

lems. The sample size is not stated in every table and is variously reported as 103, 100, and 95. Stating the total *N* used in each analysis could no doubt explain these discrepancies. A strong case is made that the three groups, being different linguistically and ecologically and having been "assiduously" controlled for "demography, income, gender-role expectations, form of governance, and the influence of outside change-agents," practice different cultures. But since all have been under Spanish Catholic influence for nearly five centuries, Galton's problem may not have been completely accounted for. This reader would have preferred much fuller discussion of the results, and perhaps a more detailed attempt to place the research within the stream of culture-bound-syndrome studies. Rubel and associates insist that susto

is a "clinically distinct syndrome," since their research "demonstrates how cultural and disease processes interact to form an entity unfamiliar to cosmopolitan medicine." This conclusion requires much more explication and theoretical analysis.

Nevertheless, the study is a model of its kind and points the way to the kind of biomedical research that is essential for clarifying the problem stated at the opening of this review. The question of whether there are culture-specific illnesses ultimately may prove to be unanswerable. But it needs to be addressed, and Rubel, O'Neill, and Collado have moved that effort a long step forward.

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## An Academic Preeminence

**The Chicago School of Sociology.** Institutionalization, Diversity, and the Rise of Sociological Research. MARTIN BULMER. University of Chicago Press, Chicago, 1985. xx, 285 pp., illus. \$29. *The Heritage of Sociology*.

**Evaluating Chicago Sociology.** A Guide to the Literature, with an Annotated Bibliography. LESTER R. KURTZ. University of Chicago Press, Chicago, 1984. x, 303 pp. \$22. *The Heritage of Sociology*.

In 1830, Auguste Comte proclaimed that a science of society could take its place among the physical and other natural sciences. Few listened to Comte's proclamation; and it was not until the last decade of the 19th century that sociologists penetrated the halls of academia. Even in Europe, where self-conscious sociological analysis first emerged, there were few academic sociologists and even fewer who saw themselves as research scientists. Research-oriented universities had existed in Germany for decades, and the founding of Johns Hopkins in 1879 marks the beginnings of the modern research university in America. But with the founding of the University of Chicago in 1890 research and graduate training were blended in a new, synergetic combination. So open and innovative was the University of Chicago that it allowed for the creation in 1892 of a small, fledgling department of

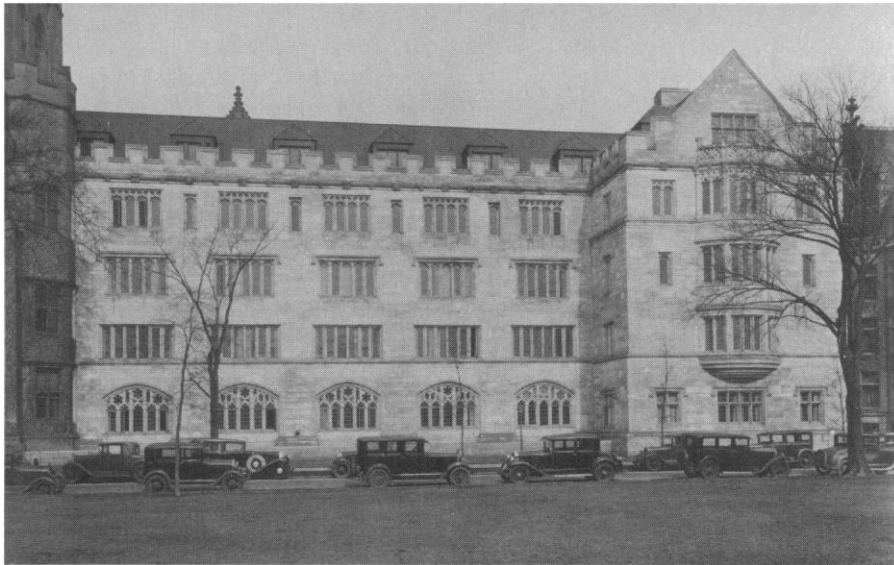
sociology under the leadership of Albion Small. Over the next four decades, the "Chicago school" was to dominate sociological inquiry in America.

