THINGS YOU CAN SAY WHEN YOU’RE TENURED: REFLECTIONS ON THE RELATIONSHIP OF
SOCIOLOGY TO CRIMINOLOGY, AND VICE-VERSA

John R. Sutton
Department of Sociology
University of California
Santa Barbara CA 93106

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I. Introduction: Toward a Sociological Criminology

I am honored by the opportunity to present my thoughts to this special session, and to share the occasion with scholars for whose work I have so much respect. I was so flattered by the invitation to participate that I accepted it without much forethought, and I confess that I chose the overly cute title for my paper because I had no idea what I would have to say. But in what Eugene O’Neill called, as best I can recollect it, the greasy gray light of a cold dawn—this was around the end of August, but we’re speaking figuratively—I realized that the prospect was quite daunting; so much so, in fact, that I tried to back out entirely. Karen Heimer talked me down off the ledge, for which I thank her. The prospect is daunting in large part because I have never done any research on the causes of crime, which is pretty much bread and butter around here, and I only began teaching courses in that area a few years ago. I can claim no comprehensive knowledge of the criminological literature, nor of the scope of this association. Still, I am a sociologist, which not only gets me invited to a lot of parties, it gives me a license to poach on the intellectual terrain of other disciplines while complaining about their many limitations. That may have gotten me in trouble: I have a sneaking suspicion I was only asked to come here because of one snide remark I made about criminology in an article a couple of years ago. Picture my inner child with a target painted on his forehead.

Hospitality calls for good manners in return, and I have no intention of getting by with snide remarks today. But let me begin by describing what I see as a scholarly challenge that is rooted in the institutional structure of the social science disciplines. For insights about criminology, I rely on the
work of Joachim Savelsberg and his coauthors (Savelsberg 1994; Savelsberg, Cleveland, and King 2004; Savelsberg and Flood 2004; Savelsberg, King, and Cleveland 2002): criminology in the U.S. has grown remarkably in the last 30 years, its fortunes rising with the perception of crime as a public problem, and as as a response to rising governmental funding; also presumably in response to the funding stream, the trend has been toward the applied in research and teaching, with undergraduate and graduate criminology programs flourishing most in second-tier public colleges and universities. Criminology is increasingly isolated from the disciplines, particularly with regard to theoretical development. As anyone who receives the same texts in the mail as I receive can attest, criminological theory ranges from debates over the evidentiary status of fingerprints, to semi-formalist models based on 40-year-old social psychology, to “critical” criminology—retread Marxism that reached its nadir under the “realists,” whose highest aspiration, as Jock Young patiently explained to me several years ago, was to pave the way for the election of a Labour government in Britain. I’m not kidding, and I hope he got what he wanted. While criminology is isolated from the disciplines, it is a mistake to call it parochial, which would imply a narrow and focused vision.

Criminology sprawls—not necessarily a bad thing.

Fair is fair, so I offer a challenge to sociology as well. As a series of network studies have shown, sociologists who study crime (and allied areas such as mental illness, drug abuse, and the catchall deviance, all of which lie in crime’s penumbra) are many, but peripheral. We tend not to coauthor, co-cite, or comember with sociologists in mainline subfields like stratification, political sociology, and organizations, nor in exciting cutting-edge areas such as culture, networks, feminist studies, or economic sociology. We are like Jupiter in Pluto’s orbit—too remote to be familiar, but too big to ignore. It is not my business to advise criminologists about their shared destiny, but for sociology, this is so not a good thing.
I’ll finish the throat-clearing part of the program with a couple of caveats. First, I come to you not only as a card-carrying sociologist, but also as one who has mainly studied punishment rather than criminal behavior as such. But a conviction I bring into my work, and into this presentation, comes from Durkheim: Crime and punishment are co-constitutive; crime is not a thing in itself—there is no more “as suchness” about crime than there is about gender or race or social class. So the study of the demand side—policies about what kinds of acts are criminalized as well as punishment practices—is inseparable from the supply-side study of criminal behavior. The second caveat is that much of what I have to say will be redundant, simply because I see a lot of exciting developments in the study of crime—I want to celebrate those developments, and suggest implications that may be easy to overlook. My goal is not to be substantively original, but to be synthetic and provocative at the same time, to challenge accepted assumptions and suggest useful linkages. What I have to say today involves a recycling of insights that are familiar to many people here, but I hope the end result is a mutual challenge: I intend a critique of what I take to be the central tendency not only in criminology, but also in the sociological discipline’s approach to crime. That central tendency is located at the intersection of a status attainment approach to crime, methodological individualism, and the general linear model. One possible innovation is that, for every insight I discuss, I identify correlative formal models and methodological techniques. The point is not to conduct a primer on methods, but to open up in formal terms a discussion of the logics of inquiry that we use, and their linkages to substantive theory.

