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THE DISINTEGRATION OF AMERICAN SOCIOLOGY

Pacific Sociological Association 1988 Presidential Address

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WHAT IS DISINTEGRATION?

What do I mean by "disintegration"? The term is used here in both an evaluative and a more technical sense. Its evaluative connotations are that American sociology is "falling apart" and losing organizational as well as intellectual coherence; and most importantly, it has lost its vision as a science of human organization. Indeed, like the American economy in general, we are now consumers in a big market that imports more than it produces; and in fact, we accept almost any trendy idea from Europe as "the latest thing," much like we buy overpriced European cars. True, we can generate quantitative techniques with the best of them—the IBM of American sociology. But we have lost our vision of what sociology is, what it should produce, and what its goals are.

American sociology is now big, disorganized, incoherent, and increasingly boring. When I was an undergraduate in the first half of the 1960s and a graduate student in the latter part of the 1960s, sociology was still exciting. There seemed to be real promise that theoretical unification could occur, that we would develop cumulative knowledge, and that we could take our place at the table of science. This theoretical euphoria was accompanied by even more enthusiasm for the methodological advances of the field as it harnessed the linear model to the computer. Equally significant, all this scientism was accompanied by a profound sense of our relevance to the real world. Indeed, there was a conviction that our scientific knowledge could be used to solve the enduring problems and dilemmas of American society. Was I just naive, or was the profession simply drunk on its growth and expansion in the turbulent 1960s; and have we now sobered up the morning after our big fling? If this is the case, American sociology was far more interesting as a swaggering and confident drunk than it is as a contrite profession today.

This is my argument in evaluative terms. Let me be a bit more sociological and state the argument on disintegration more technically. In the context of any intellectual (including scientific) enterprise, what would we mean by "integra

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tion"? An answer to this question will provide a more neutral way to assess the level of disintegration of American sociology. I visualize integration as a situation where there is a high degree of mobilization and control over resources, particularly symbolic, material, and organizational resources. That is, a field is integrated when it can mobilize symbolic, material and organizational resources, while at the same time controlling and regulating how these resources are distributed and used.

By symbolic resources, I refer to the capacity of a discipline to display stores of accumulated knowledge, to maintain common definitions of important problems, to agree upon relevant procedures, and to develop (in the case of sciences) theoretical principles about crucial processes. And, it must evidence practitioners who: (a) see themselves as members of the same community; (b) share common intellectual goals; (c) utilize certain discursive forms; and (d) agree upon standards and criteria of adequacy. By material resources, I refer to the ability of a field to mobilize and control the monies, clients, facilities, equipment, and labor needed to conduct intellectual activity. For academic fields, the most relevant material resources are research funds, university facilities, and student bodies. By organizational resources, I mean the success with which a discipline (a) can develop coherent patterns of structural interconnection and mutual dependence among its members; (b) create mechanisms of decision-making, administration, and control over its members; and (c) implement effective means for reproduction of members.

THE DISINTEGRATED STATE OF AMERICAN SOCIOLOGY

In a sense, the above portrayal of integration is an ideal type. It is not only unrealistic but perhaps it is also draconian. Yet, it gives us a baseline, or yardstick, by which to measure American sociology's level of integration. Using this ideal type, American sociology is clearly disintegrating. Symbolically, it has no accumulated body of knowledge that goes unchallenged; it reveals no real consensus over important problems; it can articulate few abstract laws, and even those that are articulated will be challenged; and its practitioners do not see each other as members of a consensual community, nor do they share goals, procedures, discursive forms, or standards of adequacy. Materially, sociology has lost students, research funds, and clients. And organizationally, sociology has lost members, while proliferating so many sections, specialty associations, journals, and subfields that structural connections, control, and administration are loose, ad hoc, and haphazard. And even in the reproduction of its members, sociology produces such an eclectic mix that relatively few of them have anything in common; those who do share a commonality isolate themselves in network cliques and speciality associations.

