metatheory: A Defense of a Subfield by a Delineation of its Parameters." *Sociological Theory* 6:187-200.

———, ed. 1990a. *Frontiers of Social Theory: The New Syntheses*. New York: Columbia University Press.

———. 1990b. "Metatheorizing in Sociology." *Sociological Forum* 5:3-15.

———. 1990c. "Micro-Macro Linkage in Sociological Theory: Applying a Metatheoretical Tool." Pp. 347-70 in *Frontiers of Social Theory: The New Syntheses*, edited by G. Ritzer. New York: Columbia University Press.

Saks, M. 1983. "Removing the Blinkers? A Critique of Recent Contributions to the Sociology of Professions." *Sociological Review* 31:1-21.

Seidman, S. 1983. *Liberalism and the Origins of European Social Theory*. Berkeley: University of California Press.

Shibutani, T. 1970. "On the Personification of Adversaries." Pp. 223-33 in *Human Nature and Collective Behavior,* edited by T. Shibutani. New Brunswick, NJ: Transaction.

Shils, E. 1970. "Tradition, Ecology, and Institutions in the History of Sociology." *Daedalus* 99:760-825.

Stinchcombe, A. L. 1968. *Constructing Social Theories.* New York: Harcourt Brace Jovanovich.

———. 1986. "Reason and Rationality." *Sociological Theory* 4:151-66.

Tiryakian, E. A. 1979. "The Significance of Schools in the Development of Sociology." Pp. 211-23 in *Contemporary Issues in Theory and Research,* edited by W. E. Snizek, E. R. Fuhrman, and M. K. Miller. Westport, CT: Greenwood.

———. 1986. "Hegemonic Schools and the Development of Sociology: Rethinking the History of the Discipline." Pp. 417-41 in *Structures of Knowing,* edited by R. Monk. Lanham, MD: University Press of America.

Toulmin, S. 1953. *The Philosophy of Science.* New York: Harper & Row.

———. 1972. *Human Understanding,* Vol. 1, *The Collective Use and Evolution of Concepts.* Princeton, NJ: Princeton University Press.

Turner, B. 1989. "Some Reflections on Cumulative Theorizing in Sociology." Pp. 131-47 in *Theory Building in Sociology,* edited by J. H. Turner. Newbury Park, CA: Sage.

Turner, J. H. 1985. "In Defense of Positivism." *Sociological Theory* 3:24-30.

———. 1989a. "The Disintegration of American Sociology." *Sociological Perspectives* 32:419-33.

———. 1989b. "Can Sociology Be a Cumulative Science?" Pp. 8-18 in *Theory Building in Sociology,* edited by J. H. Turner. Newbury Park, CA: Sage.

———. 1990. "The Misuse and Use of Metatheory." *Sociological Forum* 5:37-53.

———. 1991. *The Structure of Sociological Theory,* 5th edition. Belmont, CA: Wadsworth.

———. Forthcoming. "The Promise of Positivism." *In Postmodernism and General Social Theory,* edited by S. Seidman and D. Wagner. New York: Basil Blackwell.

Turner, R. H. and L. Killian. 1987. *Collective Behavior,* 3rd ed. Englewood Cliffs, NJ: Prentice-Hall.

Turner, S. P. 1988. "The Strange Life and Hard Times of the Concept of General Theory in Sociology: A Short History of Hope." Paper presented at the Albany Conference on General Social Theory and Its Critics: Contemporary Debates. Albany, NY, April 15 and 16.

Turner, S. P. and J. H. Turner. 1990. *The Impossible Science: An Institutional Analysis of American Sociology.* Newbury Park, CA: Sage.

Wagner, D. G. 1984. *The Growth of Sociological Theories.* Beverly Hills, CA: Sage.

———. Forthcoming. "Daring Modesty: On Metatheory, Observation, and Theory Growth." In *Postmodernism and General Social Theory,* edited by S. Seidman and D. Wagner. New York: Basil Blackwell.

Wagner, D. G. and J. Berger. 1985. "Do Sociological Theories Grow?" *American Journal of Sociology* 90:697-728.

Wallace, W. L. 1983. *Principles of Scientific Sociology.* Chicago: Aldine.

Wiley, N. 1979. "The Rise and Fall of Dominating Theories in American Sociology." Pp. 47-79 in *Contemporary Issues in Theory and Research,* edited by W. E. Snizek, E. R. Fuhrman, and M. K. Miller. Westport, CT: Greenwood.

———. 1985. "The Current Interregnum in American Sociology." *Social Research* 51:179-207.

———, ed. 1987. *The Marx-Weber Debate.* Newbury Park, CA: Sage.

Winch, P. 1958. *The Idea of Social Science and Its Relation to Philosophy*. London: Routledge & Kegan Paul.

Zetterberg, H. 1985. *On Theory and Verification in Sociology*. New York: Free Press.

Zimmerman, D. H. and L. Wieder. 1970. "Ethnomethodology and the Problem of Order: Comment on Denzin." Pp. 287-95 in *Understanding Everyday Life*, edited by J. Douglas. Chicago: Aldine.