II. Context: Wherever You Go, There You Are

One of the most salutary developments in the study of criminality over the last couple of decades has been a return to a central preoccupation of the Chicago school of sociology: the dependence of human behavior on its social context. The old Chicagoans were concerned with the ecology of
neighborhoods, communities, cities, and birth and immigration cohorts (e.g., Shaw and McKay 1942). Those insights were mostly lost in the onslaught of positivist criminology and functionalist sociology in the middle of the last century. Through the 1970s, it was mainly ethnographers who attended to context: they faithfully situated inmates in asylums and prisons (Goffman 1961; Sykes 1958), slum-dwellers in their neighborhoods (Suttles 1968; Whyte 1943), workers goofing off in factories (Roy 1958), and English lads in oppressive schools (Willis 1981). A central thrust of this work was to show how social organization—including deviance and social control—is emergent through face-to-face interactions that are situated in, and contingent on, the communities and organizations in which they occur. The methodological limitation of this kind of work is that inferences about context are limited to what is situationally present and observable.

In the 1980s, Blau and his collaborators (Blau and Blau 1982; Blau and Golden 1986) advanced a more formal contextual model of crime by linking aggregate levels of violent crime with interracial economic inequality. The “flurry” of research that followed (Peterson and Krivo 2005: 333) was provocative, but inconclusive about whether contexts of inequality accounted for racial differences in crime rates. A more sophisticated formulation by Sampson and Wilson (1995) proved to be a watershed: they argued that high rates of violent crime among African Americans can be explained by macro-level processes of deindustrialization, concentration of poverty, social isolation, and resulting disorganization that have primarily afflicted urban African American communities. Sampson and Wilson’s piece inspired an impressive wave of empirical research, much of it by Sampson with various collaborators (e.g., Morenoff, Sampson, and Raudenbush 2001; Sampson, Morenoff, and Earls 1999; Sampson and Raudenbush 1999; Sampson, Raudenbush, and Earls 1997); and while many questions remain, the evidence has tended to substantiate their argument (for an excellent review, see Peterson and Krivo 2005).
This growing body of research essays the panoramic scope of the Chicago ecologists, but brings to bear a theoretically richer sense of ecological variation: it focuses not just on demography, but on institutional frameworks, the distribution of social and cultural capital, and the urban political economy. It is no doubt familiar to all of you here today, so rather than dwell on its substantive results I want to emphasize its formal properties. The formal analog to contextual analysis is the hierarchical model (HM). HMs were developed in the context of educational research, as a means to analyze the effects of aggregated units—classrooms, schools, school districts—both directly on individual students’ achievement, and on the influence of individual student attributes. To cite a well-honed example, the effect of student SES is likely to vary depending on the average SES in each school.