While many will applaud the current situation as offering freedom to "do your own thing," as fostering a healthy eclecticism, and as encouraging exciting debate and discourse, I see this situation much differently. Sociology will not, of course, disappear under these disintegrative forces, but neither will it be important within academia as a whole and, more significantly, outside academia in "the real world." A discipline that has little symbolic and organizational coherence and that has a poor material resource base is not going to have a great impact on the world. In contrast, psychology, political science, and economics are better organized, more symbolically unified, and materially sounder disciplines; and despite the fact that their knowledge is narrow and often wrong, they have been far more successful than sociology in influencing policy-decisions in the real world. Sociology has, in a very real sense, deeded over areas to practitioners of disciplines who are far less knowledgeable and qualified than we are.

Sociologists want recognition, prestige, money, students and influence, but we do not want to organize, symbolically unify, and materially endow ourselves in a manner that can make sociology a more potent force. We prefer, instead, to renounce those who see themselves as scientists; and we embrace every trendy idea, from the latest Marxist diatribe to the most vague Franco treatise. We tolerate just about any point of view, no matter how imprecise and noxious. We proliferate a new organization, section in ASA, or journal as a means for resolving our differences. And we are colossally smug about all this, even as we whine about our lack of recognition. Sociology cannot have it both ways. We cannot be a big brokerage confederation of very different thinkers and, at the same time, be taken very seriously. We can lobby, stamp our feet, and shout until the roof caves in, but few will take us seriously until we return to our original vision: to be a science that can be used to construct more humane patterns of social organization. Symbolically, a commitment to science rather than ideology, smugness, and tolerance of any thought is the key to revitalization, but this commitment will occur only if we recognize ourselves in ways that most will not like—that is, hierarchically, and with far greater control over sociologists' activities. And we will not secure funds in great quantity, nor really top-notch students, until we recommit to science and reorganize in a less differentiated and decentralized manner. The very fact that this sounds so impossible and, for many, repugnant attests to my belief that American sociology is disintegrating—in both the technical and evaluative sense.

How did this come to pass? Was Comte really a madman when he saw sociology as a "social physics"? Were the first American sociologists, who quoted Comte (1830–1842) and Spencer (1874–1896) at length, "naive" in their faith that sociology could be a science? Let me review, most cursorily, the history of American sociology in order to see why we have come to the present state of disintegration. Perhaps there is a lesson in this history which we can use to reconstruct American sociology. But equally possible, there is the potential for seeing a fait accompli.

ON THE PATH TO DESTRUCTION

A Short History of American Sociology

The problems that confront current American sociology today are not new; they have existed since the forging of the discipline, although they were less visible. In broad strokes, my argument is that sociology never had a secure resource base and that this base constantly shifted; and as a consequence, the discipline

was never able to consolidate itself symbolically, organizationally, or materially (J. Turner and S. Turner In press; S. Turner and J. Turner In press). Coupled with this general situation were some additional factors that help explain sociology's history: the dominance of two departments, Columbia and Chicago, for much of sociology's early history; the effects of the Depression of the 1930s on recruitment; the curious age structure of the discipline before World War II that allowed very young scholars to assume leadership roles as the funding generation suddenly died or retired; and the unprecedented but short-lived infusion of material resources, students and money, during the fifteen years after Sputnik. Let me fill in the details by outlining events in various historical periods.

Before World War I

In the 1860s, there was no academic tradition in American sociology, although one or two courses with titles such as "the philosophy of social relations" could be found before the Civil War. As a result, none of the founders of American sociology—Lester Ward, William Graham Sumner, Franklin H. Giddings, and Albion Small—had any sociological training within academia, nor had any of them prepared for an academic career at all. In somewhat different ways, these founders and others who would become the first American sociologists in the 1880s and 1890s were influenced by the various reform movements of the post Civil War period. These movements, which have no counterpart today, revolved around a problem—vice, divorce, public drunkenness, unemployment—and the belief that these problems could be overcome through public crusades of edification, legislation, and regulation. Partly in response to this reformist mood, many states established bureaus of "labor research" which provided research-oriented jobs for those who would be sociologists. But most of these reform movements had ties to organized religion; and so, as these movements made appeals to social *science*, and inevitable tension emerged. The first books and articles on sociology were thus imbued with a heavily journalistic dialogue among religion, education, and science. The proto-sociologists of this early period were imbued with a reformist ethic, but at the same time, they were seeking to be more scientific (Bannester 1987; Bernard and Bernard 1965). But the resource base of the earliest sociology in the 1880s was the reform movement which provided sociology with not only the money for research and the clients for its findings, but also with the legitimating symbols for the lay consumers of sociology.

