

The background of the book cover is a photograph of vertical prison bars, creating a sense of confinement and security.

The **Origins** of **American** **Criminology**

Advances in Criminological Theory
Volume 16

Francis T. Cullen
Cheryl Lero Jonson
Andrew J. Myer
Freda Adler
editors

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American
Criminology

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Introduction: Preserving the Origins of American Criminology

*Francis T. Cullen, Cheryl Lero Jonson,
Andrew J. Myer, and Freda Adler*

Both introductory and theory textbooks are the main conduits through which criminological theory is learned and preserved across time. The very richness of theorizing confronts text authors with a daunting challenge: How to convey in a delimited space the diversity of extant crime explanations? The consensus strategy involves three steps. First, divide up theories into neat compartments called “schools of thought.” Second, show how later theories built solidly on earlier ones. And third, distill and present the key ideas of each tradition and of each theory within a tradition. Thus, out of necessity, clarity, simplicity, and parsimony are privileged.

This approach to conveying criminology is functional. Students are taught clearly how to recognize theoretical apples from theoretical oranges—strain theory from control theory from social learning theory from critical theory, and so on. The “take away message” is easier to internalize. What do strain theorists say causes crime? “Strain” is the ready answer. What do strain theorists have in common? They argue that “some form of strain causes crime.” Thus, who founded strain theory? Well, “Robert Merton.” And how did Agnew extend this early strain theory? Well, “Agnew identified three, rather than just one, form of strain.” Bravo! Undergraduates, you will do well on your multiple-choice test; graduate students, you will pass your comprehensive examination.

But this shared approach to conveying the contours of criminological thought risks being incomplete, if not misleading. It distorts social reality, reifying theories in truncated form and constructing an image of theorizing that is pristine. Ideas seem to exist in a virtual space, uncluttered by complexity and somehow detached from their authors’ biographies and social context. The growth of ideas appears to take on an air of inevitability, as though, for example, Clifford Shaw and Henry McKay’s social disorganization theory was destined to

be transformed by Robert Sampson into collective efficacy theory—or, if not Sampson, then surely by someone else. And the invention of theories is implicitly portrayed as a strictly cognitive game—of some bright folks sitting at a desk, reading previous works, and coming up with a nice new theory.

Criminological theorizing, however, is a human enterprise—a messy affair, shaped by contingency and serendipity. Odd events and questionable decisions place scholars on life-course trajectories that result in theoretical innovation. How many other scholars made different choices that thwarted their potential to create fresh perspectives? Indeed, as with any science or social science, understanding the origins and growth of ideas requires what C. Wright Mills (1959) called a “sociological imagination”—a sense of how biography and history intersect to produce unique human conduct. Sociologists of science, such as Robert Merton (1973), have understood this for many years.

As we explain in more detail below, our project is a call for criminologists to take seriously the need to engage in a *sociology of criminology*—the systematic analysis of the diverse factors that have shaped the emergence and growth of criminological theory. A key part of this enterprise is collecting data about theorists and the context in which their ideas were invented. The core purpose of this volume is to contribute to this deeper analysis by ensuring that such biographical and contextual information surrounding criminological theorists is preserved.

Deconstructing the Nature of Science

In 1962, Thomas Kuhn published *The Structure of Scientific Revolutions*—a work that sparked inordinate attention and was reissued in an expanded edition in 1970. Kuhn challenged the traditional view of scientific progress, which held that knowledge was produced through rational experimentation that led incrementally to substantive understanding about the natural world and to new and better theories. He offered the heretical claim that “we may have to relinquish the notion, explicit or implicit, that changes of paradigm carry scientists and those who learn from them closer and closer to the truth” (1970: 170).

Kuhn observed that most of the time, scholars labor within the confines of a hegemonic paradigm. They set about solving the puzzles that the reigning paradigm helps to identify. Anomalies—unsolvable questions, inconsistent findings—are pushed aside or are seen as explainable once the paradigm evolves further. Kuhn called this relatively static period “normal science.” But moments of crisis can emerge. The anomalies grow too large to be ignored, and a competing paradigm is set forth that explains more, especially about the anomalies, and that provides a host of new puzzles for scientists to address. A revolution transpires in which the old paradigm is discarded and the new paradigm is embraced. Through this process, science grows less primitive and knowledge, including increasing specialization, accrues within paradigms. Kuhn seemed to suggest that we know more about the world, but he doubted that science was on a road toward ultimate objective truth. As Kuhn (1970: 170-171) concluded:

But nothing that has been or will be said makes it a process of evolution *toward* anything. Inevitably that lacuna will have disturbed many readers. We all are deeply accustomed to seeing science as the one enterprise that draws constantly nearer to some goal set by nature in advance. But need there be any such goal? (emphasis in original).

