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MAKING CRIME A SCIENCE:
THE RISE OF EVIDENCE-BASED CRIMINAL JUSTICE POLICY

by

Johann August Koehler

A dissertation submitted in partial satisfaction of the
Requirements for the degree of
Doctor of Philosophy
in
Jurisprudence and Social Policy
in the
Graduate Division
of the
University of California, Berkeley

Committee in charge:

Professor Franklin Zimring, Co-Chair

Professor Jonathan Simon, Co-Chair

Professor Mona Lynch

Professor Christopher Muller

Summer 2019

Making Crime a Science: The Rise of Evidence-Based Criminal Justice Policy

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Johann August Koehler

ABSTRACT

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Doctor of Philosophy in Jurisprudence and Social Policy

University of California, Berkeley

Professor Franklin Zimring, Co-Chair

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This dissertation is a historical sociology of how criminological expertise makes scientifically legitimate claims to inform criminal justice policy. I approach that analysis by posing three inter-related sub-questions: first, I ask how criminological knowledge cultivates scientific legitimacy by studying shifts in the shape and form that criminological knowledge takes, with particular attention to the types of knowledge that criminology produces. Second, I ask how criminological knowledge assigns scientific legitimacy to penal policies by studying how rules emerged for making a scientifically legitimate policy claim. Third, I ask how the pursuit of scientific legitimacy affects the knowledge that criminologists produce by studying how criminological knowledge has been deployed on the policy stage.

I use quantitative and qualitative tools to answer these questions. In the first, I make use of the most comprehensive dataset of criminological thought to date to study the co-citation structure of criminological knowledge. Social network analysis and unsupervised learning tools clarify the research communities to which criminology has given rise, expose variations over time in the extent of either ferment or insularity between research communities, and yield insight into criminology's key epistemic continuities and discontinuities. The second analysis is a history of key moments of official criminological knowledge production, when the field 'emitted' a signal about a scientifically legitimate penal policy. Interviews with panelists who contributed to high-profile 'Consensus Study Reports' assembled by the National Academies of Sciences help trace the development of policy-informing criminological claims. The production of those reports provides a window into the questions to which criminological expertise could be put to use, and the strategies criminologists deployed to imbue their findings with policy value.

The dissertation contributes three principal findings. First, I observe that criminological knowledge acquired structure only as recently as the mid-1960s. Before this period, criminological knowledge cast around in search of an identifiable research agenda and set of contributions that it could make to criminal justice policy. Tensions between criminological research programs fractured, and the resultant disrepair cast a shadow over the next fifty years. Second, I find that criminologists reacted to the instability of the mid-1960s in two different but related ways. On one hand, criminologists successfully devised rules governing how to issue persuasive, policy informing claims. Those rules bore the trappings of an established scientific discipline even when other trappings were—and remain—absent. On the other hand, criminologists responded to the disenchantment of the preceding order by recasting crime as a problem they were singularly well equipped to solve. As a result, criminologists have positioned themselves as both disinterested

observers and as strategic engineers in the public understanding of crime and criminal justice policy. Third, I observe that criminology's capacity to identify successful crime control programs, emblemized most triumphantly in the 'evidence-based' movement, shapes the bounds of reasonable proposals for criminal justice policy. Although criminologists have little say in the adoption of any specific criminal justice policy, nonetheless policies that lack the support of an evidence base fall beyond the scope of reasonability. By implication, criminal justice reform should remain alert to how the evidence-based movement in criminology orders—and possibly *narrows*—the repertoire of available policy options.

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I'm grateful to the interviewees who kindly participated in this project, to Brendan Dooley at the Oral History of Criminology Project for help in arranging those interviews, and to the librarians at the Bancroft and Radzinowicz Libraries for assistance in securing archival materials. Thanks also to the Jurisprudence and Social Policy program, the Coblenz Civil Rights Endowment, and the Center for the Study of Law and Society for research support.

My committee members made my dissertation—dare I say it—worth reading. Chris Muller and Mona Lynch are dizzyingly insightful and shrewd thinkers, and I still breathe a sigh of contented relief at having convinced them to join me for this stage of the degree. I can also affirm their reputations as dedicated supporters of junior scholars are, if anything, underestimated.

Jonathan Simon and Frank Zimring co-chaired this dissertation, and after seven years in Berkeley their mentorship will be by far the hardest thing to leave behind. Each of them taught me invaluable lessons: Jonathan taught me to read not just for what's being said, but also for what's *not* being said; Frank taught me both the intellectual relentlessness and the attention to detail required to whittle an argument into one that withstands even his scrutiny. Neither of these lessons is an easy one to teach, and although Jonathan and Frank bring different styles to their mentorship they nonetheless share the same devotion to their students. I hope my work does their tutelage justice in the pages that follow and in those yet to come during the years ahead.

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before I received any of those gifts, it was my mother who patiently taught me how to read using cardboard cutout letters she'd strewn across the living room carpet. For that, and indeed for so much more besides, I smile as I dedicate this dissertation to her.

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ABBREVIATIONS

ACJS	Academy of Criminal Justice Sciences
ABF	American Bar Foundation
ASC	American Society of Criminology
BJS	Bureau of Justice Statistics
CLJ	Committee on Law and Justice of the National Academies of Sciences
CSR	Consensus study report of the National Academies of Sciences
FACA	Federal Advisory Committee Act
FBI	Federal Bureau of Investigation
FOIA	Freedom of Information Act
GS	Google Scholar
IOM	Institute of Medicine
JJDP	Juvenile Justice and Delinquency Prevention Act
LEAA	Law Enforcement Assistance Administration
NAS	National Academies of Sciences, Engineering, and Medicine
NIJ	National Institute of Justice
NRC	National Research Council
OJP	Office of Justice Programs
OMB	Office of Management and Budget
RECAP	Minneapolis Repeat Call Address Policing Experiment
SMS	Maryland Scientific Methods Scale
WoS	Web of Science

CHAPTER ONE: THE CONTOURS OF CRIMINOLOGICAL THOUGHT

Abstract

Criminological scholarship is among the key drivers of penal discourse and criminal justice policy. However, those seeking to model the relationships between criminology, penal discourse, and criminal justice policy confront the thorny matter that criminological thought's contours are stubbornly difficult to measure: efforts to date either flatten criminology's underlying variations or focus unsystematically on parts shorn from the whole. I offer an approach that makes possible more systematic and credible claims from which future analyses may proceed. I draw on the most comprehensive dataset of criminological thought to date, which comprises 61,551 Anglophone criminological journal articles citing to 231,083 reference texts drawn from more than a century of scientific production. I classify criminological ideas using socially networked co-citations, and I differentiate trends in engagement with those ideas using *k*-means clustering. I observe three periods of structural change in criminological thought: during 1910–1965, criminology *crystallized* ideas into coherent research programs; during 1965–1995, it *stratified* ideas deserving attention from those that did not at the same time that it relegated the bulk of ideas from its core to its periphery; and during 1995–2017, it *institutionalized* ideas into settled divisions of mainstream from fringe. Identifying criminology's key currents in this way helps disentangle the complex interplay between criminological knowledge on one hand and its assorted determinants and effects on the other.

Keywords

Sociology of criminology; criminological knowledge; social network analysis; co-citation analysis; *k*-means clustering

People know what they do;
frequently they know why they do what they do;
what they don't know is what what they do does.
—Foucault, *Madness and Civilization*

INTRODUCTION

What are the key currents of criminological thought? What ideas constitute criminology's mainstream and its fringe? How do criminological ideas emerge, stabilize, and wither? Answering these questions entails more than an exercise in idle reflexivity. Criminologists, as producers of avowedly instrumental knowledge, no less, have a stake in appraising the state of the field and the value of the knowledge they produce. After all, it is part of the criminologist's *raison d'être* to hold that criminological thought matters to the production of penal policy's institutional hardware and penal discourse's cultural software. But discerning criminology's role first requires characterizing its contours. It requires, in brief, a program of mapping criminological thought.

Three qualities of criminological thought make mapping its contours worthwhile. First, the accumulation of scientific knowledge triggers a need to identify both how one thing relates to the next and the gaps that deserve a closer look. Criminology's abundant state-of-the-field survey essays, hoary reflection pieces, and rich systematic reviews and meta-analyses suggest that it harbors the same need for synopses of what we know. To that end, knowledge maps abstract yet further and clarify the overall structure of ideas in general. Second, criminology's policy-adjacency disposes criminologists to grapple with what their work does and renders theorizing criminology's key determinants and effects altogether inescapable. Certainly, mapping one criminological idea's relationship to the next is intrinsic to sound science; but, for the criminologist, monitoring whether and under what circumstances those ideas matter in the world is moreover a plainly sensible exercise in professional self-preservation. Third, a great deal of work that needs doing requires characterizing criminology in one manner or another, even though doing so flummoxes many who try. In part, this is because characterizing a field—treating it as a variable susceptible to measurement for the purposes of modeling its relationships to its determinants and effects—presupposes defining and circumscribing it.

Yet criminology is a dreadfully elusive thing to define and measure (*see* Bosworth & Hoyle 2011; Dooley 2018; Holdaway & Rock 1998; Loader & Sparks 2010; Savelsberg 1994; Savelsberg & Sampson 2002; Triplett 2017). On one hand, criminology is a “hybrid product of several sciences” (Parmelee 1918:4), a self-styled “rendezvous” subject (Downes, *qtd.* in Garland & Sparks 2000) whose diffraction frustrates efforts to pin down the criminological ‘this’ from the non-criminological ‘that.’ On the other hand, it is unclear whether criminology even meets the bar of constituting a ‘discipline’ in the first instance (Savelsberg & Sampson 2002). If criminology refuses identification of this base sort, then ensuing attempts to model its role will similarly be off to a non-starter.

This paper takes seriously the problem that an inability to measure criminological thought presents. I first sketch the inescapable ubiquity of undifferentiated characterizations of criminological scholarship endemic to the study of penal discourse and criminal justice policy. Rather than adjudicating between the analytical imprecisions that those characterizations present, I instead propose a solution that leverages bibliometric tools in the service of systematic and formal modeling of criminological thought.

LITERATURE

Three approaches to specifying criminological thought's contours predominate. The first has been to define it by its *practitioners*. This has taken the form of identifying criminological thought by reference either to a detailed assessment of the work of individual scholars (*e.g.*, Beirne 1993; Cullen 2005; Daems 2009; Hagan 2012; Holdaway & Rock 1998; Mannheim 1960), or to a set of ideal-typical criminological tropes (*e.g.*, Loader & Sparks 2010). There is no shortage of typologies of this sort, although their analytic value is questionable (Wacquant 2011:445). In particular, defining criminology by those who practice it and how they do so obscures how the tropes that form the assessment's basis were selected. More worrisome still, such practitioner-based typologies also leave untroubled the assumption that the proposed tropes produce discernibly different criminologies—else the typology loses all its value. Maybe the assumption holds true; maybe not. But it would be prudent to treat the existence of such distinctions as testable hypotheses rather than as a priori commitments.

The second approach to operationalizing criminological thought focuses on its key *discontents*. There is a bewildering cornucopia of disagreement over criminology's key features and objects of interest. The complaints are legion: criminological thought is too right-wing, or it is too left-wing; it is too preoccupied with the state's concerns, or it has strayed from them; it is too bound up in its policy-informing mandate to be a science, or it is too obsessed with basic research to matter to those in the trenches; its mainstream is a factory of parochial flavor-of-the-month empiricisms, or it has lost touch with the theoretical vibrancy befitting a serious science. Thus, systematic efforts to offer authoritative statements about the state of the field commonly note a dizzying array of incompatible gravamens. These include, among other things, criminology's ahistoricism, its statist fealties, its positivist trappings, its elision of the dynamics of marginalization, its technocratic narrowing of the frame of questions worth answering, its obscurantism, its analytical imprecisions, its quantiphilia, its ideological predilection for 'root causes' over policy levers, and its perseverations over processes of meaning-making instead of intervention (*e.g.*, Cohen 1988; Scheingold 1998; Garland 1992; Phillips & Bowling 2003; Rafter 2010; Savelsberg 1994; Savelsberg & Flood 2004; 2011; Savelsberg et al. 2002; 2004; Wilson 1983; Wright & DeLisi 2016; Young 2011).

To be sure, complaints of the foregoing sort are sometimes necessary maneuvers in the pursuit of centrality in intellectual knowledge-space. This is all the more so for marginalized research programs seeking to establish legitimacy, which must do so by first articulating the status quo's inadequacy (Frickel & Gross 2005). But it is sociologically precarious to evaluate criminological thought pursuant to cultivating legitimacy for a new research program. One may well find what one sets out to prove.

The third approach appraises criminological thought by its *determinants and effects*. Here, criminologists and penologists model the contours of criminological thought according to a 'rational-functionalist,' 'symbolic-functionalist,' or 'concept-formation' model. Each model recognizes that criminological knowledge can be a key driver of penal discourse, criminal justice policy, or both; however, in characterizing criminological thought, they do so by simplifying and flattening its contours. Criminology, in each model, remains an un-operationalized variable.

First, a rational-functionalist model imagines that criminological thought's ambition is to yield credible claims about policies that 'work' so that justice policy might proceed rationally. Policymaking happens best, so the rational-functionalist model counsels, when it defers to

criminological expertise (Tonry 2013). It alternately whistles in major key about criminology's "promise" to advance knowledge about credible effects and sound policy (*e.g.*, Blumstein 1994; Cullen 2005; Petersilia 2003; Sherman 2009), and in minor key about the pathologies of a penal system that failed to heed criminology's warnings about evidence-based practices (*e.g.*, Barkow 2019; Bergin 2013; Kleiman 2008). Both cadences ultimately land on the same note: criminology is in the business of measuring observable causes and effects; to the extent that penal policy intervenes in the social world, it should do so underpinned by the 'best' evidence; and mass incarceration's grand tune of penal failure was one of policymakers declining to pay criminology's pied pipers.

Second, a symbolic-functionalist model theorizes criminology as influencing policy, if at all, only to the extent that the ideas it supplies meet an underlying symbolic need. There are as many variants of the symbolic-functionalist model as there are actors or symbolic needs: some theorize that criminological thought is incorporated into justice policy in those instances when it provides theoretical framings of crime and criminals that resonate with a political zeitgeist (*e.g.*, Gottschalk 2015; Hagan 2012); others theorize that criminological expertise enters the political field only to the extent that its deployment by justice actors supplies capital in the form of legitimacy, typically during moments characterized by prevailing institutional uncertainty (*e.g.*, Jenness & Grattet 2001; Rubin 2019; Willis & Mastrofski 2011); still others theorize that criminological thought acquires discursive resonance once it validates an underlying priority to, say, quell racial threat and stoke animus (*e.g.*, Murakawa 2014; Tonry 1995), meet labor needs (*e.g.*, Gilmore 2007; McLennan 2008), appeal to penal appetites refracted through variegated political-economic forms (*e.g.*, Barker 2009; Lacey 2008), or advance positivist imperatives (*e.g.*, Beirne 1987; Reiner 1988).

Of course, there are applications of the symbolic-functionalist model that argue from the null hypothesis. In these accounts, criminological thought bears no relationship to policy precisely because it failed to supply ideas that met an underlying symbolic need. At different times, scholars from the left and right alike have assailed criminology's intellectual sterility and inaptitude to the present moment (*e.g.*, Cohen 1988; Lyons & Scheingold 2000; Wilson 1983; Zimring 2017); in other instances, criminology's inability to reach scientific consensus or the field's meager prestige accounted for the low demand for any symbols criminology might have supplied (Koehler 2015; Leon 2011). In these accounts criminological thought rings hollow as having had few worthwhile ideas to offer—best in those moments to have left its lessons be.

Third, a concept-formation model imagines penal discourse as mediating the relationship between criminological thought and penal policymaking. In its narrower formulations, criminological thought innovates penal techniques in ways that shift the penal commonsense toward taking those techniques seriously—examples include the indeterminate sentence (Pifferi 2016) or eugenics (Rafter 2007) or institutional forecasting (Hinton 2016) or actuarial justice (Rothschild-Elyassi et al. 2019). In its broader formulations, the concept-formation model collapses criminological thought together with other discursive forms such as, say, public opinion or law. For example, some influential accounts reject the tidy pathways of cultural transmission that draw one directional arrow from criminological knowledge to penal discourse before drawing yet another one to criminal justice policy; instead, criminological thought expresses, co-constitutes, or sometimes even follows a new penal commonsense—for example, one grounded in exclusion (Muhammad 2010), neoliberal penalty (Harcourt 2011; Wacquant 2009; Xenakis & Cheliotis 2018), risk governance (Simon 2007), or incapacitation (Zimring & Hawkins 1997).

The models sketched out above encompass a dizzying array of criminological research, and the foregoing taxonomy barely scratches the surface. Rather than adjudicating their relative validity, they appear here because of one similarity that is uniform throughout: they each infer criminology's contours from the discursive or political outcomes they produce. This approach closes off the possibility that different criminologies of different stripes may have positioned themselves as deserving intellectual attention at given moments, and instead assumes that criminology emitted an undifferentiable signal that policymakers either accepted or declined. A corollary of this is that *all models inescapably characterize 'criminology' as an un-operationalized variable*. Although its role, determinants, and effects vary, each model treats criminological thought as a centrally important feature. Yet they each cabin off their shortcomings and proceed as though a passing impression of criminological thought satisfactorily stands in for a thorough appraisal of one. Methodologically, this perspective foregrounds *whether* criminology attracted attention rather than *which* criminology resonated and *what* it looked like when it did so. This is selection on the dependent variable, not rigorous appraisal.

Consequently, characterizing criminology's aims, ambit, and effects remains as inescapable as it is stubbornly elusive. Such characterizations—what is criminology, what is it for, for whom, how should it be done, and what does it do—have necessarily leant on one of two approaches. The first is the practice of imagining criminology as an undifferentiated monolith, a deliberately simplified stand-in for an admittedly more complicated whole. If the ambition is to refer to a prevailing 'mainstream,' then this approach may present few problems as long as that designation is indeed accurate. But without systematic tools to guide the determination of criminological thought's general structure, accurately pinpointing a field's mainstream could be a tall order. In the second, the common practice instead is to treat an isolated part shorn from the whole as the operative stand-in for criminological thought. If the invoked part is all the model requires, then again, there may be few problems here, either. If, on the other hand, the part is offered as a stand-in for the whole, then the operative unit misrepresents the isolate for the general. The consequences of slippages of this sort can be momentous. Both approaches make trade-offs, and neither is ideal. Yet, the simplification at the core of both approaches arises from the unmet shared need for tools to measure formally criminological thought's contours.

The analysis that follows is an effort to calm the disquiet that this unmet need inspires. The perspective I advance appraises the contours of criminological thought on criminology's own terms. By organizing scholarship into its constituent idea-sets, I provide a framework for measuring key concepts such as criminological thought's 'mainstream(s)' or its 'substantive foci' or its 'commitments'—whether methodological, epistemological, normative, or otherwise. The operative unit of analysis in measuring criminological thought, then, departs from the approaches that prevail in extant analyses that refer to criminology's *practitioners*, its *discontents*, and its *determinants and effects*. In what follows, I instead focus on the criminological *ideas* that serve both as the currency of criminological scholarship and as the vector of criminological thought itself. To that end, I build an analysis of criminological thought with the citation as the primary building block for the study of intellectual engagement.

METHODS

Criminology has been fascinated with citation analysis for more than four decades. In its earliest instantiations, criminologists tallied the frequency of "direct citations" that criminological texts garnered and were disheartened by the observed lack of putative influence that those citation

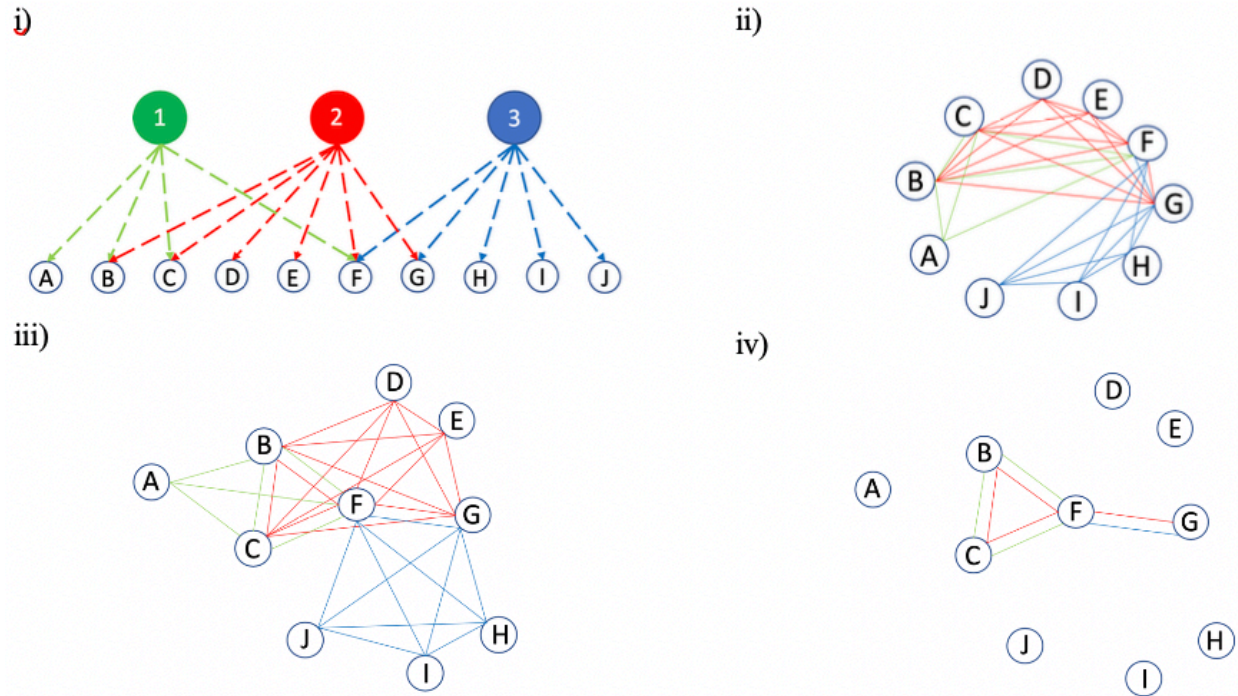
counts reflected (Green 1981; Wolfgang et al. 1978). Despite well-established and extensive qualms about the methodological weaknesses of direct citation tallies (Wolfgang et al. 1978: Ch.3), the practice nonetheless flourished in criminology. Its use has since served as the bedrock for a formidable industry of criminological analyses purporting to measure scholarly influence and productivity, intellectual reputation, and scientific quality: direct citation counts appear as an index of prestige and fashion as viewed through the lens of journals (Cohn et al. 1998; Poole & Regoli 1981; Rock 2005; Wright et al. 2012), texts (Cohn et al. 2018; Gabbidon & Collins 2012), scholars (Cohn 2011; Cohn et al. 2014; Wright 2000), departments (Cohn & Farrington 2006), geographical regions (Farrington et al. 2019; Zhang 2017), and methodological and substantive foci (Cohn & Farrington 1990; Rice et al. 2007; Wright & Friedrichs 2006; Wright & Miller 1998).

However, bibliometricians frown upon the practice of tallying citation counts that criminologists celebrate. Citation frequency may be a useful place to begin if a researcher seeks to identify a scholar, article, journal, department, or otherwise that accrues high visibility. To be sure, counting the frequency of direct citations indicates engagement with, and the prominence or salience of a document's key ideas. But direct citation captures precious little about field-level dynamics pertaining to broad conversations spanning engagements that cross between texts.

More practically, bibliometrics is alert to how tallying citation frequency presents both a methodological problem and a hermeneutic one. Methodologically, direct citation is temporally constrained: it links a citing text to a reference text that antedates the citing text. Direct citation thus misses how information is arranged into networks, such as when two contemporaneous reference texts are vectors for a similar underlying idea. By implication, direct citation analysis also requires extremely long time windows to generate sequences of citing-to-cited texts on which inferences about intellectual similarity might rest. The more salient hermeneutic problem, however, is that *texts* do not carry ideas; *our reading* of those texts does. Direct citation mistakenly draws attention away from the behavior in which we are interested (the *act* of citing) and toward the object of that behavior (the reference text). Postmodernists counsel that the same text that carried one meaning at one time can transform into another according to shifts in cultural and institutional milieux. Tallying citation frequencies improperly flattens these contingent valences of a text into a chronologically undifferentiated raw score.

