Criminology, the Chicago School, and sociological theory

JAMES F. SHORT, JR.
Washington State University, Pullman, WA 99164-4014, USA (e-mail: short@mail.wsu.edu)

Abstract. Although the Department of Sociology at the University of Chicago was never known as a center for sociological theory, major contributions were made in such areas as social disorganization, human ecology and demography, urbanism, professions, institutional development, community organization and development, as well as criminology and deviance. These theoretical contributions did not qualify as grand theory, but all were in the Chicago tradition of theoretically interpretive empirical work. The Project on Human Development in Chicago Neighborhoods – Chicago-style research at its best – continues that tradition, wherever it is practiced and whatever its specific aims.

Introduction: Theory and the Chicago School

Theory, at the time I entered graduate school at the University of Chicago, in September, 1947, was taught by Louis Wirth, in a sequence that included the historical roots of the discipline and exposure to a variety of contemporary efforts. One could always tell how well prepared he was for class by how much Professor Wirth lectured and how much he read from the works of theorists. More than one lecture consisted almost entirely of such reading, occasionally in German!

The Chicago department at that time took some pride in their collegiality, compared to a certain cauldron of conflict located in Cambridge, Massachusetts, where, we were told at the introductory gathering of what was then the largest class of new graduate students in the department’s history (remember, this was the peak of the “G.I. bulge” that hit many universities in post-world war II) that some Harvard professors would hardly speak to one another. We were soon to discover, of course, that this was also true of Chicago, where relationships between Ogburn, Wirth, Blumer, and Everett Hughes, and Phil Hauser were decidedly cool. (Readers may want to speculate as to the composition of the varying dyads and triads among those eminences!). Suffice to say that only the high intelligence and benign disposition of Ernest Burgess enabled the department to function with a modicum of collegiality. I refer you, however, to Andy Abbott and Emanuel Gaziano’s excellent

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chapter in Gary Fine’s book, *A Second Chicago School? The Development of a Postwar American Sociology* (Fine, 1995). The chapter’s title is significant: “Transition and tradition: Departmental faculty in the era of the second Chicago School,” for this was a time of conflict between the ideal of great ideas espoused by Chicago’s President, Robert Maynard Hutchins, and the department’s strong empirical tradition – not limited to quantification, please note – and intense self-examination by the department. The latter culminated in Ford Foundation funded series of seminars involving faculty and research associates in several related research centers.¹

The Chicago department was never known as a center for sociological theory; rather, its reputation was as a center for empirical research and theory about particular phenomena: delinquency, gangs, vice, suicide, family disorganization, institutions (read Everett Hughes, on the Chicago Real Estate Board, and Helen Hughes on the news and human interest, Kincheloe on churches – etc., anything having to do with urban structure, life styles, communities, and urbanism as such), etc. (for examples, see Short, 1970).

Entering the 1950s, the Chicago department had weathered the G.I. bulge and, although it was still a very exciting place to be, the senior faculty were either aging (Ogburn and Burgess retired soon after I left in 1951), ailing (Wirth died in 1951), or growing restless (Blumer and Reiss left in 1952, Hughes spent much time in Frankfort and Hauser virtually commuted to Washington, DC). The Department coped by bringing in a very young Leo Goodman and Nelson Foote; bringing back Dudley Duncan, and appointing recent graduate students Evelyn Kitagawa and Hal Wilensky. Later, Morris Janowitz returned, to spend the remainder of his life.

Several recent and current graduate students also taught in the College, along with such notables as Daniel Bell, Edward Shils, and David Reisman. Abbott and Gaziano (1995) discuss the free-floating intellectual character of the College, under Hutchins’ inspiration, as a source of animus between the Department and the College – as it was, apparently, between Hutchins and the Department. And they argue that the conflict “guaranteed that the department would not make a distinguished record in general sociological theory, precisely the kind of work represented by Riesman, Shils, and Bell in the Hutchins College” (p. 224). It did, however, serve to bring a conflicted faculty together “in their antipathy to Hutchins’s views.” (p. 224) Records of the seminars make it clear that general sociological theory was neither the Department’s major strength nor its ambition; that it was, in fact, contrary to what Chicago sociology stood for from the beginning.