The HM resembles a linear model with interaction effects: imagine one such model in which, say, the effect of an individual’s race on her propensity for crime is contingent on some attribute of the neighborhood in which she lives. The key difference is that the interaction model treats that contingency as deterministic—the neighborhood attribute is just another attribute of the individual, so predictions contain no error—while the HM treats it as stochastic: the effect of neighborhood on the effect of race is not uniform, but a distribution.1 Thought of in this way, the HM offers us considerable leverage on the hoary problem of agency and structure. Actors are allowed wiggle room to strategize, experience random events, and generally perceive the world differently from their neighbors; contexts are treated as material and symbolic entities that have their own intersubjective reality. Contexts do not determine action at the individual level, rather action and context are mutually constitutive through the medium of shared cognition. Note that this formulation avoids the need to make inferences from context to individual—e.g. that neighborhood disadvantage instills deviant values in individuals—that are often either unobservable or empirically wrong.

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1 This statement is deliberately Bayesian. Others would say that uncertainty about contingent causal effects is due entirely to measurement error or omitted variables. I find this implausible.
I would add, finally, that HM logic can easily extend beyond research on individual criminality, and is not even confined to research using quantitative data. My own research uses the HM to study how variation in opportunity structures affects the probability of imprisonment, both cross-nationally and among states and counties in the U.S. (Sutton 2004; Sutton and Rawlings 2006). There is also a strong vein of ethnographic research that implicitly uses a hierarchical style of analysis. The classic early statement of this approach is Emerson’s (1983) “holistic” model of social control decision making, in which in which official actors continually adjust their classificatory metrics to what they perceive as office- or agency-wide norms. A similar logic is apparent in Frohmann’s (1997) study of rape prosecution, and—in an exceptionally rigorous comparative study—in Heimer and Staffen’s (1998) study of attributions of child neglect in neonatal intensive care units.

III. Process: You Could Get Hit by a Bus

A second insight I want to remind you of is the importance of process in the understanding of all social behavior, including crime and punishment. This is another way of thinking about context, but along a different dimension than that attended to by hierarchical analysis. Actors are situated not only in social space, as in the HM, but in time: the history of nation states; the trajectories of racial and ethnic groups, social movements, and organizations; and their own biographies. The history of action constrains subsequent action by structuring the kinds of opportunities that are practically available: for example, failure to graduate from high school makes it much more difficult to go to college. Action is also constrained by the ways we retrospectively make sense of our individual and collective biographies and then use those insights to construct strategies for the future. For example, a young dropout might tell himself: No one in my family went to college, and they’re okay, so why should I bother?
Process models have had a powerful impact on general sociology, particularly stratification, through life course analysis. Life course studies challenged the older status attainment paradigm, which has a fundamentally static view of long-term social mobility, and emphasized instead individual adaptability and the contingency of opportunities at different stages of life. There is some connection here to my discussion of hierarchical analysis: a strong theme in the life course literature is that life trajectories are constrained by macro-level institutional structures, such as education and labor markets (Mayer 1997; Mayer 2000; Meyer 1986). A central concern for life course analysts has been the “youth-to-adulthood transition”—the process of leaving home and school, entering the labor force, and becoming an autonomous citizen—and the more or less successful ways it is managed in different societies. Since crime is disproportionately a young man’s game, this opens the door to the study of criminality.

Life course analysis was applied to the study of crime most conspicuously by Sampson and Laub (1993), and Hagan and his coauthors fruitfully incorporated it into their research on delinquency (e.g., Hagan 1997; Hagan and McCarthy 1997; Hagan, McCarthy, and Foster 2002; Hagan and Parker 1999). Once again, this work is probably familiar to you, so I will just emphasize three insights that I think are most salient:

1. Social behavior tends to be highly path dependent—that is, our current status affects our subsequent action, both because we lack the cognitive capacity to be arbitrarily creative and because life paths are enforced by powerful institutions.

2. Transitions from one kind of path to another are possible, indeed common, but they typically occur only at institutionally defined intersections.

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2 A parallel literature has shown that individual careers are shaped—often invidiously—by job ladders in organizations (Baron and Bielby 1980). This parallel is important, since both life course analysis and “the [no longer so] new structuralism” in organizations research are critiques of status attainment models of mobility.
3. Successful transitions require assets in the form of human, social, and/or cultural capital.