Therefore, the operative unit of analysis in mapping scientific structure requires techniques that reveal the patterns *between* texts, not just *within* them, that direct citation analysis hides. Thus, what is needed is a measure of how texts rely on combinations of foregoing documents, as a process of constituting knowledge domains. "Co-citation" serves this purpose. A co-citation is a tie between two documents that have been cited by the same document (Small 1973). Because of co-citation's dependence on the third work, the strength—measured as the frequency of co-citations—will both determine the proximity of content between two documents, and will also vary as new literature is added to the network. It implies one thing to count direct citations to, say, *The Bell Curve* (which prescribed tailoring policies to sub-populations sorted according to intellectual capacities). But it implies much more to compare when *The Bell Curve* was cited alongside *Crime and Human Nature* (which called for incapacitative justice policy to contain immutable offenders) to those instances when *The Bell Curve* was instead cited alongside *Inequality by Design: Cracking the Bell Curve Myth* (which exhaustively refuted *The Bell Curve*'s claims).

Figure 1.1: THE MECHANICS OF CITATION AND CO-CITATION. Inset i) illustrates direct citation, wherein citing texts “1,” “2,” and “3” cite reference texts “A” through “J”; ii) represents the co-citation ties produced by the same citation pattern; iii) spatializes reference text proximity based on co-citation frequency; iv) sets the threshold co-citation frequency at ≥ 2 .



A simplified model illustrates the mechanics of citation and co-citation analysis. Figure 1.1i presents an example dataset comprising three citing texts labeled “1,” “2,” and “3” that cite to ten reference texts labeled “A” through “J.” Those reference texts vary in being cited once (“A,” “D,” “E,” “H,” “I,” and “J”), twice (“B,” “C,” and “G”), or thrice (in one instance, for “F”). Direct citation analysis of this sort permits inferences about, say, the increased engagement with “F” compared to the rest of the data, but little else. A co-citation network using the same pattern of citations appears in Figure 1.1ii. Note, first, the ties that now connect reference texts that had also been cited by the same citing text. Note also the proliferation of ties that converting a simple direct citation network to its corresponding co-citation network implicates—15 direct citation ties translate to 31 co-citation ties. The same co-citation network can then be spatially re-arranged as in Figure 1.1iii, so that between-text distance corresponds to textual affinity. Consequently, reference texts “A,” “D,” “E,” “H,” “I,” and “J” are peripheral to the network, whereas “F” is central. Setting a threshold number of co-citations to exclude singular instances of a co-citation reveals the network in Figure 1.1iv. The inferences available in 1iv are much more powerful than the direct citation analysis that 1i made possible: first, a co-citation cluster emerges connecting “B,” “C,” and “F” and a separate cluster connects “F” and “G.” This indicates, first, that there are two separate sets of conversations that bind those texts together; second, co-citation analysis reveals that “F” serves as the conduit between those two sets of conversations.

The capacity of co-citation analysis to identify conversations occurring between texts makes mapping the contours of criminological thought possible. Cluster analysis reveals socially-networked patterns in textual affinity, as in Louvain community detection algorithms, and in trajectories of intellectual engagement, as in k -means clustering. Although this is the first application of such tools to criminological thought, similar techniques have been applied elsewhere

to map disciplinary thought-domains (*e.g.*, for chemistry, *see* Boyack et al. 2008; for computer science, *see* White & McCain 1998; for history and philosophy of science, *see* Weingart 2015; for production and operations management, *see* Pilkington & Liston-Heyes 1999; for sociology, *see* Moody & Light 2006).

A Louvain community-detection algorithm classifies and groups reference texts on the basis of common co-citation patterns. A Louvain algorithm yields clusters that maximize intra-cluster similarity by associating like with like and minimize inter-cluster similarity by differentiating unlike from unlike. The algorithm optimizes the density of edges inside a community compared to those outside the community by finding a large number of small communities of texts grouped into ‘idea-clusters.’ These idea-clusters can then be hand-coded according to the ideational theme unifying the most-cited texts in that node. *Within-cluster* ties denote a co-citation between texts that both fall within the same cluster, and *boundary-crossing* ties denote co-citation between texts that fall between clusters. The algorithm then iterates to maximize network modularity, treating the communities it detected in its first ‘sweep’ as the nodes from which to form higher-level communities of ‘idea-regions.’ It repeats the process of aggregating the nodes into larger communities through additional sweeps until no further statistically significant optimizations are possible (for further technical explication, *see* Börner et al. 2005; Trujillo & Long 2018).

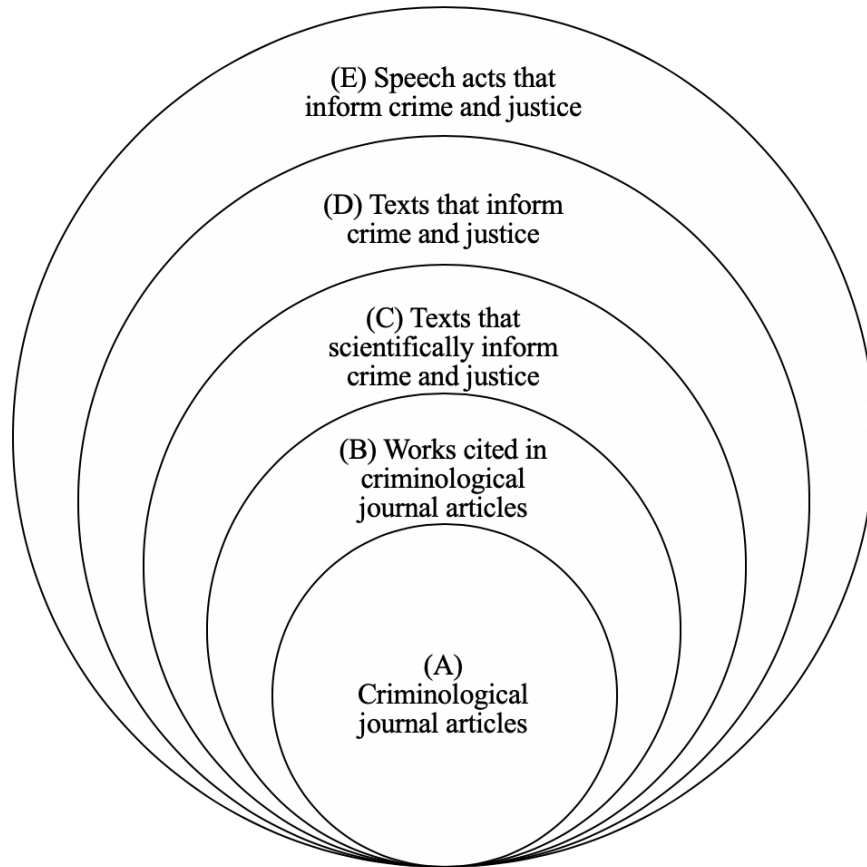
After classifying reference texts into idea-clusters and idea-regions, I model how and when those ideas emerge, acquire salience, stabilize, and wither. To this end, *k*-means clustering is an unsupervised learning technique for classifying a sample into groups: first, for each specified number of groups *k*, the algorithm randomly selects a point as an initial cluster mean; second, it minimizes the sum of squared Euclidean distances between an item and that randomly-designated mean; third, it then updates the center-point by calculating new mean values of all points in the cluster; finally, it repeats the process until cluster assignments no longer change, at which point the algorithm reaches “convergence.” Thus, *k*-means clustering repeatedly calculates distances between a point and a cluster’s designated center until no new center is nearer than the one to which it has already been assigned by minimizing the total within-cluster variation.

A concern with *k*-means techniques is that the researcher must specify the number of *k* trajectory groups before the clustering algorithm may proceed. Silhouette tools can guide this determination by providing a measure of “separation distance,” which indicates how well an object lies in its cluster across different values of *k*. High coefficients indicate distance from the decision boundary; low coefficients indicate proximity to the decision boundary. Sweeping across parameter *k* reveals the point at which increasing *k* yields diminishing marginal returns in minimizing separation distance, and thus the optimal number of clusters (for further technical explication, *see* Kauffman & Rousseeuw 2005).

DATA

The citation data forming the basis of the following analysis have been taken from the *Web of Science* (hereafter “*WoS*”). The *WoS* hosts comprehensive citation indices for social science journals aggregated by disciplinary sub-categories. I retrieved metadata for every Anglophone article contained within the category “Criminology & Penology” as of 19th September, 2018. The dataset comprised 61,551 discrete documents published in 65 journals, with a total of 231,083 bibliographical references. They extended from 1910 to the present, with seven years of missing data (1912, 1913, 1924, 1952, 1953, 1954, and 1955). This represents the largest corpus of criminological scholarship analyzed in any one study to date.

Figure 1.2: THE *WEB OF SCIENCE*'S CRIMINOLOGICAL HOLDINGS. The *Web of Science* contains metadata from journal articles designated "Criminology & Penology" (A); they in turn contain identification data on works cited therein (B); they in turn constitute texts drawn from among those capable of scientifically informing crime and justice (C); they in turn are the scientific subset of texts that inform the cultural production of crime and justice (D); they in turn comprise the textual subset of speech acts that inform the cultural production of crime and justice (E).



WoS data are well-suited to the present research design. Evaluation research that compares *WoS* to its competitor databases *Google Scholar* (hereafter "*GS*") and *Scopus* concludes that *GS*'s coverage is broadest for most disciplines and that *WoS* and *Scopus* fare similarly to one another (Harzing & Alakangas 2016). *GS* out-performs other databases in its collection of conference presentations and non-Anglophone materials, and in its assimilation of historical materials. Consequently, *GS* inflates citation estimates observed using *WoS* data (Amara & Landry 2012), and it outpaces *WoS*'s negligible retroactive expansion practices (De Winter et al. 2014). But it is important not to exaggerate *GS*'s putative superiority to *WoS*. The labor costs of extracting *GS* metadata—especially for particularly large datasets, as is the case here—have proven unworkably intensive compared to the trivial marginal benefit that those data yield when compared to *WoS*'s holdings (Meho & Yang 2007). Moreover, *WoS*'s historical archive extending to the early-twentieth century is one of its particular strengths (Li et al. 2010). Furthermore, the present research design mitigates the foregoing concerns in two ways. First, the sample includes reference texts appearing only in Anglophone citing articles from a period during which *WoS* archiving practices were already stably regimented. *GS*'s ostensibly superior holdings therefore target literatures excluded from this analysis by design. Second, both databases are reliably consistent

(Franceschet 2009), which supports inferences where, as here, the analysis relies on secular dynamics internal to criminology rather than comparisons between disciplines.

The data structure is bimodal: the first mode refers to the 61,551 *citing articles* with corresponding metadata; the second mode refers to the 231,083 *reference texts* contained in the works cited list from the retrieved documents. The references contained within the *WoS* data format comprise each cited reference's first author, year of publication, an abbreviated title, an optional entry code designating the periodical venue whenever the cited reference is a journal article, and the cited page. The bimodal structure permits analyses of two different sorts operating at two different levels of granularity. *Articles* are published documents confined to the *WoS* capture techniques. *References*, on the other hand, are those documents that the *WoS* articles have themselves cited, and are not limited to periodical articles appearing within the *WoS* dataset. In this analysis, *articles* are therefore a stepping stone to produce the sample of true interest, namely the much more diverse set of *references* cited in criminological journal articles and on which criminological thought rests. The sampling frame from which reference texts (Figure 1.2B) are drawn is the set of texts that scholars producing articles in criminological journals have determined hold scientific value (Figure 1.2C), which is itself a subset of the universe of texts that inform thinking about crime and justice (Figure 1.2D).

Reference texts were arranged into a co-citation network, which required deleting references that had been cited only once or which had not been cited in connection with any other text from which to form a co-citation. The resultant network comprised 1,333,599 co-citations.

RESULTS

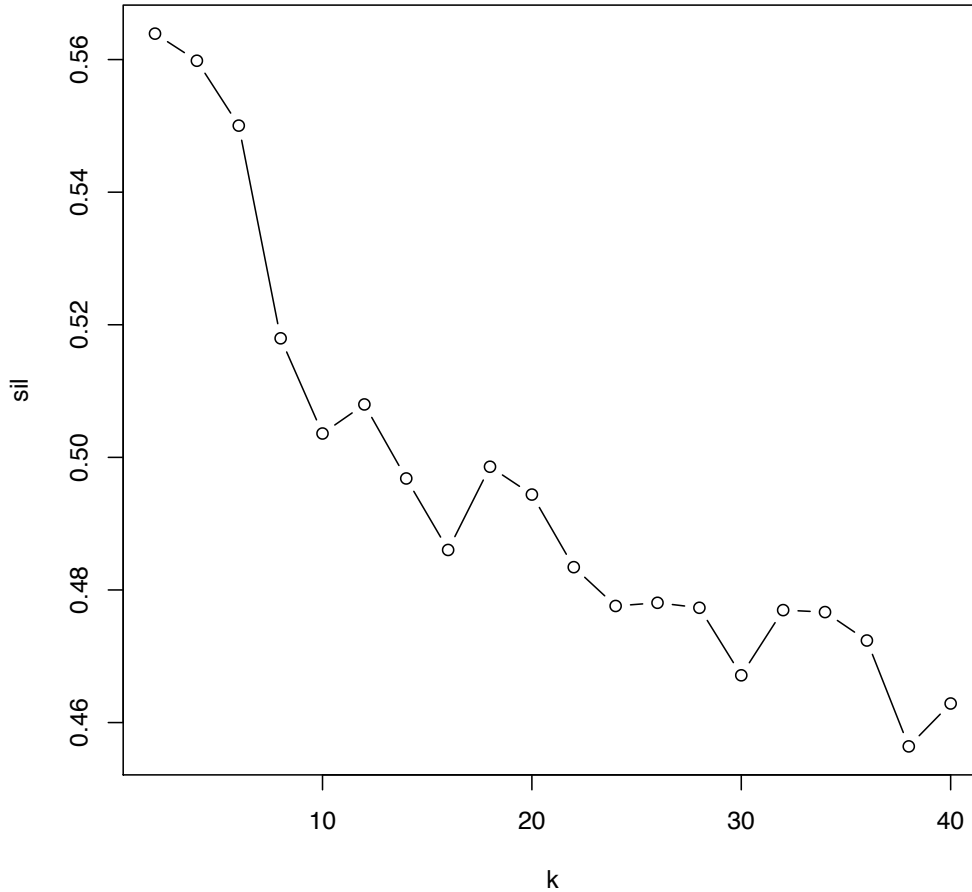
The Louvain algorithm resulted in a total of four iterated sweeps. The first sweep detected 8,148 communities of 'idea-clusters'; by the final sweep the algorithm had detected 898 communities of 'idea-regions'. The more granular network of approximately 8,100 idea-clusters was coded to each cluster's respective membership within the approximately 900 less granular aggregated idea-regions. Citations to idea-regions then served as the basis on which to calculate *k*-means groups. The results of the silhouette analysis, which appear in Figure 1.3, revealed that clustering idea-regions into 16 groups optimized separation distance. Robustness checks revealed that the addition of more groups to the analysis served only to disaggregate trajectories at the very lowest end of the sample, and thus did not substantially alter the findings.

Figure 1.4 groups those clusters into the six trajectories that are arrayed in respective inset panels. Citations to idea-regions contained across all sixteen clusters appearing in Figure 1.4 are sparse in early years, averaging fewer than ten annually. In Figure 1.4a, idea-regions break this threshold in the 1950s; idea-regions in other clusters do so from roughly 1970 onward. Generally, however, all sixteen clusters across six trajectories arrayed in Figure 1.4 resist hierarchical stratification until roughly 1960, at which point the trajectories disperse. From then onward, there is wide variation in citation patterns to criminological ideas.

Figure 1.4a shows that clusters 8, 11, and 14 comprised a total of 0.6% of the idea-regions in the sample and received the most citations throughout, culminating in a combined accretion upward of 50,000 annual citations in recent years—dwarfing citations to the idea-regions in other trajectories. Figure 1.4b shows that clusters 1, 3, and 4 comprised a total of 21.9% of the idea-regions in the sample and accumulated citations in the middle of the distribution throughout. Figure 1.4c shows that clusters 9 and 2 comprised a total of 15.4% of the idea-regions in the sample and received the fewest citations throughout. Although the trajectories arrayed in Figures 4a, 4b, and

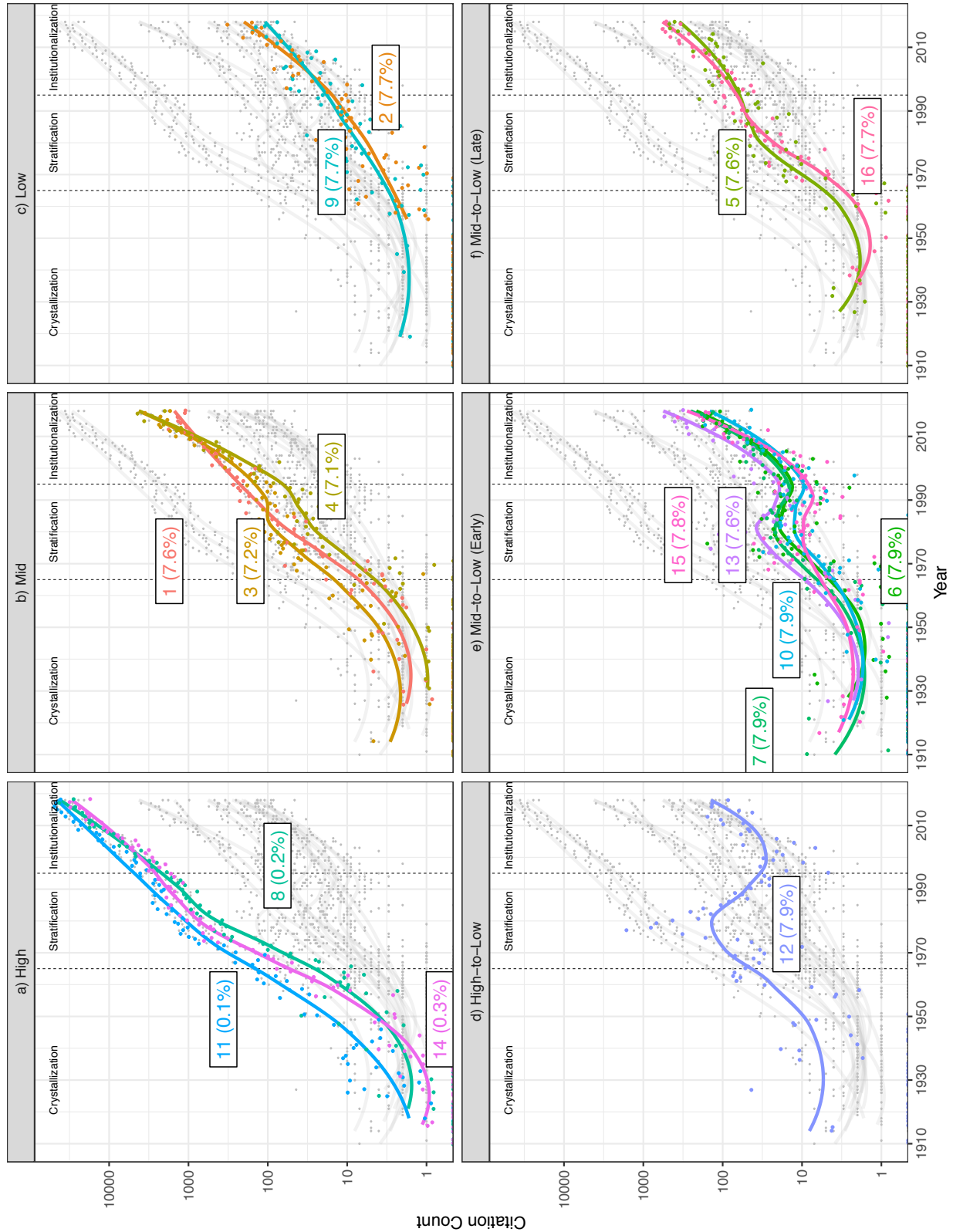
4c are stratified into three divergent levels of high, medium, and low annual citation frequencies, they increase throughout the sample period.

Figure 1.3: SILHOUETTE PARAMETER SWEEP FOR K-MEANS CLUSTERING. The marginal value of adding clusters discernibly diminishes after 16 clusters.



Three trajectories arrayed in Figures 4d, 4e, and 4f contain clusters whose citation patterns shift toward a lower frequency from a higher one. Trajectories in Figure 1.4d (comprising cluster 12, beginning with a high citation frequency) and 4e (comprising clusters 6, 7, 10, 13, and 15, beginning with a medium citation frequency) both fall in the mid-1970s to a level comparable to that observed in Figure 1.4c, whereupon from that point onward they receive a level of engagement commensurate with the lowest frequency in the distribution. Together, this contemporaneous shift accounts for 47.0% of the idea-regions in the sample. The remaining clusters that appear in Figure 1.4f (comprising clusters 5 and 16) also fall from the medium citation frequency to the low one, but this decline is more gradual and is complete only in very recent years. By 2017, 62.3% of all idea-regions are among the low citation frequency clusters in the sample and receive an average of approximately 250 to 400 annual citations each.

Figure 1.4: FASHIONS OF CRIMINOLOGICAL THOUGHT, 1910–2017. Logged citation counts to idea-regions in 16 clusters. Parenthetical values correspond to that cluster’s share of idea-regions—e.g., Cluster 1 in Panel 4b contains 7.6% of the idea-regions in the sample.



The largest and most central idea-cluster, labeled ‘OffRehab,’ appears toward the middle and corresponds to scholarship on offender rehabilitation. A much smaller set of similarly-colored orange circles appears to the southwest of OffRehab, indicating intellectual affinity in the texts covered albeit in a manner that is repelled from the central position that OffRehab holds. These idea-clusters refer to psychological analyses of specialized offending patterns such as sex offending, teen sex offending, cognitive distortions literature, and the actuarial risk assessment of sex offending. Nearby are the idea-clusters referring to scholarship on family violence and sexual assault victimization. These orange idea-clusters collectively form part of the ‘Forensic Psychology’ idea-region. This suggests that the community detection algorithm accurately sorted literatures into idea-regions that meet reasonable standards of face-validity.

A regional idea-set of six green idea-clusters encircles OffRehab, and this idea-region collectively forms ‘Criminological Theories.’ They correspond to dominant theoretical orientations in criminology, respectively: strain theory; deterrence theory; life-course and developmental approaches; desistance research; and the general self-control offending theory. Toward the network’s eastern hemisphere, the scholarship shifts both with regard to disciplinary orientation and substantive focus. It trades the psychological and correctional interests of the more network-central idea-regions for scholarship that is instead more sociological, critical, and police-focused. The light-green region corresponding to ‘Policing’ includes three nodes that refer to research on police practice, theory, and legitimacy. They encircle one of the largest nodes in the network, which refers to a solitary blue idea-cluster labeled ‘CritCrim’ comprising critical approaches to crime and justice.

A pink idea-region in the center of the network corresponds to articulations, applications, and extensions of ‘Social Disorganization Theory’—culminating most recently in collective efficacy theory. It contains a high frequency of both within-cluster co-citations and boundary-crossing ones. The former indicate intellectual density and a strongly coherent research program; the latter indicate that this idea-region serves as a connective bridge between the green idea-region corresponding to ‘Criminological Theories,’ the light-green idea-region corresponding to ‘Policing,’ and the blue ‘CritCrim’ idea-cluster.

More peripherally, the network also includes a long tail of four idea-clusters in the southern hemisphere that have been repelled from criminological thought’s mainstream. This idea-region corresponds to legal research on ‘Courtroom Processes’ including deception, eyewitness testimony, and the trustworthiness of child witnesses.