Through both faculty and graduate students, Chicago did make major contributions in a number of theoretical areas, in addition to its solidly grounded empirical tradition: Social disorganization theory (Shaw and Mckay, Korn-
houser, later Sampson and Bursik); urbanism as a way of life (Everett Hughes wrote to me as I was compiling contributions to *The Social Fabric of the Metropolis*, that “Louis used to say all those things about how the city is impersonal – while living with a whole clan of kin and friends on a very personal basis”; Short, 1971, p. xxix); the family as a “unity of interacting personalities” (Burgess) (fortunately, that one didn’t get very far!), other family studies (the depression studies of Burgess and Cavan) and others who either worked with Burgess or followed him, such as Nelson Foote and Bernard Farber; and now Linda Waite; professions, institutional growth and development (Hughes; Kincheloe, Abbott); community organization and development (Warner, Janowitz, Ohlin, Suttles, Sampson), ecology and demography (Duncan, Kasarda, with Janowitz; Hauser, Bogue, and Kitagawa). And, of course, criminology and deviance (Reiss, Ohlin, Glaser, Howard Becker, and much more recently, Bursik and Sampson). Goodman’s major contributions were to come later, after he had left Chicago. None of these theoretical contributions qualify as grand theory. Nor, until Coleman’s late entry, did the theoretical contributions of the Columbia Ph.D.s who came to the department during the 1950s (Blau, Katz, Rossi, Barton, and Coleman). All were in the Chicago tradition of theoretically interpretive empirical work. So, also, was the work of several former Chicago graduate students with whom the department flirted regarding faculty appointment during the 50s and later (including Becker, Goffman, Ohlin, and Short).2

As an aside, when I returned to Chicago, in 1959, I found the department a much more exciting place than when I left in 1951. I thoroughly enjoyed the remaining old-timers (especially Everett and Helen Hughes, but also Phil Hauser, who was departmental chair at the time). The excitement was provided by interaction with people like Peter Rossi, James Davis, Elihu Katz, Peter Blau, Donald Bogue, Duncan Macrae, Morris Janowitz, and Fred Strodtbeck; also Jim Coleman who left shortly, and Harrison White, Mayer Zald, Phillip Ennis, and David Matza, as they passed through.3 An additional aside – but a relevant one – I inherited an M.A. candidate from Coleman when he returned briefly to Columbia from Chicago, in 1960. The student was Leon Jansyn. Jansyn observed that collective activities of the gang he was observing increased, seemingly in response to declining cohesion of the group; that is, following declines in the number of gang members’ “attendance” on the street. This was true of both delinquent and nondelinquent behaviors. For years Malcolm Klein and I argued over the meaning of this finding, since Klein had found that group cohesion, rather than its decline, was positively related to criminal behavior. A couple of years ago I finally figured it out – at least I think I did. The gang that Jansyn was observing was a local gang of long standing, with much tradition behind it. The gangs Klein was observing
– and we, too, in Chicago, for that matter – were relatively new gangs, not very cohesive to begin with, and in communities without long standing gang traditions. These differences, I believe, account for the differing observed relationships between Jansyn’s and Klein’s gangs. Although this hypothesis has not been replicated, it offers a possibly significant theoretical insight into the nature of deviant groups, and perhaps nondeviant ones, as well. One wonders, for example, whether this applies to the “pre-delinquent” phase of gangs; or the “pre-criminal orientation” of groups that, by Klein’s definition of gangs, later become gangs.

I could write at length about what was happening to the Chicago legacy during this period, and about variations of, and exceptions to, the “Second Chicago School,” in Gary Alan Fine’s terminology. More importantly, however, contrary opinions notwithstanding, I do not believe the spirit of the Chicago School ever died, or even significantly eroded. Most of the arguments to the contrary, I fear, reflect disappointment that the thrust of particular aspects of Chicago sociology failed to achieve dominance in sociology; and perhaps a bit of nostalgia for their own graduate school experiences. Each of the giants of the earlier Chicago School had adherents and, though there were similarities between them, they disagreed mightily on many matters (see Abbott and Gaziano, 1995). But Blumer’s approach to symbolic interaction failed to generate sufficient empirical research until people such as Ross Matsueda (a sort of Chicago School scholar twice removed by virtue of his tutelage by Don Cressey, himself once removed from Sutherland’s Chicago). The tradition of field observation and institutional analysis – the legacy of Thomas, Warner, and Hughes – has many successors among Chicago faculty and former graduate students (Janowitz, Gerry Suttles, and more recently, Robert Sampson and the departed William Julius Wilson). Quantitative methods did not arrive at Chicago with the influx of former Columbia graduate students. Ogburn and Hauser were there long before; and Duncan, Bogue, Kitagawa, and Goodman maintained that tradition along with the Columbia invaders. Concern with, and analysis of, the urban condition and race relations (Wirth’s major emphases) were continued with even greater sociological impact during William Julius Wilson’s long Chicago tenure; and continues with the work of Sampson and his colleagues. And on, and on.