   This is as true, by the way, for transitions from straight to bent life paths as it is for the reverse, though of course the mixtures of capital required will vary.

   To my mind, there are three important contributions to a life-course perspective on crime. First, like the HM, it reintroduces social structure into the discussion of criminality, but here structure operates dynamically and with considerable fluidity. Again, individuals strategize, but only within their knowledge of available opportunities. Second, by emphasizing the contingent nature of both criminal and legitimate careers, it avoids a priori distinctions between deviants and straights, and offers us a theoretical language with which to discuss how alternative life trajectories originate, intersect, and—only rarely—permanently diverge. Third, it brings the societal response to criminality—once the exclusive domain of ethnographers—back into our theoretical and empirical purview, encouraging analysis of how scrapes with the law and the helping professions may foreclose future transitions to legitimate roles. As someone who was weaned on labeling theory and who currently studies imprisonment, this makes me happy.

   The appropriate formal model of the life course is a Markov chain—a series of states linked by transitions that make one state accessible from a previous state, with a conditional probability attached to each feasible transition.\(^3\) Markov chains are the mathematical backbone of event history analysis, the statistical tool of choice in contemporary life course research, because they provide a wonderfully flexible means of modelling all sorts of careers. For any given research problem, the relevant state space—and thus the range of likely transitions—can be defined in more or less fine-grained terms, depending on the theoretical interest of the analyst, and different specifications can be compared in terms of their fit to the data. Typically we will be interested in which states offer

\(^3\) An additional assumption is that one’s future state depends only on one’s current state, and not on one’s prior state. Sometimes this assumption is trivial: whether you are sentenced to prison depends on whether you are convicted of a crime, and whether you are convicted of a crime depends on whether you are arrested; but once you are convicted, your sentence does not depend on your arrest. Other times it is testable, given appropriate data.
pathways into some other states, but we can also ask how length of stay in one state (say, employment) affects the probability of transition to another (say, criminality or incarceration)—otherwise known as duration dependence. Some states may lead inexorably to only one destination, and others to branching paths. Some transitions are like one-way streets (like marrying or using drugs for the first time); some go in circles, leading to repeatable events (like having a child or committing a crime); some lead to dead ends, or “absorbing states” (like death). Perhaps most importantly, we can explore heterogeneity in the conditional probabilities attached to key life transitions—from high school to college, employment, or unemployment, from homelessness to delinquency, from drug use to crime to drug use to crime, from conviction to incarceration, from being a mediocre student at Yale to the presidency—across different groups in the population.

An implication of the Markov model applied to the life course is that transition probabilities are stochastic. This means simply that, however precisely we specify and estimate our models, events are subject to random influences; a point estimate is not a prediction about any individual, it is the mean (or other central tendency) of an expected distribution of individual outcomes. As with the HM, the random element of a Markov model is not just the residuum of measurement error, but an inherent property of the hurly burly of social life: individual outcomes are in principle unpredictable. Randomness is hard to acknowledge in highly rationalized western societies, where the competent self is perpetually moving down the road to the next career milepost. Nonetheless, every human culture that I can think of is rich with expressions of perplexity, amusement, frustration, and sorrow over the arbitrariness of god, nature, and the cosmos. In English, the most well-worn such expression is The best laid plans of mice and men, etc., but my favorite is more homely and working class You could get hit by a bus. Say you are a subject in a clinical trial for a new drug.

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4 When I was a rookie assistant professor, a (very) senior colleague kept in his bag of chestnuts the aphorism that “Probability is just a weak form of determinism.” That statement is wrong, as much so in social science as it is in quantum physics.
Before the end of the experiment you have a surfing accident and drown. The manner of your death is substantively irrelevant to the experiment; if the researchers are using an appropriate survival model, it is statistically irrelevant as well—in terms of the analysis, you are not dead, you are right-censored. Appropriately so. Accepting the stochastic nature of events is not an admission of the model’s failure, but rather a recognition of the limits of reductionism. The proper goal of sociological research, whether on crime or anything else, is not to make bad predictions about individuals, but strong predictions about distributions.