The six trajectories arrayed in Figure 1.4 guided disaggregating the global network into three chronological periods. In turn, these were characterized by criminology’s crystallization, then its stratification, then culminating in its institutionalization. An abbreviated sketch of the networks from each period follows:

- *Crystallization* (1910–1965): In the first period, trajectories are not yet arranged into strata of high, medium, and low levels of intellectual engagement. So, too, the content of criminological ideas is also yet to crystallize into coherent topical domains. Idea-clusters that attract the most attention during this period generally concern personality measurement techniques—chiefly in service to the design, validation, and theorization of parole prediction instruments. This corrections-centric research program eclipses the otherwise-scant offerings in criminological epidemiology and etiology, as it does research on police administration.

- However, the low frequency of both within-cluster and boundary-crossing co-citations during this period cautions against assigning a firm identity to any criminological research program.
- *Stratification* (1965–1995): In the second period, criminology both divides into three distinct layers of high, medium, and low trajectories, and at the same time it witnesses a field-wide relegation of topics into disfavor as intellectual tastes shift from those of yesteryear. Research on personality measurement and actuarial techniques that once proliferated during criminology’s ‘Crystallization’ are absorbed into a much larger and more densely connected network component whose topics also comprise theoretical and empirical investigations, particularly of “delinquent subcultures”; the establishment of programs of administrative and critical criminology concerned with the refinement of organs of state control; and incipient efforts to typologize offending and offense patterns. Legal scholarship, in particular the jurisprudences of search-and-seizure and of confinement conditions, emerges at first as an interest that is peripheral yet disconnected from criminology’s mainstream, but it attaches to the main network component by the early-1980s.
 - *Institutionalization* (1995–2017): At the most recent period’s outset, criminology’s hierarchical stratification is complete and settled into a sharp division between the ideas that receive attention and those that fall into obscurity. Within-cluster co-citations reach their peak during this period, as do boundary-crossing ones isolated to the same trajectory group (*i.e.*, ideas in a ‘high’ trajectory cite to ideas in other ‘high’ trajectories but not to ideas in ‘medium’ or ‘low’ trajectories, and vice versa). Criminology’s institutionalization is thus most apparent in the extent of that stratification: idea-regions that rise to the top of the distribution of intellectual engagement (in Figure 1.4a) consolidate into a dense network component that attracts an overwhelming share of the network’s total annual citations. The content of ideas in all three strata is thematically unbounded: all contain the full topical complement of research on policing, courts, and corrections. Accompanying the network’s prodigious expansion during these years is a commensurate proliferation of synthesis techniques and distillations of research findings—especially of policy evaluation research—though systematic reviews and meta-analyses.

DISCUSSION

There is a pressing need to identify the contours of criminological thought given its role among the key drivers of penal discourse and criminal justice policy. However, doing so requires the capacity to characterize criminology accurately. This, in turn, demands capturing field-wide changes in criminology’s shape and form, differentiating the ideas at criminology’s mainstream from those at its fringe, and evaluating the content of those ideas. The analysis herein of the most comprehensive dataset of criminological scholarship to date extending from 1910 to 2017 begins to meet that need.

The vast abundance of criminological knowledge accumulation, resting on the practice of citing from one criminological text to another that forewent it, is relatively new. This aligns with established wisdom about the proliferation of social sciences generally: neither had the institutional infrastructure been developed with which to formalize scientific production until the early-1960s, nor had conventional forms of scientific production crystallized like the ‘journal,’ the ‘citation,’ or for that matter the ‘discipline’ (for social sciences generally, *see* Abbott 2001; Ross 1991; for criminology and criminal justice specifically, *see* Morn 1995). Indeed, early efforts to cultivate legitimacy for an incipient “criminological science” depended on corralling justice practitioners into inaugurating, and publishing in, new criminological journals (Koehler

2015:520). To the extent that criminological scholarship relies upon the accumulation of new ideas resting atop foregoing ones, the data here suggest that criminological thought was yet to emerge and stabilize until at least the mid-‘60s.

The data both corroborate and complicate a conventional telling of early criminology’s crystallization. On one hand, the data bear out criminology’s topical preoccupation with actuarial and psychometric instruments that found its fullest expression among both the freshwater criminologists working in Midwest sociology departments and saltwater criminologists working in East Coast law schools alike. The rise of personality measurement during this period prefigured both criminology’s medico-legal commitment to the scientific control of the dangerous, and its technocratic commitment to the classification and supervision of risky and proto-delinquent groups (Garland 2002). On the other hand, however, those commitments have been understood as developing separately from one another, haphazardly and without the formal trappings of an outrightly ‘criminological’ disciplinary identity. The data reveal a different picture. Neither do criminology’s “governmental” products appear separate from its “Lombrosian” ones, nor does either one rise to the threshold of an established criminological current. Rather, they were detected as forming the same comingled and indivisible community of idea-clusters. A more accurate characterization of contemporary scholarship would instead hold that flavors of each appear in the same—altogether undifferentiated—mass. Indeed, early criminology’s defining feature is its eschewal of substantive guideposts around which knowledge is supposed to take shape.

However, the field witnessed dramatic changes from 1965 onward. Here, criminology’s stratification is remarkable not only for its rapidity, occurring within the compressed timeframe of a single generation, but also for its field-wide relegation of a significant bulk of the literature to low-prestige status. It is thus during this period that criminology’s ‘mainstream’ first emerges. In part, this is a function of the proliferation of scientific knowledge in the context of finite attention space: as more literature becomes available, so too more of it must perforce go unread (Xie & Killewald 2012). But criminology’s stratification evinces other dynamics, too, visible above all in the content of its mainstream ideas. Funding swelled on both sides of the Atlantic for criminological research, evident in the establishment of the American LEAA in 1968 (Feeley & Sarat 1981) and in the UK Home Office Research Unit in 1957 (Rock 1988), both of which galvanized new modes of penal thought. In particular, Great Society liberalism inaugurated both a ‘systems’ research approach to the refinement of state control and a ‘due process’ preoccupation with legalistic principles of fairness, transparency, and justice (Harcourt 2018; Mayeux 2018). Although both might have found home in the ensuing irruption of criminological thought, nonetheless the data bear out that it was the systems approach that would eventually outstrip the due process one.

On one hand, the emergence of systems analysis as the cornerstone of criminological thought over the course of the late-‘60s and early- to mid-‘70s soft-pedaled the due process legalism that might have been its twin had it the chance. For example, the relegation of legal research to Figure 1.5’s southern hemisphere is striking. Concern about the severance of research on law from criminology has a long tradition (Sykes 1974), which notes among other things that the schism separating criminology from criminal law during the 1970s resulted in the former jettisoning concerns about court operations. Perhaps this was to the field’s detriment, as questions about harm, fault, and moral wrongdoing were ‘parceled out’ of the scientist’s repertoire. The questions that fell more squarely within the criminologist’s ambit, so the argument follows, were questions about the measurement and causal identification of observable effects—the very substance that

eventually predominates in the network's central, mainstream region. The long vestigial tail toward the network's southern hemisphere in Figure 1.5, then, is a reminder of positivism's erasure of what was once a central preoccupation in criminological thought: the study of keeping justice systems just.

Another triumph of systems analysis over criminological knowledge is its placement of administrative criminology at the field's core and critique at its periphery. From the late-'60s onward, as mass incarceration emerged to whet stirring penal appetites, criminological study underwent its 'Big Bang' (Reiner 2002). Criminology's ensuing epistemological and political reconfiguration rent the foundations of policing and corrections research from one another. The cracks that the Big Bang left behind in those foundations remain, as disciplinary and topical fissures abound throughout the network of criminological thought from 1965 onward. This finding does not sit easily with assessments that celebrate criminology's disciplinary and topical heterodoxy, diversity, and eschewal of any particular research program's 'dominance' (e.g., Rock 2011). But with regard to the substantive content of criminological thought, the layout suggests a clear axis from the network's western hemisphere to its eastern one.

That equatorial differentiation supports two inferences. First, the east-west axis suggests that criminological thought is generally divisible into either disciplinary foci, extending from psychological to sociological research, or topical foci, extending from correctional to policing research. However, the aggregation of idea-clusters into consolidated color-coded idea-regions also indicates that the neat east-west differentiation captures little of the between-cluster arrangement. To the contrary, although theoretical idea-clusters generally fall within the center of the network, they are not the most central or mainstream cluster at all; so, too, the most central cluster—offender rehabilitation scholarship—is most closely associated with idea-clusters that have themselves been relegated to a peripheral position in the network's southwest. Perhaps most glaring of all, two of the network's most central nodes—capturing research on family violence and the *Diagnostic Statistical Manual*—share disciplinary affinity to the clusters around them but are isolated from associated idea-clusters within the same region. To the extent that applications of the rational-functionalist, symbolic-functionalist, or concept-formation models invoke a topical 'region' of criminological thought, doing so is vulnerable to making claims that distort and collapse mainstream criminological ideas with peripheral ones. In this sense, the analysis of the contours of criminological thought presented above highlights the need for systematic measurements of key fissures between, and resonances among, criminological ideas.

The network also breaks with other received wisdoms. For example, some bemoan the discursive hegemony that critical criminology exercises over the discipline; others complain that critical perspectives have been relegated to criminology's periphery. Both of these concerns are overstated. On one hand, critical criminology is isolated from other nodes in its color region, with the closest kin being scholarship on migration and crime appearing far to the east of the network. Although it is a large idea-cluster, it finds few fellow travelers from the idea-region it inhabits from across the network's foremost topics. On the other hand, critical criminology's appearance among the 50 foremost idea-clusters alone suggests its vibrancy. Moreover, its position fits toward the periphery of the network's principal continent of mainstream ideas. The ferment connecting critical texts with those from other idea-clusters is hardly under threat.

As a very general matter, the small subset of the 50 largest idea-clusters comprises almost 60% of the total scholarship contained within over a century's holdings in the *WoS*'s "Criminology & Penology" catchment. This concentration suggests that criminological knowledge obeys similar

patterns of distribution that scholars have observed as a “law” of crime’s concentration (Weisburd 2015) more generally—so too, there are intellectual ‘hot spot’ ideas that generate the lion’s share of criminological discourse. To the extent that the rational-functionalist, symbolic-functionalist, or concept-formation models characterize criminological thought by reference to especially salient texts, this may not be a gross error if those selected texts are indeed principal vectors for criminology’s predominating ideas.

But matters are far less clear with regard to the identification of criminology’s marginalized ideas. The trajectories that contain the clusters of criminological thought receiving the lowest level of intellectual engagement (in Figure 1.4c) emerge only fairly recently, and they draw only trivial intellectual engagement until at least the mid-‘90s. From the perspective of field-wide dynamics, complaints about which ideas are relegated to obscurity are likely to miss the mark for a moving target since accurate assessments of which ideas inhabit the periphery in one year may well be out of date the next. Nonetheless, it is still worth taking heed of which ideas are *not* in the network’s mainstream. After all, the list of topics that have been structured out of criminological conversations is endless. For the time being, it should suffice to note that any effort to catalog and then remedy absence presupposes that the record is susceptible to systematic evaluation. The techniques showcased in this paper initiate an evaluation program of that very sort. The contours mapped herein illustrate both the lines of “agonistic” agreement over what counts as a criminological question worth pursuing, and by implication the questions that have “antagonistically” been taken off the proverbial table of acceptable criminological discourse (Koehler 2019).

Those lines dividing acceptable criminologies from unacceptable ones emerge only recently, at which point their stratification institutionalizes and takes on “a life of its own.” (Selznick 1949:10) Penal historians and students of science and technology studies alike caution against periodizing shifts in a field’s shape and form in this way. The former warn that sharp divisions of the ‘old’ from the ‘new’ mistakenly flatten underlying variations to reconstrue unsettled space as settled (Cheliotis 2006; Goodman et al. 2017; Rubin & Reiter 2018); the latter hold that perennial contestation is the scientific norm (Abbott 1995; Bourdieu 1975; Latour 1988). True, disputation and contestation abound in criminological thought. Moreover, the proliferation of boundary-crossing co-citations internal to each stratum in recent years attests to healthy ferment, not decay. But the disappearance of ferment *across* strata evident during criminology’s institutionalization speaks to the field’s narrowing of the scope of acceptable scientific discourse. This, above all, is the defining feature of modern criminological thought: criminologists nowadays have more to say—*much* more, in fact—but they nonetheless choose to do so while talking about ever so much less and less.

CONCLUSION

In this paper I present the first network map of the structure of criminological ideas. That map is based on an analysis of the largest dataset of criminological scholarship to date, encompassing more than a century of criminological scholarship. I do so in an effort to address the inescapable need to characterize criminological thought in ubiquitous efforts to theorize the determinants and effects of penal discourse, criminal justice policy, or both. Absent techniques of the sort employed herein, scholars have resorted to operationalizing criminology in ways that risk distorting or flattening criminological knowledge’s underlying variations. Co-citation analysis enables the classification of criminological ideas that permit defensible inferences about the contours of

criminological thought in general, distinctive criminological ideas in particular, and the fashions in how those ideas emerge, stabilize, and wither.

CHAPTER TWO: CRIMINOLOGY'S CRYSTALLIZATION AT BERKELEY¹

Abstract

In the early twentieth century, the University of California, Berkeley opened its doors to police professionals for instruction in “police science.” This program ultimately developed into a fully-fledged School of Criminology, whose graduates helped shape American criminology and criminal justice until well into the 1970s. Scholarship at the School eventually fractured into three distinct traditions: ‘administrative criminology’ applied scientific methods in pursuit of refining law enforcement practices, ‘law and society’ coupled legal scholarship with social scientific methods, and ‘radical criminology’ combined Marxist critiques of the state with community activism. Those scientific traditions relied on competing epistemic premises and normative aspirations, and they drew legitimacy from different sources. Drawing on oral histories and archival data permits a neo-institutional analysis of how each of these criminological traditions emerged, acquired stability, and subsided. The Berkeley School provides fertile ground to examine trends in the development of criminal justice as a profession, criminology as a discipline and its place in elite universities, the uncoupling of criminology from law and society scholarship, and criminal justice policy’s disenchantment with the academy. These legacies highlight how the development of modern criminology and the professionalization of American law enforcement find precedent in events that originate at Berkeley.

Keywords

History of criminology, professionalization, radical criminology, neo-institutional theory, legitimacy, institutional logic

¹ An earlier version of this chapter appeared as “Development and fracture of a discipline: Legacies of the School of Criminology at Berkeley” in *Criminology*, 51(3): 513–544. It appears here with permission.

In the early part of the twentieth century, the University of California, Berkeley became the site for an innovative experiment in the realization of a Progressive-era project. In 1916, the first Chief of the Berkeley Police Department, August Vollmer, pined for a new approach to the administration of law enforcement based on modern scientific techniques. He called for a School of Criminology for the special training of police officers at the neighboring university, citing its establishment as “a requirement of the times.” (Vollmer and Schneider, 1916:878) Those times necessitated a consideration of the role of law enforcement as a *profession*, and Vollmer fancied himself as its champion. The legacy that Vollmer left behind in his efforts to imbue criminology with the legitimacy and trappings of a traditional academic discipline and profession is a story of fits, starts, and frustrations. Over the course of 60 years, criminology at Berkeley experienced a tumultuous rise and fall. It rose to a pre-eminent position in both the establishment of criminology and the professionalization of justice administration, and it then descended to a position of notoriety in the policy sphere in the period immediately preceding its dissolution once liberal scholarship on law enforcement reform had been displaced to the Law School. Indeed, before the Criminology School’s dissolution in 1976, it had come to be seen as the embodiment of a generally prevailing *illegitimacy* endemic to criminal justice studies in elite institutions of higher learning.

The School of Criminology at Berkeley therefore provides fertile ground to examine important trends in the development of criminal justice as a profession; the legitimacy of criminology as a discipline, and its place on elite university campuses; the uncoupling of criminology from law and society; and criminal justice policy’s eventual disenchantment with the academy. This article charts all four of these trends, and in so doing demonstrates that the development of criminology and the professionalization of American law enforcement find precedent in events that originate at Berkeley.

The School owed its founding to the efforts of Vollmer and his students, who doggedly pursued an agenda of modernization in law enforcement without ever having provided an argument for the form that such modernization ought to take beyond rudimentary appeals to technocratic ideals. They formulated a vocationally oriented ‘administrative criminology’ that was oriented toward improving the administration of justice, but this departed from the approach to criminological knowledge production practiced elsewhere in the country. Failure to specify whether administrative criminology was a vocational or academic enterprise also created ruptures with the institutional support on which criminology could rely in its early years. Thus, the absence of a coherent institutional logic in administrative criminology resulted in a gradual fragmentation and stratification of police education into various groups, without a shared value set.

By the 1960s, the School witnessed a failure to maintain cohesion between the academic and the professional members of the discipline, and support for the School’s existence on the Berkeley campus diminished. Shortly thereafter, the Russell Sage Foundation launched a program of supporting scholarship that connected law with social science and funded the establishment of Berkeley’s Center for the Study of Law and Society. Many faculty members from the School of Criminology would be closely connected with that Center and it soon arose to prominence as a producer of ‘law and society’ scholarship (Wheeler, 1994). Contemporaneously, the School expanded its faculty and incubated ‘radical criminology,’ which was characterized both by a preoccupation with inequalities of power and class and by a commitment to social activism in the neighboring community. The three intellectual strands of scholarship represented within the School, administrative and radical criminologies and law and society, were not always harmonious; each professed a different set of epistemic premises about the nature and goals of

criminological scholarship, and they derived their legitimacy from different sources, depending on the institutional field they defined for the knowledge they produced.

THEORETICAL FRAMEWORK

Previously, the story of the Berkeley School has been told as a case study in either “the limits of academic tolerance” (Geis, 1995) or as a casualty of academic politics (Morn, 1995), or it has been studied with the purpose of piercing through the fog of acrimony to clarify who was to blame for its dissolution (Schwendinger and Schwendinger, 2014).² To do so, the interactions between scholars, research groups, and university offices are represented as though the dissolution of the School was a political response to satisfy individual or academic imperatives, such as the School’s increasing re-orientation away from providing professionally trained law enforcement officials ready to fill the needs of justice administration.

This analysis of the School is oriented toward a different purpose altogether. Applying a neo-institutional theoretical framework clarifies how criminology failed to articulate a system of values and norms around which professionals working in the field could coalesce, and that this failure resulted in continuing skepticism and bewilderment about the appropriateness and function of universities in facilitating law enforcement professionalization more broadly. Moreover, the institutional logics of the various and competing scientific research programs were shaped and managed within the organizational confines of the School so as to project an institutional identity of legitimacy. However, because the criminological traditions in question relied on competing institutional logics, the School is therefore theorized as part of a complex web of intellectual projects and bureaucratic constraints, and is viewed through multiple levels of units of analysis (comprising individual scholars, cohesive research programs, intellectual fields, and institutional imperatives).

The following departs from previous analyses by eschewing a functionalist interpretation of the viability of a scientific research program or profession by reference to how well it satisfies exigencies such as the need to provide efficient solutions (including, for example, the School’s ability to produce effective policemen). It also departs from an approach to the sociology of science associated with work by Bourdieu (1988; 2004) and Collins (1998). Those frameworks see the intellectual field as part of a competition for the scarce resource of scholarly attention – thus giving rise to Collins’ claim about the axiomatic co-existence of only three to six ‘schools’ of thought at a given time. This framework is deficient in two respects. Firstly, as noted by Lamont (2001), it imagines scholars as rational actors who seek to maximize their position within the intellectual field’s attention-space. It thus discounts the many possible reasons that may explain a scientific research program’s development and survival beyond the ability of scholars within that program to cultivate network centrality. Secondly, as shown by Savelsberg and Flood (2011), the wider political economy bears more directly on the shape that a policy-oriented field such as criminology takes than Collins observed in philosophy. A complete analysis must be alert to the ways that intellectual networks draw material support from institutions that in turn depend on a political-economic environment.

² I owe much to the impressive exposition of the School’s history found in these aforementioned efforts. However, as will be explained in detail below, they under-theorize the School by overlooking how its scientific research programs are themselves distinct units of analysis, and they elide an analysis of the search for legitimacy engaged by the institutions in question.

This study also sidesteps an analysis of the validity of the scientific claims made by scholars working in any of the research programs in question (for a similar approach to the historical analysis of concept formation in early criminology, see Beirne, 1993). Instead, the viability of a criminological scientific research program, understood as a coherent set of theoretically inter-related propositions about both the means and ends of criminological scholarship (Monk, 1988), is assessed by reference to how well it articulates an institutional logic capable of forestalling external critiques about its legitimacy. The very fact that a program of criminological education at Berkeley initially staked its survival on support provided by law enforcement highlights that law enforcement and criminal justice education deserve to be analyzed as mutually constitutive *institutions* that occupy and shape a *field* and must be theorized accordingly. This requires conceptualizing the School in terms of the fields it engaged, including professional law enforcement and the varieties of academic criminology housed within the School.

The notion of an institutional logic arises from the premise in neo-institutional theory that “to understand individual and organizational behavior, it must be located in a social and institutional context, and this institutional context both regularizes behavior and provides opportunity for agency and change.” (Thornton and Ocasio, 2008: 101-102) To that end, an institutional logic is typically understood as the set of practices and symbolic constructions through which actors in an organization communicate legitimacy within the institutional field (Friedland and Alford, 1991; Thornton et al., 2012). Such logics are managed, altered, and contested both throughout the field more broadly and in precarious instances within the organization itself. For example, Townley’s (1997) analysis of performance appraisal in the university shows how academic actors can deploy multiple logics, some of which compete with or complement one another, to contest the legitimacy of their practices and identities in response to institutional pressures. Therefore, according to the institutional logics perspective, external political-economic pressures influence practices within an organization such as the university by affecting the way actors marshal claims to authority and legitimacy in pursuit of organizational change or stability.

Frickel and Gross (2005) borrow concepts from neo-institutional theory to develop a theoretical framework that explains the success or failure of collective efforts to pursue research projects in the face of resistance from others in the scientific community. They propose that a scientific movement, such as each of the varieties of criminology analyzed below, emerges by distinguishing itself as a legitimate departure from conventional discursive and intellectual practice. Its survival and prestige depends upon actors’ capacity to articulate an institutional logic that circumscribes the field from which it may derive legitimacy. Like the organization(s) in which such a movement is housed, this political challenge requires ongoing coordination within the institutional field to sustain the movement and acquire stability (Frickel and Gross, 2005). The theory therefore predicts that *a scientific research program is most likely to emerge, gain prestige, and achieve institutional stability when actors display an initial motivation to depart from the prevailing intellectual tendencies of the day, when structural conditions facilitate opportunities for collective action, and when an institutional logic emerges with which to secure support from those who inhabit the intellectual field.*

To illustrate, scholars working in administrative criminology nurtured closeness between scholarship and practice. However, practitioners became disenchanted with the School once it developed a reputation as being antagonistic toward law enforcement and the training of law enforcement had been adequately displaced to the state college system. Conversely, while proponents of radical criminology also sought to nurture closeness with the criminal justice

system, despite the activism they promoted they were unsuccessful in securing the necessary institutional support both within and outside the university administration to buttress against the prevailing sentiment that criminology was anachronistic on a UC campus. The newly-robust discipline of law and society encompassed the research agendas of the School's remaining cluster of centrist and liberal democratic scholars who were comfortably assimilated into the Law School under the aegis of a new program in Jurisprudence and Social Policy once the School of Criminology was dissolved. Thus, the variety of criminological traditions *qua* scientific movements each were forced to develop separate institutional logics through which they acquired legitimacy in their distinct scholarly fields. Furthermore, as is argued in more detail below, those logics emerged and were managed in the organizational context of a School whose bureaucratic legitimacy was itself contested within the institutional field of higher education more generally. The fields were therefore both intellectual and bureaucratic. Legitimacy drawn from one constituted, reinforced, or attenuated the legitimacy drawn from the other.