Martin Bulmer's *The Chicago School of Sociology* and Lester R. Kurtz's *Evaluating Chicago Sociology* can now be added to the growing number of accounts of the Chicago school during its "golden era" between 1915 and 1935. They are both excellent books, with somewhat diverse purposes. Both summarize the substantive, methodological, and conceptual approaches of early Chicago sociologists and both give descriptive accounts of the institutional processes by which sociology became established; but they do so in different ways. The Bulmer book is the longer and more detailed and emphasizes the historical events that led to the ascendancy of the Chicago department of sociology. The Kurtz work is narrower and concentrates on the substance of the department's research program, providing a 150-page annotated bibliography of work about, or inspired by, the Chicago school. Yet despite the differences in approach I find the works similar in one important respect: both become so enamored with the success of the Chicago department between 1915 and 1935 that they fail to address the broader institutional questions: What are the conse-

quences for a nascent discipline when one department dominates inquiry during its early years? Is it necessarily good for the cumulation of knowledge that a single department can control such a large share of a discipline's funding, its graduate population, its political offices, its publishing outlets, and its network structure?

These kinds of questions draw attention to the politics of intellectual activity. For the ascendancy of the Chicago school was more than a simple intellectual blossoming; it was also a process in which one department gained power in its field. Bulmer and Kurtz both tend to ignore the fact that academic scholarship is also a political process. For virtually all organized intellectual activity involves competition among universities and their faculties, who often gain hegemony by producing paradigms that dominate the conduct of inquiry, at least for a time, and who exercise control over the flow of not only intellectual but also financial resources. Such processes need not be consciously implemented or particularly Machiavellian, but to ignore them is to miss much of what makes science a sociologically interesting phenomenon. And thus, as I reconstruct Bulmer's and Kurtz's historical accounts, I will draw attention to what these otherwise very good books ignore: the long-run consequences of the "Chicago paradigm" and its implementation through control of academic and professional resources.

Let me begin by describing the general academic environment in which the Chicago school and its paradigm for scientific sociology first emerged. At a time when Johns Hopkins was in a transitional period of retrenchment and other universities still emphasized undergraduate instruction, the University of Chicago was building strong graduate programs, primarily with the initial endowment of John D. Rockefeller, Sr. Though the city of Chicago was a rough and somewhat unseemly place in the 1890's, the university offered real research opportunities and was thus able rather quickly to assemble a strong faculty. Other elite universities had not yet turned to an emphasis on research and graduate training, and so Chicago was at a competitive advantage in the academic marketplace generally. And in sociology in particular, where there were virtually no academic niches for research sociologists, Chicago was favored in securing funding from the Laura Spelman Rockefeller Memorial for support of a small sociology faculty, a large graduate population, and an ac-



The Social Science Research Building at the University of Chicago shortly after its opening in 1929. [From *The Chicago School of Sociology*]

tive interdisciplinary research program. Chicago also, unlike the older eastern universities, had the advantage of enthusiastic support from a growing and prosperous local community that was involved in "its" university and through which additional funding was secured. Thus, despite its newness, the University of Chicago was ahead of other universities in securing research-oriented faculty, professional graduate students, and research funding.

Within the university itself, each social science department was small, and thus there was an emphasis on interdisciplinary work. The need to share faculty resources was escalated by a comparatively large population of graduate students, who were encouraged to take courses in a variety of departments. Leadership was also a critical variable. Albion Small, the department's first chairman, was unlike his more domineering counterparts at other universities; he visualized his role primarily as that of a facilitator and mediator of research activity rather than a fountain of truth. These circumstances generated an intense collaboration between faculty and students, who used the newly obtained financial resources to conduct research in the city of Chicago. Thus, collegial approval, professional prestige, promotions to higher ranks, and, to a lesser extent, faculty salaries were connected to research activity.

This orientation was coupled with some innovative organizational features. Chicago was the first sociology department to secure large-scale extramural research grants; it was the first to emphasize graduate over undergraduate

training and to use extramural research funds to support graduate students; it was the first to encourage team as well as interdisciplinary empirical research; it was the first actively to involve a professional nonacademic staff in the research process; it was the first to purchase expensive hardware for data processing; it was the first to have intensive graduate student seminars at which research results and research methods were emphasized; it created its own journal (the *American Journal of Sociology*) and made extensive use of its university press as an outlet for research findings; it founded both a general Sociology Club and a Society for Social Research for the open discussion of research findings and as a forum for guest scholars from diverse disciplines; it established a summer institute for former students; and it published (through the Society for Social Research) a newsletter and bulletin reporting on its activities.