Kuhn's portrayal of science was not only celebrated but also subjected to withering criticism (see, e.g., Lakatos and Musgrave 1970). The important point for us, however, is not the accuracy of his science-as-revolution thesis. Rather, Kuhn's broader contribution was to remove science from the sacred and to place it in the social. Scientists were not priests and their laboratories were not altars; they were humans at work trying to gain professional status and support their families. Their allegiance to a theory was not absolute but conditioned by career interests and opportunities. Should a new paradigm emerge with a host of fresh puzzles to solve—offering grant money to undertake this “new” research—they would jump theoretical ships (see also Cole 1975). In short, Kuhn problematized and humanized science.

This message found a receptive audience among social scientists where similar intellectual developments were under way. Peter Berger and Thomas Luckmann's (1966) *The Social Construction of Reality* was a defining contribution. Berger and Luckmann did not deny that there was a real world that people could simply wish away. But how that world was understood—whether a choice was seen as “free” or “caused” or whether an offender was portrayed as a “super-predator” or as “redeemable”—was not simply objectively obvious but socially constructed by those bound up in their own biography and socio-historical context. Science, including social science, was not immune from this process. Objective reality might exist, but as a human enterprise, science was a form of knowledge construction that involved more than a cold, autonomous appraisal of the world. Berger and Luckmann (1966: 188) observed that theories should not be:

regarded, positively or negatively, as propositions of “science,” but analyzed as legitimations of a very peculiar and probably highly significant construction of reality in modern society. Such analysis, of course, would bracket the question of the “scientific validity” of these theories and simply look upon them as data for an understanding of the subjective and objective reality from which they emerged and which, in turn, they influence.

Alvin Gouldner's (1970) magisterial *The Coming Crisis of Western Sociology* echoed, extended, and solidified the constructionist view that theoretical knowledge is a social creation that may flow from and serve diverse socio-political interests. Gouldner challenged the notion that sociologists—the subject matter of his analysis—are autonomous and practice science free of social influence. According to Gouldner (1970: 25):

4 The Origins of American Criminology

The sociologists' task today is not only to see people as they see themselves, nor to see themselves as others see them: it is also to see *themselves* as they see other people. What is needed is a new and heightened self-awareness among sociologists, which would lead them to ask the same kinds of questions about themselves as they do about taxicab drivers or doctors, and to answer them in the same way (emphasis in original).

The implications of this revised perspective for understanding the nature of science are potentially profound. For Gouldner (1970: 26):

Sociologists must surrender the human but elitist assumptions that *others* believe out of need whereas *they* believe because of the dictates of logic and reason.... It will be difficult for them to feel in their bones, for example, that "scientific method" is not simply logic but also a morality.... For like other men, sociologists still commonly confuse the moral answer with the empirical thinking that what should be, is. That is, we too readily suppose that a change, particularly if it is to a theory that we ourselves happen to accept, has been made primarily because it was required by the findings of studies done in conformity to the scientific method: we thus hasten to affirm our moral convictions rather than allow the questions to remain unanswered until the studies, by which alone it could be answered, have been done (emphasis in original).

Gouldner observed that scholars' "background assumptions" comprise a key factor that shapes the invention and acceptance of theories. These assumptions are part of "sub-sociology" or a discipline's "infrastructure" and are akin to what Mills (1943) once termed the "professional ideology" of social analysts (see also Cullen and Gendreau 2001). These assumptions range from broad beliefs about the nature of society (e.g., whether the social order is due to consensus or imposed by power) or beliefs about certain social domains (e.g., whether poor people are lazy or oppressed, or whether criminals are redeemable or wicked super-predators). Gouldner noted that scholars create and believe in theories that resonate with the background assumptions they embrace. Notably, these views about society and specific domains are often learned early in life. They precede rather than reflect scientific training and any reading of the empirical evidence. As a result, they potentially play a potent, but unrecognized, role in influencing theoretical allegiances.

Gouldner did not descend into the post-modernist trap by seeing knowledge as fully socially constructed. He recognized that theories change through "internal, technical development and elaboration" (1970: 397). In essence, such intellectual renovation might be triggered by probing critical scrutiny, the production of empirical evidence disconfirming a core thesis, or the presentation of convincing arguments for how the theory might be extended. But Gouldner (1970: 397) cautioned that a theory might be transformed for a more latent reason—"as a consequence of changes in the infrastructure in which it is anchored." In short, paradigm shifts occur not simply because logic and data demand it, but because the scholarly community, perhaps buffeted by new

social experience or infused with members from a younger generation, embrace fresh background assumptions about the world. As Gouldner (1970: 398) put it, “when a theory resting on one infrastructure, one specific set of sentiments, domain assumptions, and personal reality—is encountered by those whose *own* infrastructure is quite different, the theory is experienced as manifestly unconvincing” (emphasis in original).