It is this positioning of the currency of *legitimacy* in an analysis of the School that makes it especially well-suited to a neo-institutional theoretical framework. Moreover, applying a neo-institutional analysis to the School extends our understanding of a theory of scientific movements in criminology. Firstly, outcomes operationalized in terms of mere 'successes' or 'failures' are incomplete. Instead, the process by which a scientific movement emerges and stabilizes is an ongoing struggle of prestige acquisition. This may imply vertical mobility within a field that the scientific movement already occupies (for example, the placement of radical criminology in a high- or low-status niche within mainstream criminology) or a horizontal movement to a different field altogether (for example, law and society's displacement primarily to law schools). Secondly, while a scientific movement emerges by distinguishing itself from a dominant intellectual movement, the latter's dominance may itself be contested. For example, by the time radical criminology departed from administrative criminology, the latter was already a dwindling presence both in the School and in the field of criminology generally. Indeed, radical criminology's ascendance at Berkeley occurred after mainstream criminology had fully embraced a sociological focus that borrowed from the likes of Edwin Sutherland and Thorsten Sellin. Thus, neither was administrative criminology's emphasis on the refinement of law enforcement dominant within the field generally, nor was it the most stable criminological tradition at Berkeley in particular. This implicates a related third point. The multiple competing institutional logics housed within the School invite an appraisal of the legitimacy not only of scientific movements *qua* intellectual institutions, but also of the legitimacy of the institutions *qua* organizations in which those movements are housed. For example, larger transformations throughout the field of higher education stratified the forms of knowledge appropriate to higher education campuses, making criminology at Berkeley anachronistic by the time of the School's dissolution.

DATA AND METHODS

The correspondences and scholarly output of the School's faculty and students serve as important source materials to help formulate a sense of the esteem in which the School was held. Indeed, formal expressions of the intended goals of a professionalization project deserve to be accorded serious analysis in their own right as defining an intellectual – and thus, importantly for the following analysis, *institutional* – field. I therefore retrieved documents that would provide insight into the way that institutional logics were deployed by focusing on instances in which actors across campus contested the legitimacy of either the School itself or of one of the scientific research programs housed therein. I drew on both personal and institutional archives that

comprised a wide array of published and unpublished materials ranging from reports and monographs to private letters circulated within the University. Documents were coded according to whether they contained expressions of what criminology was or what it ought to aspire toward, both on Berkeley's campus and compared to what was being produced elsewhere. Documents were also coded according to whether they expressed support for or contested the development and stability of scientific research programs at the School.

However, previous literature on the micro-sociology of institutional logics has shown that while they are "decidedly extra-individual, their construction, their transmission, and their use depend on people who themselves have interests, beliefs, and preferences." (McPherson and Sauder, 2013: 167-168) Those preferences and subjective impressions are rarely captured by academic and administrative documents alone. Therefore, an assessment of a scientific research program's bureaucratic position by reference to those documents is incomplete. What is needed is a means of capturing how the institutional logics in question were understood, deployed, and altered by those who came into contact with the School.

One method of uncovering subjective impressions of the School's institutional logics is to use oral histories. These help to reconstruct an account of how actors who were present at the time articulated and understood for themselves dynamics within the School. Although historical analyses of developments in criminology have made some use of oral histories to capture the linkage between a scholar's personal history and their intellectual contribution (e.g., Cullen and Messner, 2007; Dooley, 2010; Laub, 1983; Savelsberg and Flood, 2011; Snodgrass, 1972), the method has been applied far less frequently to analyze how institutional logics are deployed in an organization.

This article draws on two sets of oral histories of people involved with the School in a variety of functions prior to its establishment and after its dissolution, in an effort to excavate the perceived legitimacy of the School and its intellectual projects. The first set comprises primary interviews conducted by the author specifically for the purposes of this research. The selection of participants was necessarily driven by constraints imposed by the passage of time: many people with valuable insights had unfortunately passed away or were otherwise indisposed by ill-health. Nonetheless, every effort was made to secure a broad array of views drawn from students and faculty members representing the full complement of research clusters at the School. For the primary oral history sources, early interviews were secured by correspondence with former faculty members of the School whose continued scholarly output and visibility facilitated initial contact. In rare instances when an interviewee had not already done so in the course of sharing their oral history, upon conclusion of the meeting I inquired who might be a worthwhile additional person to include in the project and solicited contact details from the interviewee. This process concluded once it became clear that interviewees were independently suggesting names I had either successfully already spoken to or who were unavailable to participate.³ These conversations typically lasted between one and two hours and, after an introductory confirmation of the interviewee's involvement or association with the School, always began by asking first about their most salient recollections. Common topics of discussion included the intellectual prestige of the research

³ A regrettable number of people were unable to participate due to ill-health or because they had passed away. Since the time of writing, one interviewee (Sanford Kadish) passed away on September 5th, 2014. The current project thus represents one of the last opportunities to capture the recollections of some of the people involved with the School.

agendas housed within the School, sources of funding, scholars' engagement in activism, and social life in and around the School. The secondary sources were collected as part of ongoing projects to archive the histories of criminology, law and society, or the University of California. Some of these are archived as online video segments, whereas others are available only as transcriptions archived at the Bancroft Library. All the oral histories were coded with similar focus toward the practice of institutional logics as was adopted in the analysis of the archival materials.

Throughout the text, square brackets denote reference to archival sources or to oral histories. A complete list of those who generously provided oral histories appears at the end of the article ($n = 10$ primary interviews; $n = 12$ secondary interviews), and a brief biographical sketch of their involvement with the School appears in online supplementary materials. A separate section provides a list of the archival documents used in this research.

Disputes arose frequently in the oral histories, owing to the distant and still highly provocative events in question (for detailed explication of the methodological problems this raises in oral history research, see Laub, 1984; Murphy, 1986). Those disagreements have been catalogued and triangulated with alternative data sources wherever possible. It is hoped that the inclusion of the participants' voices, even when contested, goes some way towards repairing the problem identified by Rafter (2010: 340-341) that in establishing a sense of criminological tradition, "Where we need words, we have silence. Where we need traditions, we have forgetfulness... What we need is a history—or rather, *histories*—of our science." (emphasis added)

PRE-1950: CRIMINOLOGY AS SCIENCE

In the period preceding World War I, the respective roles played by universities and by penal institutions were sharply distinct. As Simon (2010) has described, they could be construed as faces of the State that were, for all intents and purposes, facing away from one another. In the period following the war, however, Vollmer was one of the principal engineers of a new *volte-face* between these two erstwhile opposite-facing institutions in the specific context of law enforcement.

The Progressive Era had animated a particular attitude about the appropriate and effective use of organs of public policy. A burgeoning investment in science and a fascination with rational-legal methods of policy analysis promised a new method of state administration with which the upheavals of the preceding period could be harnessed and controlled, and order could be maintained (Wiebe, 1967; see also Willrich, 2003). This view was characterized by the technocracy movement's ambition to promulgate scientific methods in domains where they had once been viewed as incongruous. 'Social' science was born. While an overview of the birth of the various social sciences is beyond my immediate scope, it is worthwhile simply to note that the new disciplines born in this period, most notably political science and sociology, displayed two important features: firstly, they originated as a *blend* of professional *and* academic disciplines in the pursuit of effective state administration (Short and Hughes, 2007); secondly, they enjoyed bureaucratic legitimacy *by default* and were forced to contest their appropriateness as 'sciences' in universities, akin to their natural science counterparts, as time progressed (Ross, 1991).

Criminal justice and criminology were no different. The urgent need for new police officers that had been created by the war resulted in the need for an institution dedicated to law enforcement modernization. August Vollmer seized this opportunity in his capacity as Berkeley's first Chief of Police to inaugurate a new curriculum of police professionalization at the local University

(Vollmer and Schneider, 1916). Vollmer's co-founder of this new program, Albert Schneider, was a natural scientist who lent the curriculum a *gravitas* that the autodidact Vollmer was keen to secure.

While Vollmer and Schneider were unsuccessful in founding a dedicated School, they managed to establish a Summer Session at the University in 1916, which would expand into a full regular session program in 1931. The curriculum emphasized instruction in the mechanics of police work, by assigning readings in toxicology, psychiatry, elementary criminal law, and microbiology. Vollmer, who had introduced handwriting analysis and fingerprinting in his police department, ensured that the natural sciences were a central focus of the new police education model. He was also instrumental in establishing scholarly journals in which he expected his subordinates to publish their innovations relating to these and other technological innovations ([Wilson, 1972]).

Among the fourteen inaugural faculty members in this new program, he envisioned hiring a Lecturer in Chemical Jurisprudence, and Instructors in Parasitology, First Aid, and Hygiene, among others. In outlining his vision, Vollmer was explicit about the proposed function of this *professional*, and not academic, unit:

The school as planned is in reality a part or branch of the Police Department, is in direct control of the Department, and is intended for the benefit of the men in the service of the Department. Thus far no provisions have been made to admit men not in the service.

(Vollmer and Schneider, 1916: 880)

While similar police education programs developed across the country, none had reached the public profile of the program at Berkeley.⁴ Although Vollmer credited the proliferation of police education programs across the country to his own efforts at Berkeley (Vollmer, 1933), developments in the 1930s resulted in recognition of the inadequacy of Vollmer and Schneider's original curriculum. Three developments in particular would shape the development of criminology for the rest of the century.

Firstly, Vollmer's contribution to the Wickersham Commission in 1929 and his dogged recruitment of talented students ensured rapidly expanding enrolment at Berkeley. During his work on Wickersham, connections that Vollmer had managed to foster with like-minded scholars at the University of Chicago hardened his resolve to institutionalize "police science" in a prestigious university. Yet his own enthusiasm eclipsed that of his hosts at Chicago, including Ernest Burgess and Charles Merriam, to the point that efforts to couple police education with academic sociology eventually fizzled out at Chicago (Morn 1995). But when Vollmer returned to Berkeley, his petition to increase the course offerings devoted to police education within the Political Science Department was met with little opposition. A major in criminology followed not long thereafter, in 1939, although the course content remained largely mechanical. In addition to the courses in 'Investigation and Identification,' 'Toxicology,' and 'Public Safety,' students were required to enroll in 'Principles and Problems of Criminology.' The scholarship produced by the School's students did not resemble scholarship in any meaningful sense of the word, as revealed by a survey of Master's theses, including, for example, *The operating cost of the administration of criminal*

⁴ Two- or three-year courses emerged in 1936 at Alabama, Washington, Harvard, and Purdue. However, these courses were both smaller than the course at Berkeley, and were modeled on Berkeley's curriculum (Morn, 1995: Ch. 2).

justice in seven California counties ([Monograph#1]), and *Defects in the administration of criminal justice with particular reference to murder* ([Monograph#2]). In effect, criminology was becoming established as a discipline at Berkeley, albeit in inchoate form.

Secondly, alumni of the program were successfully hired into prominent positions in law enforcement agencies throughout the country. William Wiltberger had been both a student and policeman in Vollmer's department at various times from the '20s onward, and had landed a position as the Chief of Police in Evanston, Illinois. Another disciple, Orlando Wilson, became the Chief of Police in Wichita, Kansas. Another student, V.A. Leonard, quickly ascended the ranks in the Fort Worth Police Department. By the end of the 1930s, Vollmer's acolytes, or self-identified "V-Men," had penetrated the upper echelons of police management throughout the country. It became clear that the program at Berkeley was producing not just cops, but *cop-managers*.

Thirdly, by 1936, the George Dean Act and the Smith-Hughes Act provided vast increases in federal funding for vocational programs in higher education. This was coupled with a considerable increase after the Great Depression in the number of police departments and police officers across the country. Vollmer was keen to make sure that criminology generally, and criminology at Berkeley specifically, became connected to a much larger project to professionalize law enforcement. This required a solidification of the link uniting the practice of criminal justice with the discipline of criminology.

Such a link necessitated the stratification of police education. There were too many students at Berkeley, each of whom was going on to too many different kinds of police work, for the existing curriculum to accommodate all needs. Vocational colleges had adequately displaced most of Berkeley's instruction in the mechanical operations and procedures of police work. If police education was to flourish at Berkeley, then it was necessary to orient the curriculum toward *executives*. William Wiltberger was thus installed in 1931 as the inaugural dean of a new police education program at San Jose State, where he became responsible for training line officers. It was agreed that Berkeley would be devoted exclusively to training managers and high-level practitioners, under the leadership of Vollmer and Orlando "O.W." Wilson. To Vollmer and his protégés, the very existence of San Jose State, which was dedicated to 'training,' affirmed the distinctive character of the criminological work done at Berkeley as *educational* in nature ([Holstrom, 1971]).

The next step in the process of professionalizing law enforcement was the articulation of a set of professional norms and values around which the profession would coalesce. Despite its apparent success, police education remained a largely inchoate and directionless industry. In 1930, for example, Vollmer published an article arguing in favor of coupling the administration of police work with scientific methods, but the claim was not substantiated beyond the warning to beware of 'pseudo-scientists' (Vollmer, 1930). Berkeley alumni were promulgating the 'Vollmerian' pedigree of police management, but it wasn't altogether clear what that pedigree entailed, beyond the old-boys'-club Berkeley cachet (Morn, 1995).⁵ The initiative to stratify police education left criminology in a fragile position wherein the need to develop an institutional logic, or guiding set of professional principles, was all the more acute and challenging. Ultimately, it was the failure to articulate this institutional logic that would have a lasting detrimental impact from which criminology at Berkeley would never recover.

⁵ Indeed, the androcentrism of criminology would persist until well into the 1960s (Adler, 1997).

By the end of the 1930s, police education was stratified into two levels: the first was devoted to the instruction of frontline officers along the lines set out by Wiltberger at San Jose State; the second was a more executive-oriented approach exemplified by the program at Berkeley. In 1941, V.A. Leonard successfully established, at Vollmer's behest, an altogether different police education model at Washington State. This curriculum would capitalize on the burgeoning literature in sociology and political science on public policy administration and theories of criminality. In short, this new model of police education was much more infused with research and social science than its two forebears at Berkeley and at San Jose State. Leonard even petitioned unsuccessfully for a doctoral program at Washington State in 1944. As the war concluded, state colleges became the *de facto* home of police education and few thought the development untoward ([MacNamara, 1996]).

The time had come to formalize the goals of higher education in law enforcement: was it to define practice standards, to improve practice, to apply science to practice, or to acquire credentials? In 1941, the leaders in the field convened at Berkeley and determined that it was all of the above. The realization of those goals demanded that they be institutionalized and codified in the form of a professional association. The National Association of College Police Training Officials was born (Morris, 1975; [Report#1]). Five years later, the Association was re-named the Society for the Advancement of Criminology, and membership was limited to "persons engaged by accepted universities and colleges to instruct or supervise in professional and vocational training programs in criminology."

However, opinions were divided in these meetings (Morris, 1975). In particular, the personalities of Wiltberger, who championed the technical and mechanical forms of instruction, and Leonard, who championed the academic form of instruction, were at odds. The 1941 meeting ended with Wiltberger feeling marginalized by the committee's decision to allow non-police officers membership in the NACPTO. Conversely, the 1946 meeting – by which point some of the prevailing sentiments had shifted – ended with Leonard upset that professors were *not* allowed membership in the SAC. Criminal justice education was fragmenting, and efforts to maintain cohesion through formalization of its goals had resulted in further acrimony.

1950–1960: AN UNEASY BEGINNING

Criminology still lacked a clearly articulated vision of what it was and what it sought to accomplish. It was thus little surprise when, after soliciting input on the prospects for a new School of Criminology at Berkeley that would build on the previous program, that the Committee on Educational Policy responsible for determining whether to establish or disestablish academic units reported unfavorably in 1947. Notably, the Committee's report took issue with three features of the proposed School's contribution: it argued that criminology as a discipline was ill-defined, it asked why the same function could not be fulfilled in one of the other departments or state colleges, and it appealed to the inappropriateness of a purely vocational college on the University's campus.

The School was created in the face of that opposition with the help of forceful support emanating from within the University and from outside of it. By this point, Berkeley criminology alumni were sufficiently influential and eager to support Vollmer's project that letters arrived from criminal justice agencies throughout the country arguing in favor of the School's establishment ([Report#2]). Moreover, the University's President, Robert Sproul, who was sympathetic to the proposal, assembled the "Committee to Consider and to Make Recommendations on the Report on Criminology of the Committee on Educational Policy." He populated this new committee with

faculty members who were similarly sympathetic to the idea of the new School, and he soon had his favorable report to bring to the Regents.⁶

The new School's dean had to be selected in order to secure funding, and the short list comprised Thorsten Sellin, Sheldon Glueck, and O.W. Wilson. Wilson did not have a PhD, but had nurtured an extensive network of professional contacts in criminal justice agencies throughout the country and he therefore offered the promise of closer ties to the professional world. Sellin and Glueck, on the other hand, both embodied the pinnacle of criminological research, and both carried with them inestimable reputations in the field and impeccable publication records. They represented the academic ideal, although their interest in professional training as part of the academic enterprise was at best incidental. For example, Wolfgang [1997] spoke of Sellin's disavowal of the professionalism that criminology had begun to take in some circles:

At that [early] point in my career I shared Sellin's perspective that criminology is the scientific study of crime and criminals, and society's reaction to both, and we were in academia. We were not in public policy, nor did we think that anything we did would necessarily have any practical payoffs in the administration of justice.

The picture is less straightforward for the Gluecks, whose interest in the practical application of their work reflected their backgrounds in psychiatry, social work, education, and law. Nonetheless, theirs remained a project of basic rather than applied research: their principal focus was on the precise identification of causes of criminal behaviors as opposed to the refinement of operations of state control (Laub and Sampson, 1991: 1428).

Ultimately, Sproul marshaled the executive decision to hire Wilson, and requested that the proposed curriculum be altered to emphasize police administration techniques. The professional model had prevailed, but only by a whisker; moreover, it relied heavily on the institutional support of practitioners and executives in criminal justice agencies who were sympathetic to the School.

The period between 1950, when the School of Criminology opened its doors, to the end of the decade marked a time of both great promise and increased fragmentation for criminology. In a letter written to the Chair of the Subcommittee on Educational Policy, Dean Wilson recognized that the School lacked a precise definition of the scope of criminology as a discipline in comparison to the competing Midwest and East Coast schools:

Our definition of criminology is undoubtedly broader than that applied in some of the seven universities [where such coursework was offered, including Chicago, Columbia, Harvard, Illinois, Michigan, Minnesota, and Wisconsin]... We are confident that our accomplishments excel those of any of the seven and that gradually more and more of them will develop programs comparable to ours.

([Memo#1])

However, the imprecise articulation of the School's institutional logic did not seem to cause Wilson undue concern. He delighted in the fact that graduates of the School directed numerous in-

⁶ The Board of Regents, which includes the President of the University of California, is responsible for overseeing the governance of all the various UC campuses (with the exception of Hastings College of the Law). The Board's responsibilities include the appointment of standing committees such as the Committee on Educational Policy. The office of Chancellor was created in the early 1950s, with the inaugural position held by Clark Kerr, to oversee the governance of the Berkeley campus.

house police research units, they included numerous chiefs of police and high-ranking officers in some of the larger departments throughout the country, and some alumni had been employed at the American Bar Foundation, the CIA, and the International Cooperative Administration. Other graduates had established successful new police education programs in state and city colleges. Moreover, Wilson proudly attested to the School's contribution in igniting the explosion of criminalistics departments throughout California. Whatever the School was designed to do, it seemed to be doing it well. However, the objective of the School remained, like that of its institutional twin the SAC, undefined (University of California Bulletin, 1950).

If there was one point of dissatisfaction for Wilson in the direction that the School had taken, it was a deficiency in the School's research profile (Bulletin of the SAC, 1954). Despite Wilson's efforts to the contrary, the School's curriculum throughout his tenure as Dean remained entrenched in the professional mold, and eschewed even basic coursework in research ([Minutes#1]). At an Academic Advisory Council meeting in 1957, the Chancellor raised the concern that undergraduates in the School might be taking too high a course distribution in vocationally concentrated classes. Wilson responded by appealing for an expansion to include courses in 'Juvenile Delinquency' and a graduate course in research methodology. Coursework at the time included 'Criminal Investigation,' 'Police Administration,' 'Physical Evidence,' and 'Traffic Engineering,' and a salutary theoretical course in 'Crime and Delinquency.' The research profile changed once he established a doctoral program at the School ([Report#3]), but even then this did little to mitigate the widespread view of the School as a 'cop shop.' ([Minutes#2])

In addition to the School's emphasis on the more technical aspects of criminal justice administration, the Society for the Advancement of Criminology had become increasingly insular and exclusively focused on practitioners. Membership and presentations at the Annual Meetings were dominated by Berkeley faculty and alumni, and were held on the West Coast throughout the 1950s. While the School retained close institutional links with the SAC, Berkeley's criminology was becoming increasingly parochial. It was also beginning to compete with departments that had emerged in the Midwest and on the East Coast that were vying for academic supremacy by resorting to a distinct brand of criminology that was set against the administrative variant at Berkeley. This alternative brand of criminology, exemplified by the scholarship of Thorsten Sellin, the Gluecks, and Edwin Sutherland, eschewed professionalization outright and was an overtly intellectual endeavor. East Coast criminology had also been successfully assimilated into mainstream sociology, where Sutherland had been elected to the presidency of the American Sociological Association in 1939. Criminology was focusing on *crime* as a social phenomenon, rather than on the administration of criminal *justice*.

The 1950s ended with the passage of far-reaching legislation that would have considerable impact on the constitution and aspiration of higher education throughout California. The Donahoe Higher Education Act of 1960, or Master Plan, re-constituted the University of California system, the California State system, and the community colleges into a consolidated network. The ambitious agenda set forth in the Master Plan included the displacement of the University's services for vocational and professional schools to the California State system, where curricula modeled on the San Jose State approach had flourished (Douglass, 2007).

Criminology at Berkeley was being assailed not just from within by the reconstituted University administration, but also from without, by an increasingly tenuous grip on its erstwhile trusty institutional crutch, the SAC. In an effort to widen its West Coast focus, the SAC redoubled its efforts to convene law enforcement educators and intellectuals across the country. It opened

membership to non-academics, it changed its name to the American Society of Criminology, and its lack of immediate applicability to police educators would come to alienate the V-men at the ASC. As a result of increased dissatisfaction with the direction the ASC had taken, a number of police educators splintered off and formed the International Association of Police Officers, which would evolve into the Academy of Criminal Justice Sciences in 1970 ([MacNamara, 1996]).

Four factors resulted in renewed attention on the part of the University administration to the appropriateness of the School of Criminology on Berkeley's campus: firstly, the student population had ballooned, and resources had become heavily strained ([Report#4]). At this point, the campus was experiencing massive expansion in its student enrolment, but the expected increases in the School of Criminology over the next ten years (175%) dwarfed those in the rest of the Social Science Division (71%). The strain on faculty members and classroom space had also been a long-acknowledged nuisance ([Memo#2]). Secondly, the re-constitution of the University of California under the Master Plan had resulted in a more general re-evaluation of the campus' aspirations and capabilities. Thirdly, the growing distance between the School of Criminology and the ASC—and especially the marginalization of the overtly professional wing of law enforcement education within that Association—accentuated the School's precariousness. Fourthly, the sympathetic President Sproul (who had engineered Dean Wilson's hiring and the vigorous pursuit of vocational training within the School) was replaced with an unsympathetic Clark Kerr (who had been one of the architects of the new Master Plan). Kerr established an Academic Advisory Committee to examine the School's suitability, and the resultant Report echoed the conclusions of the CEP findings from eleven years earlier: criminology as a discipline was ill-defined, the same function could be fulfilled in the state colleges, and it questioned the appropriateness of a purely vocational college on the University's campus.

By this point, the Committee's claims were harder to refute than before. Indeed, there was widespread recognition that there was no clearly articulated justification or vision for the marriage between higher education and law enforcement. What training was being done – and there certainly was an abundance of it – was amply accomplished in the state college system. And the Master Plan had been clear about relegating vocational programs of the School's type from the University campus altogether. In 1960, Dean Wilson left Berkeley behind to take up a position as superintendent of police in Chicago. On his way to Chicago, he would cross paths with the new Dean of the school, Joseph Lohman.

1960–1968: INTELLECTUAL PLURALISM AND FRACTURE

Lohman's administration was defined by his efforts to elevate the School's academic credentials. In a letter written to Herbert Blumer before arriving at Berkeley, Lohman wrote:

I would look to dropping some of the police vocationalism and the strengthening of work in this field through short courses, University Extension and through Institutes, once things had gotten under way. I am somewhat under a handicap at this juncture in proposing an arbitrary shift in curriculum content since the problem turns directly upon the capacity of the faculty to handle material which is probably foreign to them. This is due, of course, to the narrow police training concept which Wilson employed and which seems to have overshadowed everything in the School.