As sociology began to enter academia, therefore, it took over many of the older "moral philosophy" courses and the ameliorative leftovers from other disciplines which did not want to be so involved with reformist impulses. Thus, sociology's earliest students were driven by ameliorative goals; and to this day, this has always been the initial appeal of sociology to most students. But in securing this resource base in academia, sociology became a kind of residual discipline, taking in the moral cast-offs from other disciplines, and dependent upon religious-based reformism and problem-oriented government bureaus of labor statistics.

To unify this mix of resources, early sociologists espoused Comtean and

Spencerian scientism (Hinkle 1980). Indeed, all of the early treatises on sociology in America were epistemologically Comtean: They sought to discover the laws of human organization through theory and observations; then use them to remake the social world. In terms of substance, Herbert Spencer was clearly the dominant figure; and since Spencer was enormously popular with the lay public before World War I, the use of his ideas provided additional symbolic legitimacy inside and outside of academia. Thus, early academic sociology involved a curious mix of positivism, organicism, implicit functionalism, evolutionism, and interventionism/reformism. The material base for the discipline—money and students—was reformism; and the trick was to reconcile this resource base with science—a reconciliation that was never to be successfully achieved.

This was also a very unstable resource base. Soon, reform edification lost its punch; and municipal surveys by various governmental bureaus of labor statistics declined. Seeing this vulnerability, the early sociologists sought to mobilize their organizational resources. There were many repressed conflicts among these early founders who all had very different backgrounds, but in 1905 they created the American Sociological Society (ASS) by repressing their intellectual disagreements. They saw the need to organize since the other social sciences had already created professional associations. And so, less than 200 initial members created the society which was to include all who were interested in the field, except for the Christian sociologists who had been among the earliest supporters of sociology but whose ambivalence toward science pulled them farther and farther from the emphasis of the founding compromise: positivism, organicism, and evolutionism coupled with an emphasis on survey methods and tabular presentations (Converse 1987). These symbolic elements were, as I will argue, insufficient to unify the discipline. While the ASS grew to reputable size (1000 members), on the eve of World War I it was still very weak organizationally for several reasons: its members had sources of prestige in other associations, such as the American Statistical Association and other social science associations; two departments—Chicago and Columbia—were far stronger than the Society, most members of the Society did not hold Ph.D. degrees and were so widely diverse in their qualifications (a situation that continued after World War I with very irregular patterns of Ph.D. production which, later, was to create sudden opportunities for very young scholars to become prominent with the death and retirement of the founding generation and their first cohort of students); and perhaps most importantly, the repressed disagreements over the nature of sociology as a science and its uneasy reconciliation with reformist impulses of students, clients, and the public made the Society vulnerable to internal conflict and sporadic compromises with external sources of symbolic and material resources.

Between Wars, 1920–1945

The period between World Wars I and II involved very dramatic changes for sociology. The grand macro-evolutionary thinking of Spencer disappeared, enabling the young Talcott Parsons (1937: 1) to proclaim near the end of this

period: "Who now reads Spencer?" In its place merged a much more focused and narrower form of theorizing on specific topics—a kind of theorizing that Merton (1949) was to legitimate as "theories of the middle range." A kind of "social action" metaphor guided much of this narrow theorizing, but this could hardly be considered a unifying theory. Indeed, it appears to have been more of a response to the rise of American social psychology under the impact of methodological concerns with measuring "attitudes" (Bulmer 1984). Quantitative methodology thus became increasingly focused on survey methods—sampling, scaling of "attitudes" and other cognitive states, and tabular presentation of data. The use of historical data virtually disappeared, and even the ethnographic tradition at Chicago began to decline. Organizationally, sociology began to grow and differentiate as the regional associations emerged and as other associations, such as that for rural sociology, split away from ASS and/or formed separate organization bases. And the hegemony of Chicago and Columbia declined, thereby encouraging further differentiation of the field.