Taken together, the insights of Kuhn, Berger and Luckmann, and Gouldner—among many others (see, e.g., Gould 1981)—showed forcefully that theories about the physical and social world could no longer be seen as detached depictions of an objective reality. By deconstructing science—by showing it as a very human and thus socially influenced enterprise—they created a major turning point in the social sciences. From now on, it would be clear that the origins of theories—as well as the intensity and duration of the acceptance they achieved—were shaped not only by their explanatory power but also by a host of factors external to the logic of the ideas internal to any given theoretical perspective. In turn, attempts to convey the development of knowledge as an incremental process in which new ideas build ineluctably on old ideas were now obviously misleading.

Preserving Knowledge

Within criminology, we thus do our students and ourselves a disservice by, again, the understandable and functional practice of demarcating traditions in which the sequence of theory development is shorn of a detailed consideration of the diverse factors that shaped such theoretical evolution. To be sure, some texts more than others do place ideas within their context (see, e.g., Lilly, Cullen, and Ball 2011; Mutchnick, Martin, and Austin 2009; Pfohl 1985), but this is more the exception than the rule. Further, these analyses are often inhibited by a lack of detailed knowledge about the theorists, including their biography, university surroundings, and contextual influences. Accordingly, there is a compelling need to undertake a *sociology of criminology* in which efforts are made to explore the diverse factors that have shaped, and will continue to shape, the development of theories and the growth of knowledge within in our discipline.

These observations lead us to the current project. Although notable exceptions exist (see, in particular, Geis and Dodge 2002; Laub 1983; Mannheim 1972), systematic efforts to collect information on the origins of theories are in short supply (see also Berger 1990). Occasionally, in-depth historical analyses of a theorist and his or her work appear (e.g., Geis and Goff 1986; Morrison 2004), or an interview with a scholar is undertaken (see, e.g., Cavender 1993). Still, as a field, we have not institutionalized a means for preserving knowledge about theorists’ lives and the perspectives they create. Again, in the absence of such information, the task of fashioning a comprehensive sociology of criminology is made difficult.

This volume attempts to help fill this void in the literature by capturing the diverse sources of paradigms that rest at the core of American criminology. The chapters illuminate not simply the theories' content but, more importantly, how they were invented and subsequently developed. The goal of this volume is thus to preserve the "stories" behind the theories—to reveal the struggles, modes of thinking, and often fortuitous circumstances that have inspired the great ideas in criminology. In so doing, readers will see, and learn lessons from, the human face of criminological theorizing in ways that are not apparent in more standard summaries of the discipline's theoretical perspectives.

Preserving knowledge about theorists' lives not only is important but also confronts a ticking clock. American criminology was largely a twentieth century invention. Many prominent theorists already have passed away—oftentimes with few, if any, efforts to interview them. Unlocking the mysteries behind their work is now limited to historical investigation. Many other significant criminologists were children of the 1940s and 1950s, and their scholarly life course is progressing into retirement. They will not be with us forever. Unless fellow criminologists record the stories of scholars and their theories, this information will be lost.

Plan for the Book

In this context, the current project uses diverse methods to present, under one cover, information on theorists who have played major roles in guiding thinking about crime in our discipline. Four of the eighteen chapters are reprinted because they represent illuminating accounts that merit being included under the umbrella of any volume probing the "origins of American criminology." Beyond these essays, fourteen new chapters are presented. For scholars who have passed away, historical investigation is used for the analysis. For the remaining scholars, the chapters are based either on interviews or on autobiographical accounts. The use of diverse methods reduces the extent to which chapters follow a standardized format and provide standardized information. Even so, it contains a silver lining: methodological variation has produced a treasure of information that is both fascinating and useful. Further, this approach supplies different examples of how knowledge on our discipline can be gathered.

The difficulty of planning the contents of this volume is that when it comes to theory, criminology possesses an embarrassment of riches. We will leave it to others to decide whether the existence of multiple perspectives reflects an abundance of theoretical creativity, the complexity of our subject matter, or the fact that criminology is an immature social science incapable of building consensus around a single paradigm. Regardless, as editors, we faced the daunting challenge of selecting which theorists to include and which to omit. The very omission of influential criminologists means, of course, that plenty of space exists for future volumes aimed at recording the origins of American

criminology. We trust that our work will help to inspire and serve as an exemplar for these efforts.