([Letter#3])

Conveniently, the alignment of much of criminology with sociology continued apace throughout the 1950s (Short and Hughes, 2007), even though the sociology department at Berkeley did not appear to hold the Criminology School in especially high esteem. For example, when David Matza was offered an Assistant Professorship in 1961, his colleagues in the sociology department advised him to decline a joint appointment with the Criminology School (Blomberg, 2010: xiii). One interviewee mentioned that the research profile of the School, for its part, did not appear to be especially concerned with nurturing closer relationships with the sociology department either ([Kruttschnitt, 2013]). Nonetheless, the sociology department maintained close ties to criminological scholarship produced by the inchoate cadre of ‘law and society scholars’ housed in the Center for the Study of Law and Society. Sociology was not formally coupled to legal scholarship in the mid-1950s, but this would change considerably over the following decade (Skolnick, 2012: 4; Tomlins, 2000). Law would become integrated with social science as part of a research program that emerged in parallel with, rather than as a complement to, the Criminology School. The Russell Sage Foundation provided seed funding to establish a center for the study of law and society at Berkeley after Yale and Harvard had declined the offer due to the Foundation’s stipulations about how the money should be spent (Garth and Sterling, 1998; Wheeler, 1994). In its early stages, while the institutional logic of the promises of liberal reforms suffused law and society scholarship (Abel, 2010), its scholars were disinclined from the policy-focus found among the dwindling clutch of administrative criminologists. Donald Black would write at the time that law and society should eschew the “sheer ideology” inherent in the “moral judgments” of policymaking in favor of a “value neutral” and “strategically detached approach.” (Black, 1972)

By 1963, Berkeley had cemented a connection between law, sociology, and criminology among scholars who did not self-identify as criminologists. CSLS was largely dominated in its early days by sociologists, most notably including Philip Selznick, Sheldon Messinger, and Jerome Skolnick. At the time, those scholars identified more closely with the towering presences in Berkeley’s sociology department, which included Reinhard Bendix, Erving Goffman, David Matza, Neil Smelser, and Seymour Martin Lipset (who was responsible for Jerome Skolnick’s recruitment). Sheldon Messinger had been a student of Lohman’s as an undergraduate at the University of Chicago; however, he was recruited to Berkeley by the sociologist Philip Selznick to serve as vice-chairman of the new CSLS (Garth and Sterling, 1998). Indeed, it was Messinger who first suggested a ‘law and society’ special issue of *Social Problems* (published in Summer 1965) that would later represent the law and society movement’s maiden voyage and would provide the initial momentum necessary to establish the *Law and Society Review* in November 1966.

The relationship of the School to the CSLS was complicated: although there was no formal administrative overlap (CSLS was located within the Law School and received funding entirely separate from the Criminology School), lectures at the School were frequently taught by CSLS researchers. In addition, the Russell Sage Foundation actively funded large-scale projects related to crime and justice, so that by the mid-1960s law and society had become a well-established tradition of criminological scholarship at Berkeley.

This suited Lohman, whose arrival had from the outset been seen as a compromise to repair relations between the academic and law enforcement communities – although it was generally acknowledged upon his arrival that he belonged in neither ([Skolnick, 2013]; [Krisberg, 2013]; [Platt, 2013]). During his deanship, the School’s leading faculty profile took a significant intellectual turn away from the professional and toward the critical aspects of criminology. Jerome

Skolnick published *Justice Without Trial* in 1966 and *The Politics of Protest* in 1969, both of which analyzed how race inequalities permeate and antagonize police-citizen encounters; Bernard Diamond became a key figure in the promotion of the diminished capacity defense, which was seen as a profound irritant by the police; Caleb Foote became a high-profile criminal rights activist and scholar due to his work on bail policy; and Herman Schwendinger became the intellectual luminary behind radical criminology. In addition to the changing faculty profiles at the School, the quality and character of the students changed significantly; they became much more interested in theory than in practice.⁷

Lohman came into irreparable disrepute with both law enforcement and the University administration for having advised President Kerr and Chancellor Strong to order the police crackdown of student protests during the 1960s ([Skolnick, 2013]). Yet Lohman steadfastly maintained this uncompromising stance toward the protestors in spite of that opprobrium ([Frederick, 1984: 346]). He also de-emphasized professional training – and was vocal about his direction – and his tenure at Berkeley over the course of the 1960s resulted in vilification of the School by law enforcement ([Krisberg, 2013]) as he steered it away from its anachronistic reputation as a ‘cop shop’ ([Kruttschnitt, 2013]; [Speiglmán, 2014]). This may have been a response to the prevailing sentiment that the police professionalization project had survived its absence of an institutional logic for too long:

There are no clear criteria for what would count as better performance of police tasks; and second, we know very little about the behaviors that professionalization has produced or will produce. In other words, no one is presently in a position to accept or reject the professionalization solution. The variety of normative standpoints from which perceptions of the police role stem are not defeasible by the simple assertion of professionalism; if neutral competence is to be the claim, the relevant behaviors must still be specified to enable one to judge their neutrality and their competence.

(White, 1972: 61)

At the same time that administrative criminology waned and law and society waxed at the School throughout the 1960s, some scholars began to critique Great Society liberalism and liberal social scientists’ presumed failure to alleviate America’s social problems (Galliher, 1978). Marxist theories acquired newfound popularity across the Berkeley campus, and the epistemic premise of politically neutral and objective scholarship rapidly fell into disrepute ([Nader, 2011]). As though in direct conversation with the law and society scholars in the School, radicals published work that expressed dissatisfaction with the narrow restriction of legal scholarship. By constraining the analysis of harm to crime, radicals contended, law and society scholars overlooked the means by which the state itself precipitates and perpetuates oppression (Schwendinger and Schwendinger, 1970).

It would be a misinterpretation, however, to view the emergence of ‘radical criminology’ as a counter-movement to law and society; while radicalism certainly critiqued the liberal democratic project, healthy collaborative relationships existed between scholars from both criminological

⁷ Again, a survey of dissertation titles is instructive ([Monographs #8 to #10]): Ronald Smith (1961), *An evaluation of programming methods for the distribution of police manpower*; Anthony Platt (1965), *The criminal responsibility of the mentally ill in England 1100-1843*; Gary Powell (1966), *Violence: A function of social isolation*.

traditions. Skolnick, who had been hired by a law and society scholar using Russell Sage funds, employed radicals Tony Platt and Elliott Currie as research assistants in compiling the *Politics of Protest*. Conversely, radicals like Krisberg would frequently solicit doctoral students from among the law and society crowd ([Mahabir, 2014]). The research agendas of law and society and of radical criminology sounded a crescendo while the vocational orientation of the School that had formerly functioned as its intellectual and institutional bedrock subsided. Importantly, the intellectual traditions of law and sociology from which law and society borrowed (Friedman, 1986) had forged a strong and prestigious connection that radical criminology could not claim even if it had so wished.

Consequently, by the end of the 1960s, the School's focus had shifted almost entirely away from police education, and had embraced the much more sanguine field of corrections as its focus for vocational training ([Pamphlet#1]). Universities and prisons had developed a new "closeness" (Simon, 2010: 159). The Criminology School was no longer the hub of police education that it once was, and it therefore suffered the distaste of criminal justice agencies as being 'not professional enough.' Faculty members, on the other hand, felt the School's orientation was too professional rather than academic ([Kadish, 2013]).

This time, the sense that the School occupied an "uneasy" position on the University's campus ([Kadish, 2013]) was countered by repeated and forceful institutional support from Richard McGee. McGee was the first Director of the California Department of Corrections, and became the Chairman of the Academic Advisory Council for the Criminology School. A series of correspondences in the winter of 1960 between McGee and Herbert Blumer, the Director of the Social Sciences Division, clarified McGee's desire that the School should retain a theoretically informed, albeit still overtly professional, bias. Blumer's responses illustrate the administration's acquiescence to McGee's wishes ([Letter#1]).

Pursuant to McGee's wishes, the curriculum changed during the 1960s to include 'Etiology of Crime (Sociological),' 'Etiology of Crime (Psychological),' and methodological courses became requirements for students ([Memo#3]). Moreover, Lohman created a research institute within the School that won many lucrative government research grants ([Monographs#3 to #7]). This evidenced one of two notable features of Lohman's administration: he was keen, inasmuch as he had hired subversive and liberal democratic faculty while maintaining his own links to criminal justice agencies outside the School, to foster an academically pluralist environment ([Currie, 2013]; [Krisberg, 2013]; [Platt, 2013]). At the same time, consistent with the direction taken by the administration elsewhere across campus (Cohen, 2009: 200), he administered the department "unilaterally, undemocratically, and paternalistically" ([Platt, 2013]), such that the faculty was weak and the dean held sway. When Lohman suffered a heart attack in his office and died in 1968, the faculty enjoyed unprecedented authority in determining the School's direction.

1968–1976: DISENCHANTMENT AND PROMISE

The Berkeley criminology curriculum at the outset of the 1970s ostensibly resembled what one might expect to see at criminology and criminal justice departments elsewhere around the country ([Krisberg, 2013]; Schwendinger and Schwendinger, 2014: 23). It contained basic foundational courses in substantive areas such as theories of crime, juvenile justice, and policing, as well as methodological courses in research techniques and program evaluation. There was, however, one exceptional feature of the criminology coursework at Berkeley that deviated from the more conventional offerings throughout the country: the content of the lectures in some courses

(most notably those taught by Platt and by Schwendinger) was steeped in a radical Marxist outlook (Krisberg, 1974). This irked the administration and powerful constituencies within and outside of the University, and resulted in the School being labeled the “Home of Anti-Criminology” ([Kadish, 2013]). The former “closeness” that the School had nurtured with the penal community had descended into “fracture and crisis.” (Simon, 2010: 160)

Nonetheless, the School became the epicenter of American progressive criminal justice reform. Graduates included Thomas Murton, who went on to publicize the corruption scandal in prisons in Arkansas. Mimi Silbert founded Delancey Street, one of the country’s first half-way houses for drug abusers. George Napper became Atlanta’s first black Chief of Police. Lee Brown pioneered community policing as police commissioner in Houston and then in New York. As sheriff of the San Francisco Police Department, Richard Hongisto actively recruited deputies drawn from gay and minority communities.

Academically, the School’s credentials were also impeccable. The Prison Action Conference brought widespread publicity to the School in 1972, and the publication of *The Iron Fist and the Velvet Glove* (ISLEC, 1975) and of the journal *Crime and Social Justice* housed at Berkeley became required reading for radical criminologists across the country. In 1992, more than a decade and a half after the School had been closed down, the School’s faculty members were still the most cited in the country in introductory criminal justice texts (Sorensen et al., 1992: Table 9). An external committee assembled by Chancellor Bowker to review the School’s academic profile, chaired by Marvin Wolfgang, concluded that the School should be strengthened and expanded ([Memo#4]).

The School had also become successful in securing highly lucrative grants, in part due to the injection of government monies into criminal justice research as part of the Law Enforcement Assistance Administration. The LEAA had been set up with the express purpose of funding criminological and criminal justice projects in pursuit of improving the administration of law enforcement (see Feeley and Sarat, 1980), although its perceived distortion of the research enterprise caused consternation among the School’s radicals, who thought it left the presumption of state repression intact (e.g., Galliher, 1979; Quinney 1974: Ch.2; Takagi, 1979; for the influence of state-funded research on the contours of American criminological scholarship, see Savelsberg et al., 2004; Savelsberg and Flood, 2004). Those grants were principally obtained and researched by Lohman and the vanishing profile in criminalistics; together, these encompassed the totality of the School’s administrative criminology profile until 1968.

There is some dispute as to how harmonious the relationships were between the three intellectual threads within the School after the Lohman years. Although some former students attested to the congenial environment within the School (e.g., [Kruttschnitt, 2013]; [Mahabir, 2013]; [Omi, 2014]; [Speigman, 2014]), many of the collaborative relationships between law and society scholars and radical criminologists dissipated in the early 1970s. A former student who self-identified with the law and society scholars recounts that one of the principal radical professors, Paul Takagi, brusquely disinvited him from attending a lecture delivered by an invited radical before the lecture began, only after the student had already taken his seat in the audience ([Blomberg, 2013]). In recognition of the fact that by the early 1970s Tony Platt had become the most visible if not the most charismatic leader of the radicals, his students were labeled the “Plattitudes.” The radicals became an active and closely integrated presence on campus, and assembled a Union of Radical Criminologists ([Omi, 2014]; [Speigman, 2014]), whose purpose

was “to promote radical alternatives to the overwhelmingly racist and sexist ideology and practice that dominate criminology today.” ([Pamphlet#2])

The disagreements were more than cosmetic. The law and society movement’s approach to scholarship was characterized by an effort to be objective: “In *Justice Without Trial*, I tried not to be a critic of the police. That was not my job. My job, I felt, was to present what the police are really like. To give a fair assessment.” ([Skolnick, 2012]) The approach adopted by the radicals, who openly repudiated the detached objectivity of their law and society colleagues, could not have been more different:

Radical criminology was not simply a spontaneous response to the crisis in the state or to social protest. Activism was not external to the university... At Berkeley, radical criminology was self-consciously organized and built by a small core of faculty and students who had been politically active for many years or who had been radicalized by their experiences in the 1960s.... Radical criminology in academia was born as an activist movement. From its earliest days in the late 1960s, it emphasized practice over theory, tactics over strategy.

(Platt, 1988: 133)

Radicalism was distinctive from the other two criminological traditions found at the School in that it laid the ills of society at the feet of the state, for some to the point of perseveration. Law and society scholarship distinguished itself from its administrative and radical criminological counterparts found at the School by being less directly connected to modifying criminal justice policy, either through community activism or the instrumental state-funded research apparatus on which it relied. Administrative criminology was distinctive in that it was primarily preoccupied with refining criminal justice rather than understanding the processes by which crime itself is manufactured. All three traditions relied on manifestly different methodological and substantive premises to realize their respective projects, and in various ways these epistemic disagreements stymied the collective action necessary to protect the School’s institutional legitimacy in either the academic or professional fields it occupied.

Those political sensitivities came to a head in the selection of a Dean in 1971. The Dean of Berkeley’s Law School Sanford Kadish had solicited Norval Morris, an eminent politically centrist law professor,⁸ to deliver a lecture as a candidate for the position that had been held by Leslie Wilkins and then by Sheldon Messinger after Lohman’s death. Chancellor Bowker made it privately clear to Morris that he intended to close the School if Morris did not succeed; Bowker’s caution was ultimately fruitless, as the students made it apparent to Morris that his political position was unwelcome by a vote of 82 to 6 in a telegram sent upon his return to Chicago (Franklin Zimring, personal correspondence February 13th 2013). Morris subsequently withdrew his candidacy, and Messinger was selected as the Dean who would preside over the School until its closure.

The publicity of the School drew the attention of powerful and unsympathetic observers. Then-Governor Reagan and Attorney General Meese had not forgotten the prominent involvement of the School’s faculty in drawing public attention to Reagan’s hand in suppressing the People’s Park demonstrations of 1969. The School was by no means alone in its distaste for the

⁸ Perhaps unbeknownst to the students at the School, Morris was sympathetic to radicalism, even if his own scholarship was not interpreted to indicate as such ([Morris, 1996]).

administration's capitulation to gubernatorial interference; Chancellor Heyns reflected on the prevailing dissatisfaction with his administration, citing that he "probably was, in terms of faculty support, at a very low point here." ([Heyns, 1986: 84]) Indeed, the administration had earned the reputation of being in "institutional shambles" thanks to its response to the incipient Free Speech Movement a few years earlier (Smelser, 2010: 10). It was also profoundly displeased with some of the activities centered around the School: these included the message of the 1972 'Tear Down the Walls' Prison Action Conference, which had been clear in expressing rancor concerning racism in Reagan's law enforcement and prison policies ([Platt, 2013]), and with the radicals' unsuccessful – yet highly visible – campaign to re-structure local law enforcement to incorporate community control (Sklansky, 2008).⁹ When Meese, supported by Chancellor Bowker, was appointed to the School's Advisory Council, the School's ruination was imminent.

Bowker assembled another Committee to rebut the Wolfgang Report, and populated it with antipathetic faculty from the Political Science and Public Policy departments, who had long felt that the School was encroaching on their turf. This committee, chaired by Allen Sindler, predicated its recommendation to close the School on its intellectual departure from its original "professional mission" (Schwendinger and Schwendinger, 2014: 130). Bowker then moved to deny tenure to Tony Platt, even after Platt had received the endorsement of every other formal body ([Currie, 2013]; [Skolnick, 2013]).

When Dean Sheldon Messinger solicited support to justify the School's existence on the University's campus, the institutional backing from criminal justice agencies had evaporated. Letters from academics across the globe attested to the School's value, and support from within the community was formidable: protests at the School's closure were well attended and widely advertised. The June 8th issue of the Black Panther newspaper, for example, ran the headline "3,000 Protest Criminology School Closing" above an article celebrating founding member Bobby Seale's designation of the School as a "community institution" that had begun to "reflect the desires and wishes of the people, as against the wishes of the power structure." That power structure, according to Seale, was "mov[ing] in to destroy the institutions, cut off funds and change directions." (Black Panther, 1974)

But, in part because the School's *professional* Advisory Council had been dismantled after Lohman's death, the voice of criminal justice practitioners was silent ([Holstrom, 1971]). What had before this point been the most persuasive rebuttal of qualms about the School's appropriateness was no longer present to advocate on the School's behalf. While some sought to frame the effort to close the School as one that "[could] not be waged as an isolated struggle apart from other struggles on campus," attempts to cultivate an institutional logic that "involve[d] principles of self-determination, academic freedom and social justice," and that "affect[ed] the rights and dignity of all students and workers on campus" ([Pamphlet#3]) failed to resonate with the administration. The School had long divested itself of any support from a practitioner professional association, and the appeals of academics, student groups, and community

⁹ The School must be set within the wider context of contemporary political and social struggles. By this point, Berkeley had become a shibboleth for policymakers hoping to burnish their credentials as center-right reformers (Berger, 2014; Rosenfeld, 2012). During an electoral campaign for the position of Superintendent of Schools in California, for example, Max Rafferty had openly voiced his antipathy toward Berkeley's "four-year course in sex, drugs, and treason." (qtd. in Perlstein, 2008: 277)

organizations held little sway for Bowker when arguing for a professional school on the University's campus:

Clark Kerr had once or twice tried to abolish the School of Criminology, I'm told, and had failed because the establishment of police and public safety complained so much that he was overruled or backed down... criminology had become politicized to the point where it really wasn't taken very seriously by the public safety establishment any more... The other reason was that I thought the Criminology School wasn't serving any useful purpose and didn't need to be at Berkeley. There was some criminology at [UC] Santa Barbara which could have been the basis for whatever needed to be done at the University of California. So I decided to abolish the School, and I got a faculty committee to recommend that.

([Bowker, 1991: 16-17])

When criminology first emerged on Berkeley's campus, the administration's misgivings centered on the alleged lack of academic rigor of what was, for all intents and purposes, a technically-focused professional curriculum. Over decades, Vollmer, Wilson and Lohman struggled to infuse criminology at Berkeley with the academic standing that it ultimately enjoyed during the 1970s. But by that point, the Chancellor had different priorities: he wanted a School that would graduate practitioners rather than researchers or reformers and that would be less preoccupied with progressive reforms. Dean Messinger captured the spirit of the times in a letter to the students of the Institute of Criminology, University of Cambridge:

The School is a target because, according to some, it does not have a 'professional' program, instead having become 'overly academic.' Translated this seems to mean that the School's graduate program does not focus on the training of criminal justice personnel but, rather, mainly aims to educate planners, researchers, and persons who intend to teach criminology in institutions of higher education. It is my view, shared by faculty and students, that this is just what the School should be doing. The field needs such an educational program and other institutions of higher education, by and large, are neither capable of nor willing to offer it.

([Letter#2])

The administration disagreed, and the School closed its doors for the final time on July 15th, 1976. The thinly-populated criminalistics program was moved to the Public Health department, the radical faculty members were either dismissed or moved to the School of Education, and the "sad remains" ([Kadish, 2012]) of the School were re-constituted into a graduate program in Jurisprudence and Social Policy housed within the Law School.

LEGACIES

Typically, the story of the School of Criminology at Berkeley is told as "a case study in the limits of academic tolerance" (e.g., Geis, 1995), or as an exercise in determining how interest groups castrated the promising research agenda of radical criminology (e.g., Schwendinger and Schwendinger, 2014), or as an exemplar of the idiosyncrasies of academic politics (Morn, 1995). There is, however, more to the history of the Berkeley School of Criminology than the search to establish how or why it was closed. Within the story that culminated in the School's closure are overlapping and intertwined narratives about the place of criminology within elite universities, the fragmentation of criminology into different 'criminologies,' and disputes about whether and how to connect scholarship with policy. These are developments that lend themselves especially well

to a neo-institutional analysis of both how criminological traditions arose and acquired stability, and how those criminological traditions navigated the institutional fields they occupied.

Three identifiable scientific research programs appeared over the course of the School's lifetime, including administrative criminology, radical criminology, and crime-focused law and society scholarship. Frickel and Gross (2005: 209) contend that a scientific or intellectual movement is more likely to emerge when high-status intellectual actors harbor complaints against what they understand to be the intellectual tendencies of the day. Vollmer and his acolytes articulated an imprecise institutional logic of law enforcement professionalization that emphasized the application of (typically natural) scientific methods in pursuit of refined practice. Consequently, the project of professionalizing law enforcement became indistinguishable from the project of coupling the criminal justice system with institutions of higher learning. At Berkeley, this connection provided the needed *imprimatur* of the University of California system for a burgeoning field of public policy that was in need of legitimacy.

The field of 'professional criminology' expanded by the mid-twentieth century and the training of frontline officers became satisfactorily displaced to the growing state college system. Social sciences in general, and vocational programs in particular, were soon forced to contest their legitimacy on elite university campuses. Law and society scholars appeared at Berkeley from the mid-1950s onward who re-oriented jurisprudential scholarship away from a pursuit of doctrinal analysis of 'law on the books' and toward a social scientifically informed analysis of 'law in action.' Their administrative propinquity to the School of Criminology, with scholars housed at the neighboring CSLS but often teaching within the School, meant that law and society positioned itself as an ascendant intellectual tradition in parallel with the administrative criminology practiced throughout the School. The prestige of law and society combined with existential doubt about the purpose of the School. Consistent with the idea in neo-institutional theory of 'mimetic isomorphism' (DiMaggio and Powell, 1983), which posits that organizations acquire legitimacy in times of uncertainty by modeling themselves after organizations perceived to be successful, Dean Lohman diversified the School's research profile to incorporate the sociological orientation that had prevailed in criminology departments throughout the rest of the country.

Radical criminology, too, emerged during the 1960s as a product of intellectual doubt about the scholarship being practiced in the academy. Radicals who were dissatisfied with the contradictions of capitalist society and repressive conditions generated by the criminal justice system argued that administrative criminological and law and society scholarship were not fit for purpose. They devised a new scientific movement that they contended was more aptly suited to problematize the role of the state in manufacturing harm.

The emergence of each of the three criminological traditions at Berkeley is therefore compatible with Frickel and Gross's (2005) theory of the rise of a scientific movement. In all three cases, the appearance of a new intellectual tradition was spurred by an effort to differentiate a new scientific project from the prevailing practice. But while this theory accurately accounts for the *emergence* of all three criminological traditions, its account of the process by which a scientific movement acquires prestige and *maintains institutional stability* is less straightforward. Frickel and Gross (2005) suggest that a scientific movement is more likely to succeed when structural conditions provide access to key resources (p.213), when the movement has greater access to micro-mobilization contexts wherein the work of recruitment truly happens (p.219), and when the movement participants frame ideas in ways that resonate with the concerns of those who inhabit

their intellectual field (p.221). The currency of the analysis, therefore, is *legitimacy* and the predictors associated with a movement's acquisition thereof.

Once a bridgehead had been formed between the Berkeley Police Department and the University, Vollmer and his acolytes worked tirelessly over subsequent decades to ensure that the legitimization of criminal justice administration could continue by two means: firstly, by the placement of a certain brand of alumnus in various strata of influence throughout the country; and secondly, through their efforts to maintain a strong network of support for the field, instantiated most clearly in professional networks and publication in new journals.