These enterprises were revolutionary for their time. In a field in which the dominant mode of inquiry had been the lone scholar working with materials from libraries and archives Chicago created an infrastructure for collaborative and interdisciplinary empirical research. The organizational innovations made by the Chicago sociology department, along with its sister departments in psychology and political science, provided a model for other universities and departments to emulate; and they were what allowed Chicago to gain its influence on the profile and direction of sociology in America.

To convey the extent to which Chicago sociology came to dominate the field,

let me cite a few of the "achievements," as Bulmer and Kurtz view the matter, of this school. First, at its peak in 1925 one-third of all sociology graduate students were enrolled at Chicago; and as they radiated out and in turn produced students a powerful social network was created that was kept intact for many years through the summer institutes and bulletins from the department. Such networks are, of course, to be expected in all sciences, but this one, created in a virtual vacuum, exerted enormous influence in its field. Second, the department's journal was the first official publication of the American Sociological Society up to 1936 (when the society created the *American Sociological Review* as a response to the Chicago hegemony). Given in addition the vitality of the University of Chicago Press, Chicago students and former students thus had ready access to the most widely read and prestigious channels for disseminating information up to World War II (and to a great extent even today). And finally, as late as 1971 one-half of all presidents of the old American Sociological Society and the newer American Sociological Association were Chicago faculty or former students of the department.

This kind of control over professional resources reflects the achievement of the department's faculty and students and, on the one hand, it should be lauded as it is by Bulmer and Kurtz. But, on the other hand, such control of resources creates the potential for intellectual domination. Though the department's domination was far from complete, especially as Columbia and Harvard began to create competitive programs, the orientation of sociological inquiry at Chicago largely determined that of American sociology as a whole.

The research of the Chicago school is identified with an emphasis on field methods, relying primarily upon informal interviews, firsthand observation, personal documents, census tract data, and even newspaper accounts. W. I. Thomas and Florian Znaniecki's *The Polish Peasant in Europe and America* set the tone for this kind of research in 1918, but it was Robert E. Park and Ernest W. Burgess who pushed, prodded, and inspired students to go out into the city, which was viewed as a kind of laboratory. Bulmer sees this emphasis on field methods as a reaction against the "social survey movement," which, in Park's and Burgess's eyes, was too much concerned with amelioration and as a result was insufficiently scientific. It was much more scientific, they argued, to suspend humanitarian motives and

record the facts through firsthand observation. There is an irony here in that today field research is considered less "scientific" than survey methods in sociology and those who subscribe to field methods tend to be doubtful that sociology can be a science and to be oriented to ameliorative projects. For Park and Burgess, however, sociology could be a science by taking as its basis careful fieldwork. Though in their conception fieldwork was to be guided by a theoretical framework, the structure of the department encouraged research over theory. Sociology was increasingly viewed as a science only if it could produce large quantities of descriptive empirical data.

The atheoretical bias of Chicago sociology became even more evident as quantitative methods were increasingly emphasized in the late 1920's and early 1930's. Bulmer's and, to a lesser extent, Kurtz's analyses provide an important corrective to our retrospective view of Chicago sociology. Long before Paul F. Lazarsfeld and others at Columbia and elsewhere helped usher in the quantitative mania that still dominates sociology, Chicago social scientists were using the ideas of Karl Pearson and other English statisticians to perform quantitative work. For example, as Bulmer in particular documents, Burgess was the father of census tract analysis; and, along with Thurstone in psychology, Gosnell in political science, and Schultz in economics, William F. Ogburn, who was appointed to the department in 1927, carried out sophisticated statistical analyses. As their students radiated to other universities, these researchers were instrumental in the institutionalization of quantitative social science in America. Within sociology, Ogburn's students—Samuel Stouffer and Philip Hauser, to name just two—were to teach yet another generation of quantitative researchers and to perform multivariate statistical analysis before Columbia gained prominence in this area and furthered the "research over theory" paradigm in sociology. This shift in research orientation from field to quantitative and statistical analysis was hotly debated within the University of Chicago, and within the sociology department; in particular, Herbert Blumer often clashed with Ogburn. But in the end research took precedence over theory.