Much of this change was the result of changes in sociology's material base. The most important source of funding for sociology were the private foundations, most especially those controlled by the Rockefeller family. One such avenue of Rockefeller patronage—the Institute for Social and Religious Research (ISRR)—encouraged the spread of quantitative research under the guise of supporting "realistic" studies rather than "speculative theory" and "moralizing." The products of the ISRR—dissertations, books, articles—equated "scientific" with "statistical." Rockefeller money also supported the Social Science Research Council (SSRC) as well as the Local Community Research Committee (LCRC) at Chicago.

When John D. Rockefeller, Jr. suddenly withdrew support for the ISRR, the vulnerability of sociology to the funding whims of a single individual, or board of foundation directors, was exposed. Perhaps equally important, other Rockefeller monies did not lead to a coherent set of cumulative findings, a unifying theoretical framework, or a clear research protocol. For example, the LCRC at Chicago never produced a coherent and, perhaps more significantly, replicable research or theoretical tradition. Indeed, Robert Park, who was clearly the leading figure at Chicago during the 1920s (Mathews 1977), personally molded the reformist interest of students into a distinctive kind of sociological curiosity, but this did not involve a clear theory or a research protocol that could be replicated at other universities or at subsequent points in time (Bulmer 1984; Kurtz 1984). And when William Ogburn arrived at Chicago in the late 1920s, a new kind of accommodation between quantitative sociology in the Columbia mold (which had evolved under the lead of Franklin Giddings) and the more qualitative "Chicago tradition" was made—making it unclear as to what sociology was to be. As a result, the dominance of Chicago did not lead to either control or even a model that other universities could easily replicate—except for the quantitative portions of the Chicago program. And when even Chicago could not sustain a consistent source of funding and when it became clear that there was little market for its products in either the lay public or government, one of

the best hopes for a coherent and intellectually unified discipline was greatly compromised.

The Social Science Research Council (SSRC) also failed to define the discipline despite very conscious efforts to do so. For example: the SSRC sought to support interdisciplinary work of some policy relevance; it sponsored conferences of scholars to define and determine a scientific approach to sociology; and it created fellowships for students willing to pursue scientific and yet policy relevant work. The end result was that only a quantitative bias emerged out of SSRC efforts. No theoretical synthesis emerged, and non-quantitative sources of data were driven further to the margins of the discipline.

Thus, from this private funding, which in the 1920s and 1930s constituted the bulk of sociology's material base, the discipline failed to become symbolically unified. There was no accepted body of theory; there were no cumulated "facts"; and there was no clear resolution of the science vs. policy (or science vs. reform) tension of the earlier period, except that most sociology students had been stripped of their earlier moralistic and journalistic propensities (see, for example, Lundberg 1947; Lynd 1939).

At the organization level, sociology did significantly penetrate academia; and it grew organizationally, with membership in ASS reaching 1,500 on the eve of the Depression. Membership dropped dramatically during the Depression, back to what it had been before World War I (about 1,000 members), but the composition changed toward a more academic profile, as non-academics dropped their membership. Yet, as the regional associations emerged in the late 1920s and 1930s, membership was widely dispersed, particularly since the percentage of joint memberships between ASS and the regionals was less than 30%. Moreover, the dominance of Chicago and Columbia declined—as is evidenced by the fact that from 1918 to 1928, the Ph.D. theses written at these schools declined from 72% of the total to 33%, and by the dissociation of the ASS's ties to Chicago's *American Journal of Sociology*. While the five "most distinguished" departments in 1934—Columbia, Chicago, Minnesota, North Carolina, and Wisconsin—controlled the symbols associated with prestige, this control did not translate into high degrees of structural interdependence, since they did not swap a high proportion of their Ph.D.'s. Indeed, the elite departments in the 1930s did not have placement records dramatically different from non-elite departments; rising departments could be built with Ph.D.'s from the star departments. In fact, the emergence of new departments, which did not model themselves in either the Chicago or Columbia mold, further differentiated the field intellectually and organizationally.