Throughout the School's existence, institutional developments and personal disputes resulted in periods of questioning the appropriateness of the School of Criminology on the University's campus. Crucially, however, up until the mid-1960s every such episode ended with a concerted wave of support from prominent justice professionals who advocated on behalf of the School's professional necessity. Critics of the School were also refuted by the School's success in obtaining research funding from local and federal government sources. Administrative criminology was thus sustained by its circumscription of an institutional field that encompassed both the profession of law enforcement and higher education (for an analogue to this interdependence in the institutional logic that sustained business schools and the profession of management, see Khurana, 2007).

As the institution of higher education fragmented, so too did police training. The university, distinct from the state college, made the existence of a vocational school an anachronism on an elite campus. Although administrative criminology dwindled, it was preserved by state-funded research grants and by support from the correctional community. Once the institutional field on which administrative criminology had relied shifted from an interdependence between law enforcement and university campuses to the state colleges, the institutional legitimacy of administrative criminology at Berkeley dissipated.

Law and society at Berkeley operated within a different institutional field altogether. It coupled social science with law, and any intended impact on criminal justice policy was supposed to be indirect. It thus secured prestige through its relationship with the Law School and the sociology department rather than with law enforcement. Consequently, while law and society scholars frequently collaborated on research projects with criminologists at the School and even co-taught classes, nonetheless the institutional logic of law and society departed from that espoused in the other strands of criminological scholarship housed within the School. No doubt this was attributable to the bureaucratic distance between the professional Criminology School and the CSLS where most of the law and society scholars were housed. When the School was closed, crime-focused law and society scholarship naturally continued with only minor readjustment of bureaucratic organization by being displaced to the Law School in a dedicated program in Jurisprudence and Social Policy.

Although administrative criminology was displaced to the state college system and law and society was displaced to the Law School, it seems uncontroversial to claim that these displacements represent more of a 'success' for those scientific movements than might be claimed for radical criminology. In February 1979, the journal *Criminology* assembled six essays thematically tied to providing an assessment of radical criminology as part of the journal's first – and still only – special issue. Richard Quinney, who periodically served as a Visiting Professor at the School, wrote the opening essay. Entitled "The production of criminology," the article invited

scholars of crime, justice, and power to orient themselves toward their position in the class structure. Inasmuch as it applied a Marxist perspective, Quinney was held up as the proponent of radicalism in criminology. The remaining entries were less than laudatory: Jackson Toby (1979) excoriated Quinney's essay as "sentimental over-identification with the underdog masquerading as science"; Carl Klockars (1979) decried that "because such work is more akin to religious prophesy than criminology, it presently appears to be irreconcilable with all heretofore accepted standards of academic scholarship"; Austin Turk (1979) was no less acerbic, and referred to the "partisanry" of radical criminology as being "characterized by dogmatic theories whose validity is politically rather than scientifically determined"; the then-president of the ASC wrote that the vision charted by criminology of the type espoused by Quinney was "unjust and repressive" (Akers 1979).

As an instance of formalized disciplinary collective action, the flagship journal's special issue excommunicated radical criminology from the American scene for the foreseeable future. While it was exported to the UK, Germany, Italy, and beyond (Shank, 1999), radical criminology would reappear in the United States only after it had been filtered through European 'New Left' critical criminology (Heidt, 2011), which disavowed much of the activism that characterized Berkeley's version. Radical criminology therefore did not 'fail' as a scientific research program so much as it was transformed and relegated to a low status niche within criminology more broadly. Similarly, administrative criminology did not 'fail' as much as it became displaced to more propitious campuses where it was able to flourish beyond the parochialism that characterized Berkeley's scholarship before the mid-1960s.¹⁰

Radical criminology's comparatively inferior prestige complicates Frickel and Gross' (2005) thesis. Both radical criminology and administrative criminology emerged in the same institutional space, and yet they fared relatively worse than law and society at Berkeley. This is especially peculiar in light of the fact that – at least according to Frickel and Gross' thesis – it seems as though radical criminology satisfied the requirements to have survived the School's closure: it was wildly popular, with ballooning student enrolment; its scholarship was respected outside of the School; it maintained a dense collaborative network among its students, including the publication of influential journals and books; and it maintained successful links with progressive criminal justice policy reformers. Clearly, more is required to distinguish the comparative successes or failures of the intellectual movements within criminology than an analysis of the truth or falsity of their respective research agendas alone (Barnes, 1977).¹¹

Radical criminology's fate is much more understandable when viewed as a complex process of *institutional* change. Instead of conceiving of the School as a monolithic organization, it must instead be theorized as a space in which actors contested the legitimacy of different criminological

¹⁰ Perhaps there is some continuity between Berkeley's administrative criminology and the knowledge typically produced in criminology departments at state colleges today. However, it relates purely to the preoccupation with informing criminal justice policy as the primary normative concern of the scientific enterprise. Other continuities may be plausible, but they deserve to be treated as separate genealogies within criminology (Garland, 1992) and are thus well beyond my current scope.

¹¹ In this respect, the approach adopted by Laub and Sampson (1991) in evaluating the legacies of Sutherland and the Gluecks is helpful. They contend that Sutherland's ascendancy in the academy was attributable to broader shifts in the socio-intellectual context of the 1930s, the academy, and the discipline, rather than an exclusively academic assessment of the content of his or the Gluecks' scholarship.

traditions with respect to differently circumscribed institutional fields (see also Camic's [1995] analysis of similar developments in the early days of the sociology departments at Columbia, Harvard, and Chicago, which situates those scientific research programs within a wider context of jurisdictional disputes between neighboring departments housed across campus and in a wider field). Both radical criminology and administrative criminology engaged with the criminal justice policy community and with the academic criminological enterprise; law and society engaged the Law School and the social scientific enterprise. Each of the three scientific movements predicated their institutional legitimacy on a different set of epistemic premises about the aspirations and methods of criminological scholarship. When the criminal justice policy community withdrew its support for the School's intellectual project, administrative criminology dwindled. In this context law and society scholars articulated dissatisfaction with the prevailing intellectual tendencies supportive of criminological scholarship directed at refining criminal justice operations. While this supported the *emergence* of the new scientific movement of law and society, the scientific enterprise they advocated relied on objective scientific analysis that drew less on legitimation from the criminal justice policy community than from the Law School. Therefore, their legitimacy was *sustained* despite the disappearance of criminal justice support for the School.

Curiously, radical criminology engaged a similar institutional field to administrative criminology:¹² in being explicitly oriented toward the improvement of criminal justice agencies both through activism and systematic scholarly analysis, it was similarly reliant in the university's eyes on the legitimacy of the criminal justice community. That source of legitimacy, however, had been rescinded long before radical criminology emerged on campus. Without institutional support for the sustenance and stability of radical criminology the death of radical criminology becomes less salient than the question of how it managed to survive for so long.

CONCLUSION

This represents the first attempt to provide a comprehensive theorization of the Berkeley School of Criminology. Doing so requires building a history that steps beyond changes within the field of criminology alone, and is instead situated within a complex web of institutional and intellectual projects, some of which may not have been directly related to criminology at all (Rafter, 2010). Neo-institutional theory facilitates analyzing the ways in which legitimacy permeated assessments of the viability of the three competing scientific research programs housed within the School. Those 'criminologies' each relied on different sets of epistemic premises and normative aspirations about the goals of criminological scholarship, and they thus occupied different institutional fields from which they solicited legitimacy and support. Both administrative and radical criminology sought to improve the operation of criminal justice policy and they each derived legitimacy from the successes they could claim in that domain, albeit in different ways. The institutional field that law and society occupied, however, meant that it derived legitimacy from different sources altogether. Precise specification of these different fields and sources of legitimacy clarifies how and why the three scientific research programs emerged, stabilized, or subsided.

¹² The notion that radical criminology shares important similarities – both normative and otherwise – with its liberal, administrative counterpart may be less surprising than the radicals at Berkeley originally anticipated (Groves and Sampson, 1987).

When framed in these terms, the legacy of the School becomes emblematic of the legitimacy of criminology as a discipline and of law enforcement as a profession for much of the early- and mid-twentieth century. Once the professionalization of law enforcement was satisfactorily displaced to the state college system, there was no longer a need to retain a high-profile University site for a vocationally oriented professional School of Criminology. Support for the School from external criminal justice policy practitioners disappeared, and with it an influential source of legitimacy. The dissolution of the School followed soon thereafter and the various criminological research programs were displaced either to new sites elsewhere on campus, or to a more suitable home among the state colleges, or to ignominy.

CHAPTER THREE: CONSTRUCTING THE CRIMINOLOGICAL FACT

Abstract

What do criminologists know, and how do they come to know it? In the shadow of criminology's mid-1960s 'fracture,' scholars redoubled their efforts to provide policy-makers with valuable insights about crime and justice. Doing so, in turn, required developing techniques with which criminologists might produce policy-informing criminological claims. I analyze the development and deployment of those techniques through an analysis of authoritative fact-issuing reports authored by committees assembled under the aegis of the National Academies of Sciences' Committee on Law and Justice. I study the construction of criminological facts by analyzing both the scientific consensus that appears in published reports' *frontstage* and the processes by which criminologists reached that consensus in the panels' *backstage*. Content analysis of thirty Consensus Study Reports revealed that the National Academies deploys criminological consensus either to reaffirm known wisdoms *inductively*, or to settle unknown ones *abductively*, or to intervene and chart a new research path *reflexively*. Interviews with eleven panel participants revealed that criminologists negotiated the scientific imperatives of dissent and consensus on one hand, and the political demands of policy-informing facts on the other, by strategically inhabiting and disinhabiting spaces of scientific, political, and moral discourse. Criminologists thus construct facts by resorting to a consensus that stabilizes tensions between positivist, progressive, and postmodern traditions.

Keywords

Sociology of criminology; criminological knowledge; consensus; facticity; evidence-based policy; National Academies

Consensus is a horizon that is never reached. . . . someone always comes along to disturb the order of ‘reason.’

Lyotard, *The Postmodern Condition*

INTRODUCTION

What do criminologists know, and how do they come to know it? A first-order priority in making demands for policy deference is the certitude that criminological expertise can valuably contribute to policy-making conversations about crime and justice. Criminology’s epistemic disarray emblemized by the Berkeley School of Criminology’s ‘fracture’ was in considerable part a function of an accompanying existential disarray about the criminologist’s proper role *vis-à-vis* the policy world. But operating in the shadow of criminology’s mid-1970s disrepair, criminologists redoubled their efforts to imbue observed facts with the credibility their science was due and to make compelling, policy-informing claims. Yet the history of those shared rules, whereby criminologists could establish, agree on, and make use of scientific facts, remains largely untouched.

In this chapter, I focus on a set of instances of formal scholarly collective action when criminologists assumed the mantle of deciding solutions to complex policy problems by issuing authoritative pronouncements with the imprimatur of the National Academies of Sciences (hereafter “NAS”). NAS reports ostensibly represent instances when a scientific discipline sorts the knowledge worth privileging from everything else, and in so doing they canonize important research questions and answers. NAS reports also emblemize a transformation described in Chapter One, when criminology refashioned itself from a discipline that *crystallized* in the years up until 1965 to one that *stratified* and then *institutionalized* in the years that followed. From roughly the mid-1960s onward, criminological thought reconstituted itself so as to make possible the kinds of policy-informing claims that would imbue the discipline with the legitimacy befitting a serious science.

To that end, the key vector of disciplinary legitimacy is the ‘policy-informing criminological claim’ that serves as this chapter’s unit of analysis. The rise of evidence-based crime and justice research, as I later show, tracks efforts to establish rules about what kinds of recommendations and guidance criminologists might offer the policy stage. Here, I study both the internal features of the claims that give those recommendations and guidances weight, and the mechanisms by which they are produced.

LITERATURE REVIEW

Scholars disagree about the point at which a scientific claim may defensibly inform policy. The Received Wisdom is that evidence never speaks for itself; if it is to inform policy at all, it must be deployed as part of a policy-informing argument (Cartwright & Hardie 2012; Pawson 2006).¹³ Evidence’s influence in the world is intelligible only through its deployment, and deployment is itself agentic, interpersonal, and strategic. But those strategic maneuvers are not well understood, in part because scientists are less prone to see themselves in the strategic business

¹³ It is this skepticism about evidence’s ability to ‘speak for itself’ that underscores the ‘underlaborer’ motif’s emptiness in commentaries about evidence-based policy-making (Pawson 2002a:176; *see, e.g.*, Loader & Sparks 2011).

of argument-construction than in the measurement business of observing and explaining phenomena (Lamont & Molnár 2002).

In science and technology studies, the deployment of evidence falls broadly within the study of “science advice” (Jasanoff 2009; Pielke & Klein 2010), which poses three inter-related questions: One concerns whether scientists may inform policy, and is thus a question of *professional remit* that touches on how a scientific community is positioned with regard to policy. Another concerns what constitutes a fact, and is thus a question of *scientific authority* that touches on what imbues a discovery with the trappings of credibility and legitimacy. Another concerns when criminologists may confidently make policy recommendations based on their knowledge, and is thus a question of *evidentiary thresholds* that touches on what kind of scientific knowledge may defensibly be projected onto the policy stage.

Professional remit

May scientists inform policy? The question presupposes that a choice exists between science advice on one hand and cloistered scientific reflection on the other. Three answers prevail: the *positivist*, the *progressive*, and the *postmodern*.

The *positivist* school of thought admits of a distinction between the policy-making and scientific spheres, but emphasizes that the boundaries separating the two are unassailable. For example, early sociologies of science leveraged Enlightenment beliefs in the divisibility of the natural and social worlds—what Laudan (1984:5–6) calls the “Leibnizian ideal.” They argued that natural truths found in the world would guard against scientists trespassing beyond their remit. They viewed the processes of scientific truth-making as wholly distinct from its products—this was especially the case for natural science. According to Mannheim (1929), it was the natural world’s autonomy from social perception that would keep the fallibilities of scientific truth-making in check. Merton reaffirmed Mannheim’s position for decades, asserting that “sooner or later, competing claims to validity are settled by the universalistic facts of nature.” (1957:554) The natural properties that inhere in, say, gravity and photosynthesis, so the argument followed, meant that natural scientists were “trying to discover the next page of a book that had already been written.” (Cole 1992:3) By implication, it was the scientist’s charge to know that which already existed in the world, not to engineer its essence.

Mannheim’s argument for science’s constraints became the bedrock of Merton’s sociology of scientific detachment. In *A Note on Science and Democracy* (1942), Merton enumerated the four institutional imperatives of science’s “ethos”: universalism, organized skepticism, communalism, and disinterestedness. The latter imperative of “disinterestedness,” in particular, represented Merton’s commitment to the notion that a scientist’s professional remit was orthogonal to her personal interests, howsoever construed. Neither disinterestedness nor the three other imperatives in Merton’s list made room for a Marxist exhortation to go from interpreting the world to changing it. Yet even though the positivist school of thought discouraged scientists from intervening in policy, nonetheless Merton crucially did *not* imagine disinterestedness in a ‘value-free’ sense. To the contrary, for Merton it represented the scientist’s preternatural inclination not to defraud and deceive, and the accompanying recognition that science’s legitimacy was pinned to the trust that consumers placed in its producers (Douglas 2009:46–47). Moreover, it was the *rejection* of Mertonian moralism that paved the way for the subsequent progressive ‘value-free’ backlash.

The *progressive* school of thought shares the positivist’s imagined separation between the political and the scientific, but welcomes the scientist’s role in shaping and guiding policy. After

the Mertonian disinterestedness dominated the philosophy of science through to the late-1950s, progressives engineered two intellectual shifts to make way for their competing position. Only upon first dislodging Merton's legacy that personal interests were anathema to sound science could scientists thereafter advocate in favor of intervening in progressive policy-making as part of their professional remit.

The first shift, surprisingly, came from Kuhn. It was the *Structure of Scientific Revolutions* that revived what by that point had been a defunct philosophy that imagined sound science as isolated from the world it inhabited. In his canonical formulation of "paradigm shifts," Kuhn (1962) suggested that science transitioned from one stage of "normal science" to another through disruptive "revolutions." Each successive paradigm evinced a healthy science through settling on agreed-upon norms that gave rise both to questions of mutual concern to scientists and to the techniques with which answers to answer those questions. It was this mutual investment in the questions worth asking and the tools with which to answer them that constituted the "paradigm," and it was only by preserving science's autonomy from the concerns of the world beyond—*especially* political concerns—that science could meaningfully continue (Kuhn 1962).

Kuhn, then, was responsible for re-injecting a 'value-free ideal' into scientific production (Douglas 2009:60–64). All that remained in solidifying the progressive approach to science's role in policy-making was to re-establish the promotion of social justice in the scientific enterprise. Evidence-based policy-making, in particular, thrives on this value-free ideal (*e.g.*, Harcourt 2018). Where policy problems are susceptible to choices between putatively non-ideological options, evidence-based policy-making seductively presents outcomes as costless Pareto improvements. At the soft end, it recasts science alone as the value-free interlocutor in a science-policy exchange (*e.g.*, Thacher 2015; Thacher & Rein 2004); at the hard end, it recasts even the policy itself as value-neutral, too (*e.g.*, Sunstein 2014; Thaler 2015).

The *postmodern* school of thought disputes altogether the premised distinction between 'the scientific' and 'the political.' For example, that current of Foucault's thought that most sustained his work—critiquing the contemporary conditions of "the human sciences"—was always an exercise in exposing how the construction of knowledge expressed and gave form to power's exercise as *savoir-faire* (Gutting 1989; Rabinow & Rose 2006). In earlier archaeological work on the psychological, psychiatric, and medical origins of knowledge, Foucault showed that science was never autonomous from the objects of study and was instead rooted in specific, contingent conceptions of the human body (Foucault 1961; 1963). Once Foucault moved from archaeology toward genealogy, the method changed but the argument remained. Criminology and pedagogy emerged to constitute the very disciplinary controls that they were tasked with studying (1975; 1976). The implication of Foucault's treatment of science is twofold: first, the scientific and the political constitute one another, inseparably so; second, the processes by which they do so are so close to the surface and so plainly in view that they make questioning science's role in policy-making laughable. One could ask whether scientists might abstain from informing policy as easily as one might ask what the week might look like without Tuesday.

In sum, the question of whether scientists should—or, indeed, whether they even can—inform policy presupposes a raft of further questions about how they understand their professional remit, and about the content of the science they produce. Those further questions might, but need not, encompass the extent to which the objects of science are found in the world or made through scientific discovery; what personal values a scientist brings to their work; what moralism undergirds the scientific enterprise; and the propriety of scientific intervention.

Scientific authority

What constitutes a scientific fact? Theories of science historically ascribe authority to a fact that the scientific community accepts as important or true (*e.g.*, Latour & Woolgar 2013). ‘Acceptance’ is thus the operative behavior in the manufacture of facticity, discernible through the formation of scientific *consensus*. Again, practitioners from positivist, progressive, and postmodern schools of thought imagine the process of consensus-formation in different ways.

The positivist school of thought treats consensus-formation as a straightforward and Whiggish process that inexorably leads toward an objective truth. Building on the premise that facts pre-exist in the world before scientists seek to discover them, positivists hold that science is a process of continual refinement and progress. Because nature’s constancy serves as a check against invalidity, each successive scientific test moves discovery incrementally closer toward an accumulated representation of underlying truth. By implication, at least in Bacon’s traditional formulation, scientists could and would ultimately know all there was to be known (Rosenberg 2012). The premise of objective truth is thus as essential to the production of positivist facticity as the processes of consensus-formation with which that truth becomes discernible. Both presume that scientific knowledge is somehow autonomous from its beholder, and that the best ideas acquire legitimacy on the merits of their intrinsic truth-telling value.

Neither the progressive nor the postmodern schools of thought countenance the objective purism found among their positivist counterpart (*see, e.g.*, Berger & Luckmann 1966; Myrdal 1969; Poovey 1998). They also both abandon the Whiggish teleology that imagines scientific discovery as one of unilinear and untrammelled progress. On the contrary, the progressive school of thought adopts a significantly more complicated view of facticity—one wherein processes of consensus-formation differ during moments of normal science and of revolution: although moments of revolution are characterized by an abrupt deterioration of consensus, those moments are nevertheless scarce and fleeting before then giving way to the stable consensus that characterizes normal science.

According to Kuhn, sustained disagreement indicates a discipline’s incapacity to ascribe scientific legitimacy to facts. On the other hand, consensus not only indicates mutual investment in a common enterprise, but also mobilization to police the boundaries of what constitutes a legitimate research question to ask and the legitimate methods with which to answer it (Kuhn 1962). Upon settling into a new normal state, scientists replace the wisdoms of the previous paradigm with those of the new. Crucially, however, because the adoption of a particular paradigm cannot be assessed against its capacity to yield objective truths, it accordingly cannot be categorized as superior or inferior to any paradigm that forewent it (Kuhn 1962). Kuhn’s model thus generates useful hypotheses with which to test the trajectories of scientific agreement over time (*see, e.g.*, Shwed & Bearman 2010); however, it is less informative for interrogating what processes of consensus-formation—even those limited to moments of normal science alone—imbue discoveries with the facticity from which scientific authority derives.

The postmodern school of thought emphasizes the social processes through which scientific truths are made. Unlike the progressive concern with whether consensus takes on one paradigm’s characteristics or another, the concern here is instead with the legitimacy that attaches to a discovery’s consensus through the recruitment of allies with authoritative capital. According to Actor-Network-Theory (Latour 2005), for example, scientific fact-making is less the pursuit of transcendent truths than the cultivation of networks of authority capable of buttressing a given

discovery's scientific legitimacy. The premise that evidence never speaks for itself finds its fullest expression here, where a discovery acquires meaningful support and consensus only once it has been deployed in ways that both marshal and capitalize on its political resonance. Where the actors within a network agree on both the substance of a claim and the means by which one produces it, so the theory suggests, the claim takes on self-reinforcing meaning and becomes a fact. Thus, scientific consensus is its own intrinsic virtue in the production of scientific facts.

In sum, consensus and objectivity are the bread and butter of scientific fact-making. Although schools of thought differ about their interplay, no framework eschews both altogether. Yet of the two, it is objectivity that has endured sustained critical assault extending more than half a century. Consensus, on the other hand, has gone largely unnoticed as underpinning 'the criminological fact.' This is unusual for two reasons: first, although the positivist, progressive, and postmodern approaches outlined above differ in the extent to which they rest on the discovery of 'objective' truths, they all share as a premise that consensus matters to the discovery of scientific facts; second, consensus is ripe for the kind of analysis necessary to make sense of how criminologists imbue claims with the legitimacy necessary to inform policy.

The puzzle is that, like the process of identifying 'objective' truths for which no known method exists whereby scientists might remove their subjective biases, so too no method exists whereby scientists may set their personalities aside and render consensus-based truths (Martini & Boumans 2014). Instances of scientific consensus are so rare that they may be altogether illusory. After all, scientists struggle to agree on much, and they rarely if ever agree on everything—and the foregoing description of positivist, progressive, and postmodern schools of thought attests that there is not even a consensus about the virtues of scientific consensus. At the very moment that it appears on the "horizon," to borrow Lyotard's epigrammatic metaphor, it spurs scientists to upend the received wisdom in favor of a new one (*see also* Feyerabend 1993).

This, then, is the Freudian Bind of Scientific Knowledge Production. On one hand, a scientific claim requires core agreement before it can be elevated to the status of a fact; scientists must therefore reach a consensus about the conventional wisdoms upon which future contributions build. On the other hand, a scientific claim invites refutation and even falsification as the basis of intellectual development and progress. To sell a proposal on the policy stage, it must be the product of a scientific consensus, but the very notion of a scientific "discovery" presupposes that no consensus exists on it yet. Although intellectual contributions must build on those that forewent them, prestige and value attach to a novel contribution only insofar as it can be distinguished from its precursor. Scientific knowledge production this requires aligning oneself with one parent and repudiating another.

Evidentiary thresholds

At what point may scientists defensibly guide or recommend policy change on the basis of the knowledge they produce? Even if criminologists are capable of observing facts about crime and justice, they must then decide at what point those facts warrant performance on the policymaking stage. This is ordinarily understood through the frame of evidentiary uncertainty (*e.g.*, Petrosino et al. 2001), which holds that tentative findings do not earn such a warrant and certain ones do. Common interpretations of this frame follow either a Bayesian approach that imagines science

advice as updating one's prior belief about the validity of a given policy, or a Berkeleyan¹⁴ approach that imagines science advice as a process of accounting for and exposing the scope of the unknown.