As Chicago came to the forefront of quantitative sociology in the 1930's, it became even less theoretical. Thomas, Burgess, and Park had all believed that, in principle, theory and research should be integrated, but Ogburn and his students were less interested in theory than

in the collection and correlation of "hard facts"—indeed, Ogburn was suspicious of abstract theory. And so as quantitative sociology was born in American sociology it had an antitheoretical bias—a bias that both Bulmer and Kurtz underemphasize.

This distrust of highly abstract and formal theory is, I feel, the main legacy of the Chicago school. Of the theory that does endure from Chicago's golden era, the greatest legacy, which Bulmer deliberately ignores because he feels it has received too much attention, is that of the philosopher George Herbert Mead. The human ecology perspective also survives, but it was near the Chicago school's decline in the 1930's that Louis Wirth ("Urbanism as a way of life," *Am. J. Sociol.* 44, 1–24 [1938]) reformulated Park's and Burgess's vague ideas, and it was not until after World War II, with the publication of Amos Hawley's *Human Ecology: A Theory of Community Structure* (1950), that human ecology became a systematic theory. Perhaps only in criminology did theory develop at Chicago, but in this instance it was subsequent generations working elsewhere who did the real theoretical work. Current theory of race and ethnic relations in sociology ignores, and for good reasons, Chicago school ideas. Contemporary theories of deviance owe their inspiration more to Mead than to anything the central figures in sociology produced. Moreover, the Chicago school tended to underemphasize two areas where theory is most developed in modern sociology—stratification and complex organizations. Both Bulmer and Kurtz make a noble effort in summarizing Chicago's theoretical contributions, but I find their arguments rather weak, because there simply is not much theory to summarize.

American sociology today is still a relatively atheoretical discipline. In getting scholars out of their armchairs and into the field I think Chicago arrested the development of abstract theory in sociology. In the name of being more scientific, it underemphasized the basic goal of all science: to develop abstract models and principles. As a consequence scientific sociology has great difficulty accumulating knowledge because it has so little systematic theory to guide research or to organize the vast quantities of data that have been collected.

What does endure from Chicago's golden era is an image of a discipline that must do research before it theorizes, that must induce theory rather than test it, that forces theorists to produce data and researchers to generate theory. Yet I suspect that could Auguste Comte see

what became of his positivistic dream for a science of society—a "social physics," as he preferred to call it—he would be disappointed. Along with others who have told the story of the Chicago school, Bulmer and Kurtz have only given us part of the tale and have emphasized only the positive portions of its legacy. For as long as sociology defines its scientific mission as quantitative analysis of large data sets it will remain an immature science. This is the negative legacy stemming from the fact that Chicago dictated the paradigm for scientific sociology.

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## Visions of Social Order

**Technological Utopianism in American Culture.** HOWARD P. SEGAL. University of Chicago Press, Chicago, 1985. x, 301 pp., illus. \$30; paper, \$14.95.

One striking characteristic of American society is its utopianism. We have been a nation divided between those who claim a special mission for the United States and those who lament the failure to live up to the promise of the new world. A nation that creates itself out of nothing, perhaps, cannot avoid thinking such grandiose thoughts. But if there is a powerful strain of thinking of America as paradise found or paradise lost, there is also another powerful element of utopianism that is less pretentious, occupying a sort of "middle landscape." This, as Howard Segal defines it, is the world of technological utopias, a peculiarly American variety of literature that flourished from the early 1880's to about 1933.

The technological utopians Segal identifies were some 25 authors who held a common vision of America's future. Writing in a period characterized by the substitution of mechanical power for human labor and the reorganization of work and living space that created modern America, they believed that more technology, better applied through better organization, would solve the glaring social problems surrounding them. Thus they take their place among thoughtful men and women who lived in a period in which optimists could hope for technological solutions to almost any problem.

Modest social critics that they were, the technological utopians occupied a