There were efforts at consolidation of power, control, and symbols, however. The Sociological Research Association (SRA) represented one such effort, involving an elite, invitation-only membership group of one hundred who would discuss the "problems" of the discipline. Much like the failed efforts of SSRC in its support of conferences among elites, SRA could not achieve consensus about how to produce scientific sociology. And, contrary to its goal of consolidating the discipline, it divided sociology into elite and non-elite factions—thereby

further splintering a field already fragmenting as new regional associations, new departments, and new specialty associations were emerging.

This organizational fragmentation was accelerated by the lack of symbolic unity in the profession. No general theory comes to us today from this period—except for human ecology (which is simply a downsized version of Spencerian sociology) and symbolic interactionism (and perhaps for vague pronouncements about social action). What did come to us was quantamania, as survey research and attitude scaling became the dominant research tools of American sociology. Moreover, the science vs. policy question resurfaced in frequent debates and diatribes revealing that the reform sentiment of the early period still stood in tension with sociology's efforts to become a science. Science, however, was increasingly viewed in statistical rather than in conceptual (or mathematical) terms. Thus, the efforts at symbolic unification under the banner of science divided as much as it unified the discipline as it entered World War II.

The War and Its Aftermath

Immediately after the war, there was an infusion of material resources into American sociology. Student enrollments increased dramatically; and by 1950, almost 8,000 bachelor degrees were being awarded yearly—a figure about equivalent to psychology at this time. The production of Ph.D.'s increased, though somewhat haltingly, to about 150 per year. In addition to these demographic resources, there was a renewed burst of private foundation funding. Fresh from wartime success in "proving" the worth and utility of sociological research, a network of connections composed of private foundation officers and largely private universities, particularly the Ivy League, emerged. Somewhat later, peer review and, increasingly, governmental sources of money were to replace private foundation funds as the major source of funding for sociologists. But the foundations, which competed with each other to fund "significant" research, were the force behind the strong departments developed at Harvard, Chicago, Columbia, and other elite private universities. Some of sociology's most famous works—*The American Dilemma, The Lonely Crowd, The Authoritarian Personality*—were published with foundation money; and the research that led to the rapid production of Parsons' functional theory in the early 1950s was funded by foundation money. There were also monies from the armed services, which would eventually fund considerable research in the 1960s, although most of this money went to public universities. Moreover, sociologists began to lay claim to internal research funds within the university, as was the case at Wisconsin and other midwestern schools. And, anticipating what would be a massive infusion of money in the 1960s, sociologists began to get significant amounts of money from the National Institutes of Health and the National Institute of Mental Health, although social science was still excluded from National Science Foundation funds in the early 1950s.

These material resources encouraged an expansion of organizational resources. Membership in the ASS increased dramatically, going from less than 1,500 in 1945 to almost 7,000 by 1960. More significantly, there was a movement

within ASS to consolidate organizationally and to rework ASS into a large, powerful, and effective professional organization (Parsons 1959). Along with these efforts by Talcott Parsons, Everett C. Hughes, Robert Merton and others at organization consolidation, the SSRC also engaged a systematic effort at symbolic unification by developing programs in universities to increase the mathematical and statistical competence of graduate students.