The Bayesian approach imagines science advice as incrementally informing decisions about an otherwise uncertain world. The traditional formulation of an “experimenting society” (Donald Campbell 1969; 1973; 1991; Dunn 1998) casts all state intervention as a form of experimentation on the social world, and all policy reform as an effort to ‘update’ that intervention so as to better achieve a politically desired end. In this formulation, the distinctive value of the social scientist is to offer the tools with which that (routine and unavoidable) state experimentation might instead proceed with rigor. The social scientist is thus in this sense a “methodological servant of the experimenting society” (1973). Accordingly, The Bayesian approach to science advice openly embraces setting a lower threshold for the point at which evidence earns its policy-informing warrant than it does for science (*e.g.*, Blomberg et al 2013; Nagin & Weisburd 2013). Because science is but a special case of policy-informing experimentation, evidence need offer only marginally superior certainty to what is already known to clear the bar of policy-making usefulness (Sherman & Cohn 1989). Whichever insight produces the most certain guidance available at the point that new policy must be adopted will serve as a basis for sound, evidence-based policy reform (McCord 2003; Sherman 2009; Weisburd 2003).

Critics worry about the low threshold that the Bayesian approach sets for policy-informing evidence. In response, although the Berkeleyan approach to science advice shares the Bayesian premise that scientists work toward providing certainties in an uncertain world, it instead introduces more skepticism about scientists' capacity to yield credible policy-informing guidance. The epistemological challenge of science advice, as Manski (2007; 2013) explains, is that the error bars associated with most scientific findings are untenably wide to support credible policy-informing guidance. The mismatch between science's better-suited capacity to yield confidence bounds and policy-makers' thirst for point estimates gives rise to the anecdote that an economist presented a forecast as a likely range of values for a quantity under discussion, to which Lyndon Johnson brusquely replied “Ranges are for cattle. Give me a number.” (Manski 2007:8). The Berkeleyan challenge to science advice is thus one of epistemological humility about a scientific finding's reach in a policy circle. That humility springs from the deduction that the majority of science advice is inapposite to the complexities of the policy-making process (Cartwright & Hardie 2012; Deaton & Cartwright 2018; Greenhalgh & Russell 2009; Sampson et al. 2013). Accordingly, the evidentiary threshold for policy-informing knowledge is much higher in the Berkeleyan approach than its Bayesian counterpart.

Both the Bayesian and Berkeleyan approaches understand the threshold point at which scientific evidence may inform policy as a function of that evidence's ‘scientific’ or ‘political’

¹⁴ To avoid confusion with the previous chapter, “Berkeleyan” here does not refer to the Berkeley School of Criminology except indirectly through its campus's namesake. George Berkeley—Reverend Bayes' contemporary and chief antagonist—authored the eighteenth-century's foremost epistemological rebuke to Newtonian calculus and, by implication, Bayesian mathematics in *The Analyst: A Discourse Addressed to an Infidel Mathematician* (1734). In *The Analyst*, Berkeley satirized the leaps of faith he found endemic to untenably irreligious mathematical proofs; by extension, where Bayesian approaches to science advice predicate sound policy on science's capacity to update a policymaker's priors, Berkeleyan approaches caution against predicating policy on immodest leaps of scientific faith.

certainty. On one hand, the Bayesian approach imagines scientific and political knowledge as coterminous, but constitutes a dividing boundary between the scientific and the political based on identity distinctions between scientists and laypeople. All knowledge informs policy, so the proponent of Bayesian science advice holds, but scientists distinctively produce more certain knowledge and will—in the long run—guide an experimenting society toward better policy. On the other hand, the Berkeleyan approach imagines a dividing boundary between the scientific and the political altogether. Scientific discovery is fundamentally limited, so the proponent of Berkeleyan science advice holds, and cloaking policy-informing recommendations in the guise of a scientific evidence base is likely both to mistakenly bless unscientific findings with scientific oil, and to mislead the credulous consumers of those recommendations into taking ill-gotten leaps of false faith.

Criminologists play out the tensions between the Bayesian and Berkeleyan approaches to science advice in the emergent field of “translational criminology” (*e.g.*, see Laub & Frisch 2016; see also the establishment in 2011 of the magazine *Translational Criminology*). There, criminologists deliberate at what point science may defensibly guide policy change in a context limited science on one hand and increasingly urgent demands for policy-informing knowledge on the other. Reconciling these two constraints requires reaching a consensus about the appropriate evidentiary threshold beyond which scientific knowledge may properly guide policy.

METHODS

In this chapter, I study the processes by which criminologists produce ‘policy-informing criminological claims.’ I draw on data collected from interviews with criminologists who have collaborated in the production of official Consensus Study Reports that the National Academy of Sciences published under the National Research Council’s Committee on Law and Justice.

Site Selection

The NAS ostensibly produces the most authoritative statements that a scientific community is capable of issuing. The NAS originated in 1863 by an act of Congress after the Civil War had reached its bloodiest apex and President Lincoln had begun to give thought to the country’s postwar administration. Designed with the remit to provide scientific and technological advice to the nation, the NAS was one of the earliest instantiations of both Lincolnian proto-Reconstruction and of the coming Progressive Era. Although Andrew Johnson abandoned Lincoln’s vision for a muscular and interventionist central government upon the war’s conclusion (Foner 2011), the latter imprinted a technocratic zeitgeist that would flourish for the next fifty years into the Academy’s institutional core, and would elevate the Academies to the forefront of a wider movement to pin state operations to scientific and technological expertise. Three of the Academy’s features constitute and reinforce that elevated stature: its technological expertise, its political independence, and its policy-informing mandate (Boffey 1975).

All three of these features bear on the production of scientific facts. Take, first, the Academies’ technological expertise. Ostensibly, the Academies function as an honorary institution responsible for the conferment of membership that draws from among more than 2,000 of the nation’s foremost technical minds, and it burnishes its credentials accordingly at the vanguard of American science. The kind of expertise for which the Academy stands is thus of a narrow sort: it is hierarchical, and is legitimated by reference to the structures of approval that exist beyond the

Academies alone. Technological expertise in this sense is a consensus-based claim about the allyships that have conferred expert status on its members.

Next, consider the Academies political independence. The NAS is not a federal agency and as a result it admits no appropriated public monies from Congress or beyond. The overheads for its operational and administrative sustenance are small, which is in part a function of relying exclusively on the pro bono labor that experts contribute to the panels, symposia, and reports that the Academies produce. Funding for those research outputs, in turn, derives primarily from contracts and grants that are earmarked to specific projects. In this second sense, consensus imbues political independence with its own self-reinforcing argumentative heft: in the Academies' eyes, neither is the science it produces ideologically-driven, nor would ideology threaten the conclusions of any given report because the consensus that gave rise to those conclusions was itself a bipartisan product, thus rendering ideology moot.

This political independence enables the NAS to inhabit an idiosyncratic position at the fringes of both federal and private oversight. Efforts to pull back the front-stage curtains of its research processes and peek into its back-stage have for the most part been unsuccessful (Hilgartner 2000:Ch.2). The Academy is insulated from the Freedom of Information Act (FOIA) altogether, thanks to the NAS's successful petition to the District of Columbia Circuit in the mid-1970s, whereupon the court was convinced that the FOIA's spirit of openness and transparency would compromise the NAS's objectivity (*Lombardo v. Handler*, 1976). Separately, the Federal Advisory Committee Act (FACA) wields no governing force over the Academies, except for the narrow class of public access hearings that intermittently punctuate the NAS's main operational rhythms. However, even this loophole came only at the conclusion of an embittered contest that ascended to the US Supreme Court, in which the Academies contended that submission to the full complement of the FACA's requirements would irretrievably undermine the NAS's independence. The Academies ultimately prevailed only once Congressional intervention in 1997 categorically removed the Academy from the offending FACA clauses altogether. The fragile balance between transparency and secrecy is thus a core feature of the NAS's work, and this remains the only study of which I am aware that reveals the processes that produce Consensus Study Reports.

Finally, the Academy's initiating Lincolnian charter vests it with singular authority to make recommendations to the government on matters requiring scientific advice. Historically, the NAS's mandate encompassed technocratic questions of a different sort than those with which the Committee on Law and Justice (CLJ) typically grapples today. CLJ first appeared within the Academies' Division of Behavioral and Social Sciences and Education as the Committee on Research on Law Enforcement and Criminal Justice in 1975, and it ultimately changed its name to the Committee on Law and Justice with the mission "to improve governmental decision making and public policy, and promote the understanding and dissemination of research in matters involving law and justice." (National Academies 2019) That mandate has since expanded once the establishment of the companion Academies of Engineering and Medicine absorbed the NAS's more mechanical and logistical question-answering functions. Now, the CLJ is housed within a dedicated Division of Behavioral and Social Sciences and Education, for which the profile of commissioned reports takes on questions of targeted social-scientific significance while still maintaining the institution's advisory ambitions.

The combination of the NAS's technological expertise, its jealously guarded political independence, and its technocratic spirit thus present an ideal site from which to discern how criminologists set the agenda of key scientific facts. These three features embed an institutional

tilt in favor of a style of scientific reasoning that is at once positivist in its disinterestedness, progressive in its ambition to inform policy through social science, and postmodern in its recruitment of prestige.

Although the NAS's full suite of activities encompasses briefings, conferences, policy studies, and much more besides, the institution's highest-profile activity is the production of Consensus Study Reports (hereafter "CSRs"). A CSR bears more of the trappings of a *per curiam* judicial opinion from a high court than of a scientific monograph from a university employee. Its authorship byline names the panel committee as a whole as opposed to its individual panelists, their deliberations are shrouded in secrecy but the published CSR is freely available for the public to download, and the CSR represents the culmination of a process that has whittled different perspectives to a proposed research question into a single answer, and has polished distinctive expressions of the contributing panelists' voices into a patina of univocality. Both the consensus at a CSR's core and the formation of that consensus thus provides a window into the discursive tools at criminologists' disposal and the ends to which they wield them, and the health of criminological science writ large.

CSRs therefore perform a drama of epistemic and aesthetic credibility. That drama plays out on both a backstage of strategic negotiation between panelists whose views might differ, and on a frontstage where disagreements have been made invisible (Hilgartner 2000; *see also* Vaughan 2006; 2016). Moreover, the authority they command in the field imbues them with the authoritative heft to canonize knowledge, which allows them to serve either as consolidating existing wisdoms, or correcting misplaced ones, or redirecting future lines of research, or assigning the path ahead with the NAS's imprimatur. Each of these maneuvers provides insight into knowledge-ordering techniques and the construction of criminological facts. However, these maneuvers happen on *both* sides of the dramaturgical curtain—both in the published report and in the processes of their manufacture and reception. A full understanding of how reports *construct the criminological fact* demands an analysis of what happens on both sides of that dramatic curtain. I therefore gather data on both the CSR's frontstage and its backstage.

Data and Coding

The National Research Council's Committee on Law and Justice has produced forty-seven CSRs since it was first established in the mid-1970s. I hand-coded their relevance to the present study, which resulted in excluding ten reports for their narrow applicability to forensic science and a further seven reports for their broad applicability beyond criminology alone. Titles of the thirty eligible CSRs appear in Appendix 3.A, together with the italicized titles of the seventeen excluded reports.

By definition, each CSR's frontstage was visible in the published report. I coded each of the thirty eligible reports into ideal-types. Following Weber, these ideal-types emerged not as *a priori* constructs but rather as an imposed ordering of homogeneous attributes that are rarely constrained to a single dimension (Hendricks & Peters 1973). In the canonical formulation, they represent "a great many diffuse, discrete, more or less present and occasionally absent concrete individual phenomena, which are arranged ... into a unified *analytical* construct (*Gedankenbild*). In its conceptual purity, this *Gedankenbild* cannot be found anywhere in reality." (Weber 1949:90) The ideal-typical categories into which I coded the thirty eligible CSRs were thus not "real" except insofar as they captured thematic patterns in the methodological, substantive, or normative orientations contained within the text. Reports could plausibly have fallen anywhere along a three-

poled spectrum in between these three ideal-types. Their assignment into one category as opposed to another is therefore deliberately more argumentatively useful than analytically true.

To analyze the backstage construction of criminological facts, I conducted interviews with panelists who contributed to the authorship of CSRs that the CLJ had commissioned. The composition of the thirty eligible panels ranged in number from five panelists to twenty with a mean of 13.6, and many panelists contributed to multiple CSRs. Consequently, the subset of living panelists drawn from among the 261 discrete contributing chairs, vice-chairs, and members—thus excepting Study Directors, Staff Associates, Consultants, and other panel roles ordinarily drawn for their operational rather than intellectual expertise—constitute this study’s sampling frame. I purposively sampled panelists who agreed to face-to-face interviews. This chapter draws on a sample of eleven hour-long interviews with panelists listed in Appendix 3.B, who collectively served on seventeen of the thirty eligible panels. Quotations from interviewees appear in square brackets throughout.

Each interview followed a semi-structured script that encompassed questions divided into two sections. In the first instance the questions pertained to the interviewee’s understanding of the meaning that CSRs hold in criminology generally before they then delved into the specific CSR(s) to which the interviewee had contributed. I asked about how the interviewee came to be included in the report, how they interpreted and responded to the ‘statement of task’ presented to the panel, and how they believed the report had been received. In the second section of the interview, I then asked about how the interviewee interpreted and understood the role that the report played in the wider criminological, public, and political field. To that end, I asked what aspirations they held for the report, and the extent to which the report met those hopes, before I asked about how the interviewee approaches criminological work in light of the themes discussed above. Interviewees steered the question sequence, and I also presented them with the opportunity to speak about any unprompted aspect of the report or topic related to our conversation that they hoped to share. In some instances, interviewees exercised their right to impose anonymity, but none exercised the prerogative to discontinue the interview altogether that I made clear to them at the interview’s outset.

I coded the interview transcripts according to emergent themes that coalesced around what a policy-informing criminological claim might be, how it was constituted, and what rules governed its use. Referring to a ‘policy-informing criminological claim’ is a question-begging description of this chapter’s unit of analysis, and one that deserves further elaboration, given that what is in question is precisely how one defines each of the unit’s components. For example, to presuppose that ‘evidence never speaks for itself’ is to render the evidence in question inert. This is defensible only up to a point, and it is precisely where that point lies that serves as one of this chapter’s explanandi. How something moves across analytical categories will thus form the basis of the results that follow; for now, it will suffice merely to arrange the terms. Let us call piece of evidence that is inert in the manner described above a ‘fact,’ and once it is argumentatively deployed for some use it transforms into a ‘claim.’ Let us call such a claim ‘policy-informing’ if the use to which the evidence has been put is one that seeks to offer guidance beyond features that are intrinsic to science alone, such as the observation and explanation of the given fact. Finally, let us call a policy-informing claim ‘criminological’ if it relies on the trappings of authority that science about crime might lend. Each of these distinctions contains implications that may be counterproductive. Most notably, they reinforce boundaries between the scientific and the political that the results in fact belie. At the same time, however, they sharpen focus for the discursive and

rhetorical rules on which evidence-based crime and justice research rests, and therein lies the Findings' import.

RESULTS

Frontstage

The thirty CSRs deemed relevant to the formation of policy-informing criminological claims were divisible into three ideal-types: *inductive*, *abductive*, and *reflexive* (see Table 3.1). *Inductive* reports presented generalized descriptive or explanatory conclusions based on an appraisal of the state of the scientific field. They ratified existing wisdoms about which the field manifested little to no disagreement, and offered the canonizing imprimatur of the NAS to legitimate a statement behind which the field already stood.

Table 3.1: APPRAISING THE COMMITTEE ON LAW AND JUSTICE'S CONSENSUS STUDY REPORTS. The thirty CSRs that were eligible for inclusion in this study have been coded into *inductive*, *abductive*, or *reflexive* ideal-types.

Year	Title	Inductive	Abductiv	Reflexive
2018	Modernizing crime statistics, vol. II: New systems for measuring crime			X
2018	Proactive policing: Effects on crime and communities		X	
2016	Preventing bullying through science, policy, and practice		X	
2016	Modernizing crime statistics, vol. I: Defining and classifying crime			X
2015	Understanding the US illicit tobacco market: Characteristics, policy context, and lessons from international experiences	X		
2014	Implementing juvenile justice reform: The federal role		X	
2014	The growth of incarceration in the United States: Exploring causes and consequences	X		
2014	Estimating the incidence of rape and sexual assault	X		
2013	Confronting commercial sexual exploitation and sex trafficking of minors in the United States		X	
2013	Priorities for research to reduce the threat of firearm-related violence		X	
2013	Reforming juvenile justice: A developmental approach		X	
2012	Deterrence and the death penalty		X	
2011	Budgeting for immigration enforcement: A path to better performance		X	
2010	Strengthening the National Institute of Justice			X
2010	Understanding the demand for illegal drugs	X		
2009	Ensuring the quality, credibility, and relevance of US justice statistics			X
2008	Surveying victims: Options for conducting the National Crime Victimization Survey			X
2008	Parole, desistance from crime, and community integration	X		
2005	Improving evaluation of anticrime programs			X
2005	Firearms and violence: A critical review	X		
2004	Fairness and effectiveness in policing: The evidence		X	
2001	Informing America's policy on illegal drugs: What we don't know keeps hurting us		X	
2001	Juvenile crime, juvenile justice	X		
1994	Violence in urban America: Mobilizing a response		X	
1986	Criminal careers and "career criminals," vol. II	X		
1986	Criminal careers and "career criminals," vol. I	X		
1983	Research on sentencing: The search for reform, vol. II	X		
1983	Research on sentencing: The search for reform, vol. I	X		
1978	Deterrence and incapacitation: Estimating the effects of criminal sanctions on crime rates		X	
1977	Understanding crime: An evaluation of the National Institute of Law Enforcement and Criminal Justice	X		

Abductive reports, like their inductive counterpart, presented appraisals of the state of the scientific field. However, unlike the inductive report's production of generalized descriptive or explanatory conclusions, abductive reports instead either yielded tentative programmatic policy statements or resolved technical disputes (see Timmermans & Tavory 2012). Moreover, whereas inductive reports ratified existing wisdoms, abductive reports instead anointed a preferred scientific solution to a (putatively unanswered) policy conundrum. They therefore wielded the NAS's canonizing imprimatur to impose an answer where one might have been absent, or by quieting the noise that conflicting policy recommendations had stirred. Inductive reports were thus modestly targeted toward the summary of knowledge rather than the redirection of policy; abductive reports were instead targeted toward the modification of perceptions about key questions of uncertainty or dispute.

Finally, *reflexive* reports took a different form altogether than their inductive and abductive counterparts. Where an appraisal of the state of the scientific field served as the backbone of inductive and abductive reports, such appraisals were instead incidental to the production of reflexive reports. Reflexive reports were more wholly consumed by either the research process itself, or the infrastructure necessary for ensuring the health of that research process.

Each ideal-type thus leveraged the NAS for different consensus-based ends. A brief overview of two CSRs illustrates how inductive reports deployed the NAS imprimatur to ratify an existing wisdom. First, the double-volume report on *Criminal Careers and 'Career Criminals'* (1986) evaluated the feasibility of criminal career prediction. The report reviewed the fruitfulness of basic research on criminal careers, and it assessed the reliability and validity of actuarial instruments in predicting and reducing crime. Before the report's publication, although developmental and life-course criminology had been an inchoate subfield, the field's positive early reception of the Philadelphia Birth Cohort Study (Wolfgang et al. 1987) ensured that developmental criminology was by no means fringe or avant-garde. Similarly, after the report's publication, criminal career research enjoyed a period of paradigmatic ferment characterized by empirical refinement and theoretical extension, but not invalidation (Piquero et al. 2003).

More recently, the 2014 report on *The Growth of Incarceration in the United States* assembled evidence about the causes of mass incarceration and its consequences for families, communities, and American society. The report's conclusions were primarily descriptive, such as the observation that the foregoing forty-year rise in American incarceration had been "historically unprecedented" and "internationally unique," (p.2) its considerable harms disproportionately befell the marginalized, that those harms extended beyond punishment and were difficult to account, and that the uncertain effects of punitive penal policy on crime had been at best modest and more likely had been counterproductive (2014). Like the conclusions of the *Criminal Careers* report, those contained in the *Growth of Incarceration* report in the main spoke what was already criminology's common tongue. Where the report took a firm stand—such as describing or explaining mass incarceration—it ratified known facts; where the report soft-pedaled its conclusions—such as in the certainty of incarceration's deterrent effect or in the centrality of race to American criminal justice—it reproduced the field's existing hesitations.

Both the *Criminal Careers* and *Growth of Incarceration* reports were representative of other inductive reports in both substance, if not in form. In substance, all twelve inductive reports ratified those generalized descriptive or explanatory conclusions that had already garnered significant field-wide support. In form, however, the *Growth of Incarceration* was an outlier from the remaining eleven inductive reports. That panel's composition drew from a wide and diverse

disciplinary set encompassing psychologists, historians, sociologists, lawyers, statisticians, and non-academics. This differed from the panel composition found among the other eleven reports, which typically drew from a much narrower and more homogeneous scholarly profile. For example, the 2015 report on *Understanding the US Illicit Tobacco Market* drew primarily on health policy economists and epidemiologists, with two lawyers; the 2014 report on *Estimating the Incidence of Rape and Sexual Assault* drew exclusively on survey methodologists from sociology and statistics; and the 2008 report on *Parole, Desistance from Crime, and Community Integration* drew sociologically trained criminologists and public policy scholars and one lawyer.

Inductive reports, therefore, constructed consensus not as a matter of the panel's manufacture, but rather as a matter of design. Inductive CSRs served to reproduce and legitimate a consensus that preceded the report: this was visible both in the state of the scientific field into which the panel intervened, and in the profile of scholars represented in the panel's composition.

Examples of abductive reports reveal how they differed from inductive reports. Both the 2012 report on *Deterrence and the Death Penalty* and its forerunning 1978 report on *Deterrence and Incapacitation* confronted a matter of abiding policy uncertainty. After *Gregg v. Georgia* (1976) halted America's four-year moratorium on the death penalty, the 1978 report concluded that "available studies provide no useful evidence on the deterrent effect of capital punishment (NRC 1978: 9). Three and a half decades thereafter, the scientific community's uncertainty appeared to linger and the disarray of "widely varying conclusions" (2012: x) triggered the NAS to impanel a successor report. The conclusions of the 2012 report echoed those that had appeared first in 1978, finding that "research to date on the effect of capital punishment on homicide is not informative about whether capital punishment increases, decreases, or has no effect on homicide rates." (2012:2) However, unlike the 1978 report, the panel in 2012 further concluded on its second page that

these studies not be used to inform deliberations requiring judgments about the effect of the death penalty on homicide. Consequently, claims that research demonstrates that capital punishment decreases or increases the homicide rate by a specified amount or has no effect on the homicide rate should not influence policy judgments about capital punishment.

Where the 1978 study had abandoned scientific debate about capital punishment's moral justification, the 2012 panel attempted to make obsolete the study of capital punishment's deterrent effect. Consequently, the consensus that both abductive reports imposed on death penalty research foreclosed altogether criminology's role in subsequent policy deliberations about the value of capital punishment in American society.

The substance and form of abductive reports differs markedly from what is to be found in their inductive counterparts. In substance, all twelve abductive reports positioned themselves to yield tentative programmatic policy statements or resolve technical disputes that had thrown the field into disarray. In form, the twelve abductive reports shared roughly the same average panel size, averaging just below fourteen panelists per panel. However, abductive reports displayed a striking diversity in their disciplinary profile. For example, the 2016 report on *Preventing Bullying* included experts in nursing, health, law, education, and medicine; the 2013 report on *Sex Trafficking* included experts in law, public health, clinical psychology, medicine, nursing, and public policy; and the 2001 report on *Informing America's Policy on Illegal Drugs* included experts in economics, sociology, statistics, law, political science, medicine, public policy, and

health. Moreover, in addition to scholarly experts, abductive reports also included a robust representation of non-academic panelists drawn from law enforcement, policy-making, and the judiciary.