Criminology

What does all this have to do with criminology? In a recent paper, my good friend and colleague, Charles Tittle, distinguishes between various approaches to theorizing in criminology (see Tittle, 2000). Although he recognizes that the search for empirical regularities is an important task, he observes that
some of us seem content to “troll” through data sets without much thought for their general theoretical significance, save for ad hoc speculation. Others focus on particular phenomena as though they require unique theories of causation – one theory for crime, or a particular crime, other theories for non-criminal behaviors and variations among them. In contrast, general theorists begin with the assumption that individual and organizational behavior derives from general principles and seek always to understand how their research informs general processes and relationships.

Initially I misinterpreted the distinction Charles was making, and I protested that I studied gangs, not because I believed gangs were so unique as to require unique explanatory principles, but because I was offered a unique opportunity to do research with theoretical implications that I hoped might be more generally applicable to human behavior. Insofar as gangs, or any other particular phenomenon, are theorized about per se, the theories are based on more general principles (of learning, and ecological theory, for example). Indeed, I have become bored with much gang research that, despite the failure of gang researchers even to agree on the definition of what sort of group or groups constitute a gang, is not more generally oriented. “Of course,” Charles responded, “you are a general theorist.” My bruised ego thus assuaged, I happily embraced the distinction!

That, of course, is what the Chicagoans sought. Read Park’s “Editor’s Preface” to Thrasher’s book. Read any of the wonderful research monographs that the early Chicagoans produced, or the work of latter-day Chicagoans. That is what it is all about, and that is what I have argued, ad nauseum, is necessary if different levels of explanation – macrosocial, individual, and microsocial (situational and interactional) – are to inform each other and be bridged theoretically (Short, 1997, 1998). My own view is that the microsocial level has the potential to bridge the gap between the individual and macrosocial levels. For example, understanding the nature of group processes, and interactional processes related to the unfolding of events in situations, can inform the conditions under which types of individuals and macrosocial settings produce both individual and collective behaviors of interest. Clearly this goes back to Chicago roots, with the stress on the nature of group life.

It goes back also, however, to institutional and macro-economic factors (with which my own criminological adventures began, particularly as extended via collaboration with Andy Henry (Henry and Short, 1954); and includes work such as Gary LaFree’s examination of institutional legitimacy and crime (LaFree, 1998) and recent work by John Hagedorn and Paul Goldstein (1999) who find tentative evidence that high homicide rates are associated with declining cities, while revitalized cities tend to have lower homicide rates. Declining cities are characterized as having relatively low levels
of human capital (as measured by percentage of workers with bachelor’s degrees, few major universities, low rates of innovation), manufacturing based economies (with much of that located in suburbs) with few global market links; low elasticity (that is, high internal segregation, low levels of political power for minorities, and weak links with suburbs), drug-centered informal economies and relatively unregulated drug markets (i.e., markets that lack stability, and have a high degree of free-lance dealing). Dynamic cities differ in all these respects. They are high-tech, information-based economies with elevated levels of human capital. They have large minority middle-class populations and political participation of minorities is high. Metropolitan government involves close cooperation among and between suburbs and central cities, and they export capital and have many links to world markets. Importantly, the informal economies of dynamic cities provide opportunities for services to the more affluent; and therefore for those who might otherwise become involved or stay involved in drug dealing to get out. Although the research is at this point quite impressionistic, the linkage of changing political economies, drug markets, and homicide rates is provocative and stimulating. By studying homicide rates in several cities, over time, and roughly classifying cities according to their relative declining or revitalized status, Hagedorn and Goldstein join type of urban political economy and sociological perspectives (Castells, 1991; Sassen, 1991, 1994). This type of research, and theorizing about it, offers still other opportunities to explore the relationship between human and social capital and crime.

We need to know a great deal more about how levels of explanation relate to one another. Bob Bursik (1998) criticizes – quite justifiably – my failure to produce an integrated theory in Poverty, Ethnicity, and Violent Crime, my most recent attempt to apply levels of explanation (Short, 1997). He suggests, generously, that I show “convincingly” that my use of “social and human capital orientation” is a promising basis for theoretical integration (Bursik, 1998, p. 907). I applaud the suggestion, and note that “social capital” is a theoretical construct that was introduced and elaborated by a Columbia-trained Chicago sociologist, the late, great Jim Coleman.

Social capital, criminology, and sociological theory

Harold Finestone once suggested that the gang was a concept that might bridge the gap between sociological and the psychological theories of crime and delinquency (Finestone, 1972). Certainly we need bridging concepts and perspectives, but the gang is too limited and, given the state of gang research – the lack of agreed-upon definitions, etc. – that seems a forlorn hope. In contrast, the microsocial level of explanation, including both situational and interactional analysis, has that potential; as does the social/human capital
nexus, which are bridging concepts, par excellence. They recognize illicit, as well as conventional qualities and relationships. Both operate at macro- and individual levels of explanation, and both influence interaction and the unfolding of events in situations. Clearly what is required are multi-method, multi-theory and, above all, general theory approaches.