For a time, these organizational initiatives were paralleled by what appeared to be symbolic unification: sociology was to be a science; functional theory, particularly the Parsonian variety, was to integrate conceptually the many specific subfields of sociology; and quantitative methodology, as advocated by scholars like Paul Lazarsfeld and Samuel Stouffer, was to be the dominant mode of research. Indeed, except for the brief period at the founding of the American Sociological Society (ASS), the discipline never appeared to be more unified. True, there were critics, such as Herbert Blumer (1969) and C. Wright Mills (1959), but they did not constitute a significant voice until the 1960s when this apparent intellectual unity began to unravel. The reasons for this unraveling are partly intellectual: functional theory and quantitative methodology were somewhat strange bedfellows; functional theory had a large number of fatal flaws (see Turner and Maryanski 1979); non-quantitative methodology never ceased to pose a challenge to quantamania; Marxist alternatives were soon to exit their closet existence imposed by McCarthyism; European intellectual traditions were beginning to re-invade American sociology; and the science vs. practice tension, which had not been resolved, was about to take on a whole new dimension with the reflowering of Marxism and the importation of European critical theory. But more fundamentally, sociology had a weak organizational base: no clear professional standards, save for statistical competency; no central administration and control of sociological activity; no capacity to control credentialing of sociologists; no clear market or clients for sociologists outside academia; no real control over journals, save for the few published by the ASS; no control over the funding of research, teaching of students, or hiring of professionals; no organizational rewards of any great significance (especially material ones) to offer its members; no clear capacity to regulate regional associations; and no ability to stop the proliferation of alternative associations and journals. Thus, as sociology approached its Golden Era, it was organizationally unprepared to deal with the dramatic increase in its material resources.

The Golden Era and the Portents of Decline

The post-Sputnik era, fueled by the National Defense Education Act, increased spectacularly the financial resources available to academia. Coupled with a newfound concern with social problems, especially as these were highlighted by protests over the Vietnam War, the material base of sociology increased to what would have been unthinkable proportions a decade earlier. Money and students were flowing into sociology. For example, while private foundation support for the social sciences decreased between 1960 and 1980 in constant dollars, govern-

ment support increased seventeen times over from 1956 to 1980 (from $30 to $524 million); and, coupled with a 27-fold increase in internally generated funds within the university, there was now abundant money for faculty recruitment, research, and graduate student support. For sociology, federal funds for all programs inside and outside academia went from about $10 million in 1960 to over $120 million by 1971, creating opportunities for research within colleges as well as in private research firms. As a result of increased student interest in sociology as the most relevant social science for the heightened social and political awareness of the 1960s, new academic markets for professors were created. For example, sociology bachelor degrees went from less than 8,000 in 1960 to over 36,000 in 1971; and correspondingly, Ph.D. production to teach these students increased from around 150 in 1960 to almost 750 by 1972. Similarly, masters degree production surged from less than 500 in 1960 to well over 2,000 by 1971.

Organizationally, ASS membership stood at less than 7,000 in 1960, but by 1971, it was almost 15,000. Moreover, the old American Sociological Society (ASS) became the American Sociological Association (ASA) in 1963, marking a change toward a full-time staff to handle the professional affairs of sociologists. While very modest in 1963, the staff and programs of ASA all increased rapidly during the next decade, as did the membership dues to pay for these organizational changes. Formal ties to regional associations were soon terminated (Pease and Hetrick 1977); and the ASA embarked on a strategy of expansion of its membership base, often at the expense of regional associations. Of particular significance, the ASA embarked on a co-optive program of expanding sections and publishing journals for potential or actual constituents. But this growth and differentiation was not accompanied by increased organizational control. Rather, the ASA simply sought members and tried to accommodate them with a section, journal, or session at the annual meetings. The result was that as the ASA grew rapidly, it differentiated in ways that further fragmented the discipline into distinct subfields. While the ASA may have desired to create a powerful organization which could lobby Washington and certify sociologists for the academic and non-academic markets, it did just the opposite. Instead, it created a very weak brokerage association, seeking to compete for members with the regional associations and the rapidly growing numbers of specialty associations (Rhoades 1981).

Growth not only stimulated organizational differentiation within the ASA, it increased the number of journals (now well over 200) accepting sociological research, the number of specialty associations (well over 100), and the number of non-academic research organizations (uncountable). Thus, not only was the ASA becoming internally fragmented, but the situation outside of the ASA was much the same. And, eventually, the existence of alternative journals and organizational bases would begin to determine the ASA's capacity to exert even limited organizational control over the discipline.