IV. Relations: It’s Who You Know

Criminological theories have often attended implicitly to the importance of network ties in producing criminal behavior. Most conspicuously, differential association theory argues that people learn criminality from their intimate contacts, and control theory centers on individuals’ associations with peers, families, and legitimate institutions, and the assumed impact of these associations on internalized values (Gottfredson and Hirschi 1990). Less obviously, Mertonian strain theory invokes local associations as the source of shared responses to deprivation.

The stickiest problem with these orthodox theories of crime is the causal links they draw between associations and behavior. In control, strain, and differential association theories (indeed in many kinds of sociological theories), the relational properties of groups, organizations, neighborhoods, and societies influence crime only through individual-level subjective phenomena—internalized values, attitudes, motivations—that are either unobservable constructs or, at best, retrospective accounts.5 Contemporary networks research, by contrast, flows more from Blau, and from Simmel before him, in insisting that population distributions and network ties have direct

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5 In control theory the commitment to methodological individualism runs deeper, since it argues that individuals’ fates are to a large degree sealed for life by their early childhood socialization. Thus subsequent experience, including aging itself, is thus not a meaningful variable (Gottfredson and Hirschi 1990, ch. 6). See Laub and Sampson (1993) for a critique.
causal implications for patterns of behavior, thus avoiding the necessity for causal detours through spooky unobservables like social solidarity or internalized values. Blau argued that all sorts of social behavior, including violent crime, can be accounted for by the distribution of groups within a population, and the kinds of relations that emerged from those distributions (Blau and Blau 1982; Blau 1994; Blau and Golden 1986). The focus here is on the ties themselves, not on the individual attributes they are presumed to represent; social structure is the organization of ties in “Blau space” (McPherson 2004)—a multidimensional system defined by distributions of socioeconomic variables. In this last section of my comments, I want to outline briefly the implications of this more hard-core approach to networks for the study of crime.

For the purposes of this paper, the most powerful insight of relational sociology is the homophily principle (McPherson, Smith-Lovin, and Cook 2001): In a population, people with similar social characteristics (especially with regard to race and class) tend to concentrate in the same locations, to forge endogamous network ties of marriage, friendship, and organizational comembership, and to possess similar attitudes and information. Birds of a feather flock together. This is a simple idea (an empirical fact, actually) with radical implications. Mainstream sociology, characterized by the analysis of survey data using the general linear model, relies on the idea that individuals are fixed entities with variable attributes. The procedure is then to pluck these individuals from their environments and treat them as atomized realizations of their measurable attributes:

The meat grinder of regression puts all the information about social context into the maw of the independent variable, and crunches out values of the dependent variable with the teeth of the estimated parameters, shredding any sense of social structure left in the data (McPherson 2004: 204).

The finding of correlations among individual variables—between, say, social class or race and crime—is taken as evidence of a causal association; when we have included enough variables to bring the proportion of explained variance to a respectable level, we conclude that our theory explains something. A strong relational approach deflates this whole procedure by suggesting that
our correlations are spurious byproducts of the fact that people tend to associate with others who are sociodemographically similar.

From this point of view, network ties are important for the production of crime because they facilitate and constrain flows of information. Important types of information include not just practical skills and techniques for legitimate or illegitimate occupations, but also cognitive understandings of things one finds it natural and reasonable to do. Thus an important byproduct of social networks is the production of shared meanings, in the form of language, attitudes, scripts, recipes, accounts, identities, and tastes, within localized social spaces. Sociodemographically similar people tend to share the same cultural “toolkits” (Swidler 1986) because they interact more with each other than with people who are dissimilar. This opens up an important link between criminology and the core concerns of mainstream sociology: we can treat networks of addicts sharing knowledge about how to score and networks of middle-class high school students sharing information about how to get into college as generically similar social phenomena, differentiated by their location in social space rather than the immanent attributes of the individuals they comprise. “Criminal subcultures” are thus special cases of a fundamental social process.