Perhaps the best current example of the Chicago legacy as it has evolved and is likely to continue evolving is The Project on Human Development in Chicago Neighborhoods. The project meets all of these criteria. The first major publication to emerge from this effort was a 1997 article in *Science*, “Neighborhoods and violent crime: A multilevel study of collective efficacy” (Sampson et al., 1997). Collective efficacy, defined as the willingness to intervene for such common goods as supervision of children and maintaining public order, had a strong effect in suppressing violent crime in Chicago communities. A second major contribution to both criminology and to sociological theory concerning community and social disorganization is more recent (Sampson et al., 2000). Briefly, Sampson and colleagues find that collective efficacy with respect to children is related to intergenerational closure (the linkage between adults and children), reciprocal local exchange (inter-family and adult interaction with respect to children), and expectations that community residents will share responsibilities for control of children. The significance of the work is greatly enhanced by sophisticated research design, rigorous data generation and analysis.

The notion of “collective efficacy” combines insights from a variety of sources: ethnographic (see, e.g., Elijah Anderson, 1990, on the significance of “old heads” for social control in inner-city neighborhoods), historical (Jackson Toby’s, 1957, examination of ethnic differences as they relate to promoting a “stake in conformity” as a means of effective social control), and more recent ecological and institutional research by Kasarda and Janowitz (1974; Janowitz, 1978), and Bursik (1988) and especially Coleman’s theory of “social capital” (Coleman, 1988, 1990), and the seminal work of Sampson and his colleagues.

This is the sort of research that, in my view, will increasingly enrich both criminological and general sociological theory. It is Chicago-style research at its best, wherever it is practiced and whatever its specific aims.

**Conclusion**

Readers will not be surprised that I close this paper much as I did that ASC presidential address. Abbott and Gaziano conclude that “the idea of the Chicago school . . . became an animating force in some people’s minds, an obnoxious fiction in the eyes of others. It matters little what its history actually
is. The maxims and insights are practices that make up the objective face of the school were available in many other places as well. And the burning subjective experience of sociology as a commitment was felt in Cambridge and Morningside Heights and Bloomington as in Hyde Park. What has made Chicago unique is merely the ritual rehearsal of these things through an obsession with the tradition itself. Foolishly exclusive as that ritual often seems to be, it has the value of preserving a high ideal of what sociology – and more broadly, social science – can and should be.” (p. 257) To which I can only add a resounding, AMEN!

Notes

1. Abbott and Gaziano’s discussion of these seminars, in which the nature of the discipline and detailed discussions of how the Chicagoans viewed social psychology, social organization, and ecology, together with written exchanges between faculty members, and between them and various administrative officers, is an important historical and intellectual record, and fascinating reading.

2. Department Chair, Phillip Hauser, invited me to lunch at the Quadrangle Club in a gracious attempt to get me to remain at Chicago. I ordered a salad, which came in a bowl with a rounded edge. As Phil was reaching the peak of his pitch as to why it was in my best interest to stay, I pressed too hard on the edge of the bowl, spilling the salad in my lap! Phil’s great sense of humor overcame the seriousness of the occasion. Following his loud guffaw, we agreed that I would think about it!

3. My main job was as P.I of the NIMH-funded research program on youth gangs. I lectured occasionally to classes taught by others (including a few in the College, and in the Schools of Law, Social Work, and Divinity, as well as in the department), I taught a seminar on the gang research, and supervised a few other graduate students (including, at one time, Andrew Greeley). The Youth Studies Program – the official title of the gang research – occupied space vacated by the Family Studies Program, available with the departure of Nelson Foote. And we held brown bag seminars around the huge cherry table left by the family folks, as well as in the Social Sciences Building at 1126.

4. The large influx of graduate students in the post-World War II period must have inundated the faculty. It certainly led to a great deal of differentiation among graduate students, socially as well as intellectually. Many of us were veterans of the war, married, and ready to settle down. Alone among my close friends in graduate school, Andy Henry and I formed a sort of Ogburnian cohort, mainly because we admired his scientific rigor. We and our wives also socialized a great deal and collaborated in other ways. When we both wanted to take classes taught during the same hour by Ogburn and Blumer, we alternated class attendance each week, sharing notes and discussion. Midway in the quarter Andy took a teaching position at Illinois Tech which made this arrangement impossible. We coped by having Andy’s wife, Mary, alternate with me. At the end of the quarter we both received A’s in both courses.
References