Growth and organizational differentiation also encouraged intellectual fragmentation. There are now some 50 subfields listed as graduate specialties in the ASA's official publications, although this number is probably a low estimate

because some of these have further partitioned themselves—creating subfields within subfields. More significantly, in the wake of the brief period of functional hegemony, theoretical sociology splintered into a series of "perspectives" or "orientations"—functional and neo-functional, interactionism, conflict, exchange, structural, structuralist, and phenomenological. And these general orientations further differentiated specific varieties—for example, role theory, Chicago symbolic interaction, Iowa interactionism, dramaturgy, Marxist conflict theories, Weberian conflict theories, world system adaptations of Marx, critical theory, behavioral exchange theories, utilitarian/rational choice theories, conflict exchange theories, evolutionary exchange theories, systems theories, network theories, ethnomethodological theories, and so on. These general orientations and their specific varieties often view each other with antagonism; and as a result, the unifying force of any intellectual discipline—theory—is simply incapable of providing any guidance to sociology. Indeed, with a few notable exceptions, there has now been a significant split between theory as a whole and research, with each going its own way (Wells and Picou 1981). In fact, metatheory has emerged as a way to budge the divisions within theory, or at least talk about them. But, as with specific theories, metatheorists are differentiated into various camps.

Even quantitative methodology cannot provide much intellectual integration (McCartney 1970). True, quantitative methodology dominates, but it is raked by a certain faddishness which, increasingly, many sociologists are unwilling to pursue (who wants to learn the latest technique, only to have it go out of fashion?). More importantly, there has emerged a real sense of disillusionment with quantitative methodology which has yet to demonstrate that it is vastly superior to alternative ways of examining the social world. In fact, as one critic has argued, statistical analysis has produced far fewer intellectual breakthroughs in our knowledge than alternative procedures (Collins 1984). Whatever the merits of such arguments, the euphoria of the 1960s over what the linear model and its variants could accomplish is considerably dampened; and while quantamania held sociology hostage until the mid-1970s, it's influence has decreased considerably. The result is for one more basis of integration—perhaps a dubious one—to be undermined.

In addition to these intellectual questions, other symbolic issues remained unresolved during the 1960s and 1970s. In particular, the relation between science and amelioration was, as it always had been, a potentially disintegrating force. Research monies and student demand were, to a great extent, tied to sociology's promise to say something important about social problems and issues, but the prestige system favored those who contributed to science—however defined. Thus sociology's material base depended upon amelioration, as it had from the inception of the discipline in America, but elites have tended to advocate various brands of scientism. Such advocacy became ever more difficult to sustain as the ASA opened its doors and created sections for everyone and as new journals and speciality associations proliferated. And as a result, there has been no integration of these symbolic poles around which sociology has sought resources, legitimation, and public support. Thus, sociology remains

uncertain about its basic mission: science vs. practice, or some combination of the two.

The Big Fall

In the early 1970s, points of intellectual cleavage became ever more evident as the faith in quantitative methods declined, as theory splintered into camps and subcamps, as the science vs. practice issue remained unresolved, and as more substantive subfields could be identified. These sources of potential disunity became especially more acute as resources became scarce: pure research money began to dry up; student enrollments declined dramatically, as is evidenced by the fact that bachelor degrees went from about 36,000 in 1971 to less than 15,000 today; public interest in social issues declined, making it less receptive to sociology; private research organizations proved more effective at simple data gathering than academics, or at least better able to write contract grant proposals; and ASA membership declined from almost 15,000 in 1971 to less than 11,000 in 1984 (although it is back up to about 12,000 today).

This contraction of resources was not accompanied, however, by a consolidation of organizational control, by a clarification of sociology's goals, by a renewed commitment to a set of methods, or by a theoretical renaissance like that ushered in by Parsonian functionalism. Part of the reason for the failure to retrench was increased competition for limited resources. Another, and more important, reason is that sociology was already too fragmented, making it impossible to put Humpty Dumpty back together again. Indeed, there was little motivation to integrate the discipline because most sociological activity could survive on a restricted resource base (except, perhaps, big grant survey research). For as long as each subfield could sustain its courses in the curriculum, maintain a specialty association or informal network of like-minded colleagues, support a second or even third level journal, and secure a section and/or sessions from the ASA and regional meetings, there was no reason to reintegrate organizationally the substance of American sociology. Further, as long as theory continued to splinter into perspectives and subperspectives, there was no intellectual "force" that might pull sociology back together. And, as quantitative methodology lost much of its material base in academia (when monies dried up, or when they were grabbed by private research companies), there was no common research protocol that could guide the field.