Thus a key variable is social capital, or the strength and distribution of network ties. Hagan (1994: ch. 3; Hagan and Palloni 1990) has done a good job of putting this concept on the criminological agenda, so I will end by highlighting two levels at which social capital can have a decisive influence. First, individuals possess social capital in the form of access to basic skills and techniques for managing life course transitions, and individual-level variation in network assets may go a long way toward explaining tendencies toward positive or negative mobility. Obtaining a legitimate job is easier for those that have extensive (not necessarily strong) social ties providing information about available jobs (Granovetter 1974); a professional thief draws on his social capital for technical lore, partners, neutralizing accounts, and support in times of legal trouble (King and
Chambliss 1984; Sutherland 1937); and check forgers’ careers tend to be short and nasty precisely because their role precludes the formation of stable social ties (Lemert 1972). Second, social capital is also an attribute of neighborhoods and communities. Where legitimate network ties are profuse both within and beyond a community, young people find it relatively easy to obtain knowledge leading to educational and occupational achievement. Inner-city youth must rely on a small group of “old heads” for such information, but tips about transitions into illegitimate roles are likely to be abundant (Anderson 1999). This is where micro meets macro, where context, process, and relations come together: Over the last several decades, aggressive political and economic disinvestment in urban areas in the U.S., combined (ironically) with some residential desegregation for middle-class blacks, has led to the impoverishment of social capital, weakening of opportunity structures, and concentration of poverty in black inner-city neighborhoods (Hagan 1994: ch. 3; Sampson and Wilson 1995; Wilson 1987). Research on the intersection of these issues is among the most promising in criminology and sociology.

V. Conclusion: Durkheim Was Right Again

In this paper I have tried to identify a few major themes in what I see as the best of sociology and the best of criminology, and, by exploring their formal underpinnings, to point out some implications of these themes that I think are too easily overlooked. My goal is a modest one: not to convert the heathen to the gospel of sociology—after all, most of the authorities on whom I draw are already much honored here—but rather to insert myself into a conversation that has been underway for some time, and to outline, on the fly, what I think is an emergent agenda for research on crime and punishment. This agenda rejects methodological individualism, but also the reification of social structure; it combines a constructionist sensibility to the arbitrariness of criminal labels with a realist awareness that crime, like shit, happens, and tends to flow downhill. It does so by insisting
that causal influences operate stochastically: actors have agency, but only within limits set by their access to capital; structure creates more or less well-rutted pathways for both positive and negative mobility—it directs the traffic, but doesn’t determine the destinations of particular actors.

The lessons of sociology for criminology are simple: context matters, process matters, relations matter. That’s pretty much it. But there is an important lesson as well for sociology from criminology: crime matters. Put with more theoretical pretension, Durkheim was right: crime is normal—it is neither exotic, nor pathological, nor recondite; it is routine, quotidian, and deeply interwoven with the experience of everyday life. Yet, for reasons that are quite sociologically comprehensible, sociologists who study inequality and the life course tend to leave crime and punishment to the criminologists; they tend to focus on positive outcomes, such as educational and occupational attainment. This is unfortunate, because crime and punishment are fundamental processes of social differentiation; we ignore at our peril their influence on the structuration of the moral order and patterns of social mobility. The evidence is beginning to pile up that, especially for people of color, negative life events—unemployment, mental and physical illness, rehab, imprisonment—are not just rare time outs, but routine life course experiences, with profound long-term effects for individuals and communities (Western 2002; Western and Beckett 1999; Western and Pettit 2000; Western, Pettit, and Guetzkow 2002).
References