Our selection of theorists for inclusion in this volume was guided by five considerations. First, since the 1930s, American criminology has been intimately shaped by three enduring theoretical traditions: anomie/strain theory, differential association/social learning theory, and control theory (Agnew 2001; Hirschi 1969; Kornhauser 1978). Together, these form what is often referred to as “mainstream criminology.” As a result, we sought to ensure that the chief theorists—past and present—in these traditions would be included in the current project. Thus, strain theory is represented by a chapter on Robert Merton, a chapter by Richard Rosenfeld and Steven Messner on their institutional-anomie theory, and a chapter by Robert Agnew on his general strain theory. The notion that crime is learned is represented by a chapter on Edwin Sutherland and his differential association theory and a chapter by Ronald Akers on his social learning theory. Finally, control theory is represented by a chapter on Walter Reckless and Simon Dinitz’s containment theory, a chapter on Travis Hirschi’s social control/bond theory, and a chapter by Michael Gottfredson on his general theory of crime with Hirschi.

We should also note that Part VI of the volume is organized around the debate that has long raged between social learning and control theories (see, e.g., Costello 1997; Matsueda 1988, 1997). In brief, the notion that crime is learned suggests that antisocial attitudes and skills must be acquired for people to break the law. The motivation and capacity for crime thus require positive learning. For control theory, however, the motivation to offend is rooted in humans’ desire for easy and immediate gratification. Criminal acts entail few skills, because most offenses are easy to commit (e.g., taking a camera, shoplifting clothes). Instead, the theoretical challenge is to explain why people do not act on their natural inclination to gratify their needs. The answer, of course, is the presence of controls, whether internal or external.

Second, on the macro-level, the Chicago school’s social disorganization theory has been the field’s dominant perspective. This earned the inclusion of a chapter on Clifford Shaw and Henry McKay, who used social disorganization as the organizing construct for their work on juvenile delinquency in urban areas. In addition, Robert Sampson has been instrumental in revitalizing interest in using social disorganization theory to explain community variations in crime rates. His collective efficacy theory is the chief contemporary extension of this tradition.

Third, in the late 1960s and into the 1970s, mainstream criminology was challenged by new ways of thinking about crime—most of them inspired by the social transformation of the sixties. In this context, more attention was paid to structures of inequality and to the ineffectiveness of state power, including the capacity of the criminal justice system to reduce crime. In Part V, we thus include chapters on some of the major theoretical innovations that emerged from this era and that continue to guide theorizing today. These include the peacemaking

criminology of Richard Quinney and a chapter on John Braithwaite. Both of these works challenge the wisdom of organizing a society that is non-supportive of its members and that responds to crime by harming offenders rather than finding ways to restore them to the community. A chapter by Ronald Clarke and Marcus Felson represents a very different view, but one that also questions the capacity of formal criminal justice sanctions to lessen crime. Based on routine activity theory, Clarke and Felson favor situational crime prevention efforts that seek to reduce opportunities for crime by making targets less attractive and by increasing guardianship over targets.

Fourth, the University of Pennsylvania was a unique intellectual community, producing ideas and scholars that left an indelible mark on criminology. Three chapters explore the history of Pennsylvania criminology. We start with a chapter on the founder of this tradition, Thorsten Sellin. A chapter by Lawrence Sherman traces the diverse contributions of Pennsylvania scholars. A chapter on Freda Adler illuminates how even feminist criminology has roots that lie within this academic environment.

Fifth, we end with a discussion of a theoretical model—life-course criminology—that promises to define criminological theory and methodological analysis in the twenty-first century. One chapter is devoted to early advocates of this perspective, Sheldon and Eleanor Glueck. The Gluecks' work is salient both for its substantive findings and because the Gluecks' data subsequently were used by Robert Sampson and John Laub (1993) in their pathbreaking book *Crime in the Making* (see also Laub and Sampson 2003). A second chapter covers the theorizing of Terrie Moffitt, who proposed that criminal development is guided by two developmental pathways: one in which youngsters are mainly troublesome during adolescence (adolescence-limited offenders) and one in which entry onto a criminal pathways starts early in life and persists well into adulthood (life-course-persistent offenders). Moffitt's (2006) developmental theory and Sampson and Laub's (2005) age-graded social bond theory have been chief rivals in the current development of the life-course perspective.

Toward a Sociology of Criminology

We must caution that the eighteen chapters are organized under different formats and probe issues in unique ways. In part, this reflects the different methods used to collect data for the chapters: historical investigation, interviews with scholars, and autobiographical accounts by theorists. But in part it also reflects our desire not to try to impose a preordained structure on the collection of information that might have inhibited the free emergence of recollections and insights. Nonetheless, if we are to move—even in a beginning way—toward a sociology of criminology, we need to develop a framework for organizing the data we collect on theorists and their ideas.

Toward this end, we suggest that the knowledge contained in the chapters can be seen as falling into three broad categories. These categories comprise neither