Such is the state of American sociology today: differentiated and still differentiating on a much smaller resource base with little to pull it together. What does this trend foretell of our future? Let me provide my answer in a few concluding remarks.

CONCLUSION

For many—perhaps most—sociologists, there is nothing wrong with the current state of affairs. Indeed, while many sociologists espouse collectivist-regulatory solutions for other people's social problems, they are rugged individualists and the ultimate 19th century liberals about matters closer to home. They want to do

"their thing" in "their own way"; and any effort to constrain them produces howls about restrictions on "academic freedom." I must confess that my own personal and ideological biases lie in sympathy with the profession on this score. But when I put on my "sociological cap" and examine what such "liberalism" legitimates, I become somewhat dismayed. Let me elaborate.

First, such liberalism legitimates many varieties of anti-science and, in so doing, cuts us off from important resources—the merger of science and amelioration as both a base for material support and public legitimation. Comte was correct in this sense: social problems can be effectively addressed by sociology as a science which can accumulate knowledge through tests of theoretical ideas. Other kinds of efforts to deal with social problems are based upon ideological commitments, guesswork, and extrapolation from past experiences. Thus, I have never seen a real conflict between amelioration and science—but the discipline has. And we are now organized in ways that preclude our capacity to address social problems with any degree of effectiveness. We have just organized another atheroretical section and journal to deal with these "practice" issues. We are thus structured in ways that institutionalize self-flogging and foot-shooting.

Second, liberalism legitimates chaotic organizational forms that preclude our ever being an effective force inside or outside academia. Few of us would accept a well-integrated discipline, however, because integration would involve: (1) hierarchical control of financial resources, of credentialling, and of access to publication outlets; (2) dense networks of interconnection and mutual dependence among professions for resources; (3) accepted theories that guide discourse; preferred or even mandated research procedures; (4) powerful and constraining values emphasizing a commitment to science; and (5) rejection of political-ideological intrusions into scientific discourse; and so on (Fuchs and Turner 1986). In a sense, the issue is moot because the discipline is now so organizationally fragmented, symbolically diverse and cut off from its traditional legitimating symbols (science and social problems), intellectually chaotic (virtually anything goes), and materially weak that we will never achieve the state of affairs at the founding of the ASS in 1905 (precarious as it was) or the brief period of functional-quantitative hegemony of the 1950s and early 1960s (incomplete and incorrect as it was).

Sociology will, of course, survive and perhaps even prosper during periods of renewed public interest in social problems. But each time we are given the chance, we will fail to impress the public, students, and decision-makers, because we will not be able to give them a coherent set of ideas or guidelines.

Our future is for sociology to muddle along and be a label for very different kinds of practitioners who march to the beat of distant and different drummers. Some subfields will spin off from sociology and develop their own professional identity—as criminologists have done, or as network sociologists are now doing. In a sense, it would not be a bad thing for sociology to simply disintegrate into specialty associations or to merge into other interdisciplinary programs. Only in this way could sociology rise again and realize the promise that Comte saw for a natural science of society. This will not occur because sociology is now too well

established as an academic discipline in colleges and universities. And so, we will all just have to plod along with fellow intellectual strangers and just "do our own thing." This is, of course, not a bad lifestyle, but it is not what will make sociology a force in a world that desperately needs a mature and well integrated science of society.

Acknowledgements: This article represents my conclusions, stated too polemically, that have emerged from two collaborative and only just completed books with Stephen P. Turner who, I should add, does not fully share my viewpoint. See our *American Sociology: Its History, Structure and Substance;* and *The Impossible Science: An Institutional History of American Sociology.*

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