Abductive reports, therefore, constructed consensus not by legitimating a settled wisdom that prevailed in the field, but rather by intervening into a contested clamor and imposing quietude. Accordingly, the burden of proof for discontinuing a line of research was significantly higher among abductive reports than it was for inductive reports. The elevated burden of proof among abductive reports accordingly necessitated a more robust consensus manifested through a diverse disciplinary representation in the panel's composition.

Finally, reflexive reports differed from both inductive and abductive reports. For example, the 2009 report on *Ensuring the quality of US Justice Statistics* conducted a sweeping appraisal of programs that the Bureau of Justice Statistics (BJS) administered. The result was a defense of both the agency's political independence and its scientific objectivity (p.8), a proposal to remove the agency from the Office of Justice Programs (OJP) and to modify its director's appointment process (p.9), and the promotion of BJS data visibility during legislation and policy development (p.18). Similarly, the two-volume report on *Modernizing Crime Statistics* that appeared in 2016 and 2018 responded to a charge that BJS and the Federal Bureau of Investigation (FBI) had set to develop a new classification of crime, to optimize data collection systems, and to propose how such systems might be effectuated (2016:1). The result was a technical overview of criminal justice data collection instruments, an emphasis on an "incident"-based measurement of crime, and a proposal to rearrange the governance of data collection authorizing the Office of Management and Budget (OMB) to oversee the advancement of the panel's recommendations.

Because reflexive reports served a very different function than their inductive and abductive counterparts, they accordingly wielded consensus in a very different fashion. All six reflexive reports evaluated the role and scope of criminology and criminology-adjacent fields in the production of policy-informing knowledge. They then appraised the extent to which the field succeeded in meeting those aims, and what operational and administrative modifications would promote those goals. Therefore, their conclusions spanned boundaries internal to science (*e.g.*, through methodological refinements in evaluation research; NAS 2005) and external to them (*e.g.*, through increased support to the National Institute of Justice; NAS 2010). Unlike inductive reports, where consensus served as a declarative statement that ratified a known wisdom or abductive reports, where consensus served as a declarative answer to what was not yet settled, reflexive reports instead wielded consensus as an imperative request for a new status quo. The interventionism that undergirds requests of this sort both sets *and at the same time transgresses* boundaries that separate the scientific from the political. As a result, the petition is typically modest and cast in the rhetorical flourishes of being a narrowly scientific matter. This fits well when the request is intuitively internal to science, where privileging methodological rigor in evaluation research invites little opprobrium (NAS 2005). However, when the request transgresses into the political field, such as recommending that an appointee undergo Senatorial advice and consent, the appointment in question is limited to the BJS Director.

Some reports that were coded into one ideal-type might plausibly have fallen into another. This is most visible in those instances when the frontstage performance of consensus nonetheless reveals cracks in the curtain that allow glimpses into backstage dissent. Two examples illustrate this categorical liminality, one as an inductive CSR that appeared abductive, the other as an abductive CSR that might have appeared inductive. In the former, NRC rules provide recourse for

panelists to compose a dissent, but this occurred only once in the thirty reports in the sample, in the 2005 report on *Firearms and Violence: A Critical Review*. There, James Q. Wilson's three-page dissent critiqued the committee's interpretation of evidence on the relationship between right-to-carry laws and crime. Wilson complained that the committee had failed to follow the standard adopted in other CSRs in the face of uncertain answers to scientific controversy. In particular, he objected that the pertinent Chapter Six had not adjudicated which methodological approach would have yielded the most robust answers, and concluded that the evidence instead suggested that such laws suppressed homicides but had an ambiguous effect on other crimes (NRC 2005: Appendix A).

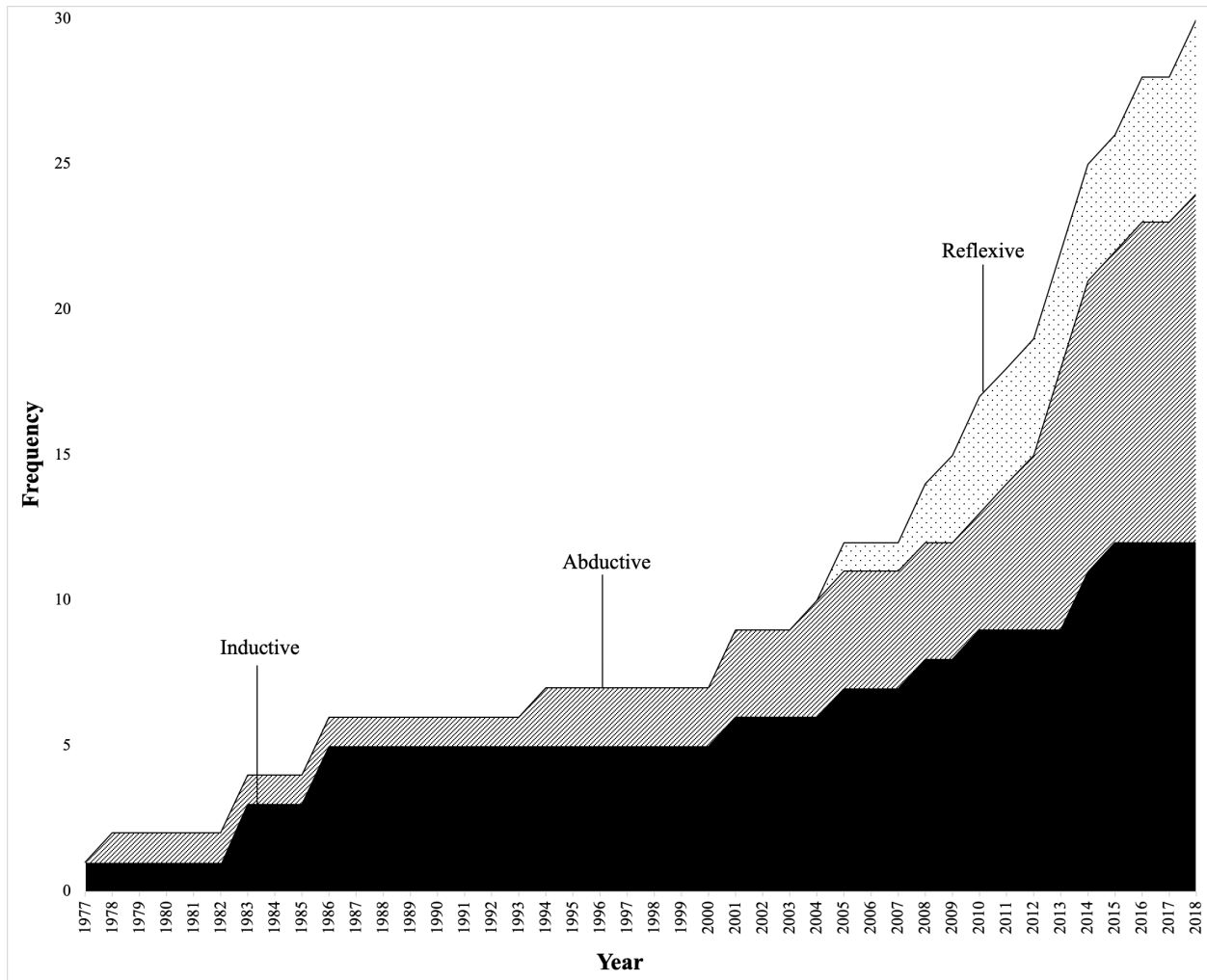
The committee marshaled a remarkable response to Wilson's dissent. In addition to multiple references to his dissent throughout the main body of the report, the committee also published a four-page "Response" in an appendiceal rebuttal. All of those references emphasized the narrowness of Wilson's gravamen as targeting "only" Chapter Six. "It is important to stress at the outset," the response opened, "that his dissent focuses on one part of one chapter of the report. Except for the effects of right-to-carry laws on homicide, the entire committee is in agreement on the material in Chapter 6 and the report overall." (NRC 2005:272) The response thus found Wilson's dissent "unconvincing" and "inaccurate." (NRC 2005:274)

The response's substance, however, is less noteworthy than its form. Unlike Wilson's dissent, authored by a single member, the response contains no personal byline. Large typeface announces that it appears on behalf of "The Committee," and its voice, too, is a clear stylistic reversion to the report's default 'author of record,' namely the committee in its totality. The response's form thus buttresses its substantive content that purports to refute Wilson's arguments on their merits. Moreover, the response's form affirms that Wilson's dissent had been given the due that transparency required and his misgivings had accordingly been absorbed into the report's unimpeached conclusions.

The *Firearms and Violence* report thus might have been considered an abductive report insofar as it settled a controversial question. But it is more accurately an inductive report insofar as its frontstage announces to the reader that, in fact, no new intervention has been made in the literature. The avowed purpose of the report, instead, codifies an *already*-settled criminological matter by assigning the NAS's imprimatur to a set of generalized descriptive and explanatory conclusions—hence the report's titular *Critical Review*.

The second example of a report that might have been differently classified is the 2001 report on *Informing America's Policy on Illegal Drugs*. Like the *Firearms and Violence* CSR, at first blush the *Illegal Drugs* report's frontstage performed a different purpose than a closer look might have suggested. Unlike *Firearms*, whose appendiceal dissent and response indicated that it was abductively settling a dispute when in fact it was inductively codifying an existing wisdom, *Illegal Drugs* instead bore the trappings of an inductive report that appeared to assemble drug policy scholarship's uncontroversial wisdoms when in fact it settled a heated criminological dispute. The dispute in question hinged on a contest between two studies that proffered competing approaches to the measurement of enforcement effectiveness for reducing cocaine consumption. Thus, where *Firearms* (2005) revealed the legitimating value of the NAS appearing *not* to have settled a dispute in favor of reiterating what was already familiar within the field, *Illegal Drugs* (2001) revealed the value of the NAS *intervening* to settle a scholarly dispute whose uncertainty undermined criminology's legitimacy on the policy stage.

Figure 3.1: CUMULATIVE PUBLICATION FREQUENCY OF THE COMMITTEE ON LAW AND JUSTICE'S CONSENSUS STUDY REPORTS, 1977–2018. Results are disaggregated by ideal-type, displaying the frequency of *inductive*, *abductive*, and *reflexive* CSRs published by year.



The cumulative publication frequency of CSRs appears in Figure 3.1, disaggregated by the cumulative frequency of inductive, abductive, and reflexive reports. The rate of total publication frequency increases dramatically in the twenty-first century, before which point the modal CSR is inductive. Criminological CSRs are less a phenomenon of the twentieth century than the twenty-first. The CLJ published only seven CSRs from its inception in the mid-1970s until 2001, after which point the remaining twenty-three reports have appeared at a steady rate of approximately one or two per year. After the century's turn, the CLJ turned its attention toward the production of abductive reports, and added the production of reflexive CSRs to its portfolio from 2005 onward. Abductive reports are now as numerous as inductive reports ($n = 12$ each), and reflexive reports now number significantly fewer ($n = 6$).

Backstage

Interviewees shared how the construction of policy-informing criminological claims differed between the inductive, abductive, and reflexive CSRs to which they contributed. Interviewees

focused in particular on the panel's deliberations over the *finding* to which policy guidance should attach, and the *role* they inhabited as a policy-advising scientist.

All eleven interviewees attested to the process of negotiation and contestation that produced consensus in the reports to which they contributed. When asked whether the final published report conformed to the one they expected to produce, all spoke about how the conclusion was at no point “foregone” and that compromise and debate generated the CSR's ultimate findings. But the process of constructing facts differed across panels that produced inductive, abductive, and reflexive reports. The chief difference was in the form and type of consensus that panels reached. Although interviewees commonly agreed that the NAS applied a heavy hand to ensure that the panel adhered to the statement of task—one interviewee jokingly described the NAS's interventionism during the review process as “torture” [Reuter]—the interviewees described three different kinds of consensus formation: consensus as *competence*, *deference*, and *product*.

Inductive reports were most commonly characterized by two forms of consensus, one of which was ‘thick’ (*consensus-as-competence*, for which interviews attributed consensus to the panelists' combined skill and expertise) and the other was ‘thin’ (*consensus-as-deference*, for which interviewees attributed consensus to the authorial partitioning of sections of a CSR to a panelist designated as uniquely well-qualified, and whose authority on that section went unquestioned).

In the *consensus-as-competence* model, interviewees described a kind of ‘thick’ consensus borne out of panelists negotiating competing commitments and marshaling them into a unified voice. Interviewees who had served on many different types of reports attested to their co-panelists' intellectual “horsepower” or “firepower” [Cook; Lauritsen; MacCoun; Meares; Nagin; Ratcliffe; Sherman]. Even when recalling disagreements—sometimes those inspiring outrage and embitterment—interviewees were quick to compliment their co-panelists' capabilities. But among those interviewees who had served on inductive reports, interviewees' responses evinced far less compromise and negotiation to reach consensus than had been the case among abductive and reflexive reports. For example, one interviewee who had served on both inductive and abductive reports differentiated the “strength” and “meaningfulness” of the conclusions between the two. He noted that for abductive reports,

[Nagin] part of reaching what I would call a strong and constructive [report] the committee was populated by a number of really able and themselves clear thinkers. And who were able to come together so each of them had the capacity and history of making strong [pause] being able to reach conclusions about some things that were meaningful.

The competences that generate strong consensus implicate an equally strong boundary between the knowledge that is included and the knowledge that is excluded in the report. One interviewee reflected, in particular, on how the insistence on consensual knowledge in inductive reports “leaves a lot out” [Hagan]. In response to whether consensus had been successfully reached, he replied,

[Hagan] Yes, I don't think the issue is whether they could. With enough effort, reasonable people reach consensus. I think they can and do. The question is what do they lose? Are they unable to address issues that we have to address if we're going to move forward as a society?

When invited to provide an example of the kind of issues that would be omitted through this process of consensus-formation, he continued,

[Hagan] Sure. A broadly historical view of race relations in America and their impact on crime and justice. ... Often what's characterized as irrelevant is the most important, so when we think about deep root causes of problems in society, including race relations in America on crime and justice... I think the Kerner Commission went deep on race and justice in a way that may not be possible thus far for the NAS, but the jury is still out.

The partitioning of knowledge that interviewees described also pointed to a second model of consensus-formation called *consensus-as-deference*. In some instances, a particular panelist's expertise overrode the other consensus-forming process that was supposed to be the CSR's governing principle. The result is a successful consensus, "but on many issues, it's really, I shouldn't say least common denominator, but it's really written by a sub-group and sometimes an individual." [Reuter] The expertise in question may not have been isolated to scholarly expertise—practitioners may in some instances have been the operative spokesperson for what would ultimately become the CSR's published voice—but deference serves as the key mechanism through which panelists delegate away tasks over which they claim to lack expertise.

Consensus-as-deference undermines the NAS' stress on the depersonalized authorship of its reports. Following Lumet's (1957) tropes, deference to a charismatic Juror Number Eight or a defiant and obstinate Juror Number Three can delay the production of a CSR's verdict and accordingly elevates the importance of which specific experts are impaneled for each CSR. Many spoke about the NAS's commitment to achieving methodological and disciplinary balance among the panelists [Feeley; Hagan; Nagin]. But the NAS's committed sensitivity to methodological or disciplinary balance in the panel's composition did not transfer to other dimensions of difference, which earned the NAS a reputation as being "a lot less sensitive to political and ideological balance." [Reuter] Indeed, the interviewees were divided about how panelists had been selected. Some emphasized the panelists' subject-matter expertise as a qualification, but others voiced the NAS's commitment to a panel having been "deliberately composed of scholars who in fact had never published on the subject" so as to preempt the appearance of the panel's ideological predisposition:

[Cook] That was partly my suggestion. I thought [firearm violence] was such a loaded issue, or set of issues, politically, that it would be good to have a group of people that really would appear to be uncommitted going into it.

In sum, interviewees expressed ambivalence or tepid positivity about the force of consensus that inductive CSRs generated. Although deliberations included a wealth of "pushing and tugging" [Cook] among panelists and between the panel and the NAS staff, interviewees also described the centrality of some panelists' "entrepreneurialism" that either triggered the report's production [Cook; Feeley; Nagin]; or in some cases quelled disagreements during the writing process [Cook; Hagan; Reuter]. As a result, interviewees reported that inductive CSRs had experienced an impact on the field from one that "once upon a time, thirty or forty years ago would have been given immense deference... and it would have been unassailable" [Feeley] to one that today vests "landmark documents" of the kind that package "less original" statements with the prestige of "a consulting company with a fancy address" [Reuter] and that hence "aren't widely read" [Hagan].

Abductive reports, like inductive ones, were similarly characterized by consensus-as-deference. However, they were also characterized by a ‘thin’ form of consensus called *consensus-as-product*, for which the mere fact of the report’s completion alone attested to the successful consensus that had been formed during its production.

In abductive reports, deference produced much stronger reports, and interviewees generally attested to much greater confidence in the report’s contribution than when interviewees spoke about inductive reports. Like inductive reports, some abductive ones shared the deferential property that “the final product more reflected a range of chapters by specialists and set in different areas” thanks to “faith in the people” who became responsible for other section of the CSR [Ratcliffe]. However, interviewees attributed that deference to the pursuit of strong consensus where matters were viewed as susceptible to “objective” and “scientific” analysis, and weak where matters were viewed as “subjective” and “unscientific” (race being a commonly cited example; [Ratcliffe]).

A common approach was thus to construe the statement of task as partitioned into “scientific” and “nonscientific” aspects. For example, interviewees at the outset might have understood the task scientifically, and in such instances disagreement was narrowly methodological [MacCoun]. In other instances, interviewees described the statement of task as encompassing both “objective” and “scientific” matters and “subjective” and “non-scientific” ones alike. There, deference figured most prominently. In the 2004 and 2018 policing CSRs, for example, interviewees voiced contest over the scope of the statement of task (most notably, how centrally studies of fairness and procedural legitimacy deserved to figure alongside those of its effectiveness; [Meares; Sherman]). In both reports, deference became the preferred mechanism for securing consensus.

In the *consensus-as-product* model, the mere fact of the report’s completion alone attested to the successful consensus that had been formed in its production. When I inquired with one interviewee whether the panels to which they had contributed succeeded in reaching consensus, he flatly responded that consensus had been a matter of fact of the report’s final publication, and was indeed its defining feature—“they have to be. It’s almost [as though] they have to be consensus, because everybody on the panel has to sign off on the report.” [Nagin] More properly, interviewees who attested to the success of the consensus process by dint of its *product* referred more openly to the idiosyncrasy and narrowness of the panel itself as opposed to the breadth of the findings. Thus, two further interviewees who shared Nagin’s belief in the consensus-as-product model were quick to note that consensus represented both the institutional aspiration and a raw indication that the product had been completed [Feeley; Lauritsen].

However, costs attached to the consensus-as-product model. Most notably, by attesting to the successful formation of consensus purely as a matter of fact inferred from the project’s completion alone, consensus-as-product traded the imprimatur of a ‘job well done’ with the limited value of a CSR whose consensus was anemically narrow. For example, as one interviewee attested to the CSR’s success in forming consensus, he underscored that “at best you could say it’s a consensus *of this particular committee*.” [MacCoun] Another interviewee described in further detail how consensus-as-product tended to generate only the most conservative findings at the bottom “rung” of a metaphorical “ladder” of research value and “papered over the differences” by advocating only that “more research is needed.” [Cook; *see also* Meares; MacCoun; Ratcliffe]

The limited scope of the consensus-as-product model threatened the breadth and depth of a CSR’s findings. For example, one interviewee explained that the 2001 CSR on *Informing*

America's Policy on Illegal Drugs stirred sufficient upset among addiction researchers that the journal *Addiction* published a raft of commentary expressing scholarly displeasure at the report's conclusions [MacCoun]. Field-wide reaction extending the CSR's conclusions may be a function of budgetary misallocation that results in too few funds devoted to dissemination [Reuter]. But the field's mobilization in reaction to the report's findings illustrated that the CSR failed to serve as the scholarly field's final word that the NAS insists it serves. By implication, even the model of consensus-as-product undermines the NAS's advertisement that the consensus it constitutes in a CSR represents a scholarly field.

However, interviewees generally spoke most optimistically about the value of abductive CSRs. Although the 2004 report on *Fairness and Effectiveness in Policing* appeared to have garnered relatively little attention [Sherman], abductive CSRs do have the capacity to move a scholarly and political agenda. For example, one interviewee described how the 2018 report on *Proactive Policing* reflected a culmination of efforts first given significant voice in *Fairness and Effectiveness* that positioned procedural justice alongside crime reduction as a twin imperative of the police function was the product of "a fight" that "we won." [Meares]

[Koehler] How do you think you won?

[Meares] The evidence that we won is that the President's Task Force is the report to which most progressive policing agencies refer, number one. They don't take, they don't say that effectiveness doesn't matter, but this new thing is definitely the lodestar...

Why else do I think we've won? Well, when Tom Tyler and I presented these ideas [to the COPS office] the then-Head of Research of the IACP was like 'This is bullshit, I'm leaving,' and then six years later, at that same meeting, it was the procedural justice show... How did we do it? I don't know, I mean the theory is compelling.

In sum, when speaking about abductive reports, interviewees expressed the same ambivalence that had been shared in reference to the strength of inductive reports but this ranged to a much more optimistic attitude about the capacity of abductive reports to contribute to the policy stage. That optimism, however, was tempered by the construction of the object of criminological study as narrowly scientific.

All three different types of consensus formation were manifest in the production of reflexive reports. Unlike inductive and abductive reports, one interviewee shared that the function of the reflexive report to which he had contributed was more internally referential to the field than had been voiced for other CSRs, and described a much more muted backstage contestation over reaching criminological consensus.

[Lipsey] I don't think anybody on [*Improving Evaluation of Anticrime Programs* (2005)] thought we were settling any issue in any final way. The word guidance is salient—we were giving guidance to the field, but [the report] definitely had a proactive thrust. I mean, the idea was to shape and to provide guidance for shaping other research.

Thus, although interviewees attested to strong consensus among all the reflexive CSRs, they attributed this to a modest scope in the panel's statement of task. The preservation of "high quality research" itself came to be viewed as the CSR's policy objective [Lipsey; *see also* Lauritsen], even

when the statement of task allowed panelists to make recommendations about administrative appointments it was viewed as “going out on a limb” beyond the panelists’ expertise [Lauritsen].

Trepidation about the panel’s contribution to policy was more pronounced among reflexive CSRs than for inductive or abductive reports. In one instance, a CSR’s panelists engineered an additional publication effort to complement the CSR itself. The panelists who composed the 2005 report on *Improving the Evaluation of Anticrime Programs* collectively authored a 2006 special issue of the *Journal of Experimental Criminology* to repackage the same findings for a more targeted audience [Lipsey]. The differences between the CSR and the journal special issue recast the dramaturgy of scientific credibility: unlike the CSR, whose formal authorship is *on behalf of the NAS* and therefore stands as the NAS’s official position, the special issue was instead authored by the named co-panelists. The frontispiece bore none of the fanfare of Lincolnian science advice. There was no accompanying conference to announce the special issue’s publication, as had been the case for the foregoing CSR. In short, the original 2005 report had succeeded in the formal sense of producing the report with which it was tasked; yet even so, it triggered additional action that reproduced the same findings in a different venue, with a different imprimatur, and with a different byline. By implication, the consensus that panelists might constitute in producing a CSR’s ‘public’ may nonetheless be incomplete or insufficient to project the findings on the desired stage.

DISCUSSION

Studying the frontstage and backstage production of CSRs sheds light on four features of the construction of criminological facts. First, CSRs signal that criminology has valuable insights to offer the policy world. Unlike the ‘fracture’ that characterized criminological knowledge toward the late-1960s and early-1970s, CSRs instead project outward an accepted way of knowing about crime that the panel managed to forge through consensus. The drive to reach such consensus attests both to redoubled efforts to produce authoritative policy-informing criminological claims and to marshaling the resource capacities to give those claims institutional heft.

Second, consensus takes different forms and is deployed for different ends. In some instances, the consensus is a ‘thin’ agreement on modest conclusions that are the product of minimal deliberation. These forms of consensus are especially common across inductive reports, which borrow the NAS’s imprimatur to lend legitimacy to wisdoms that have already been firmly established. In other instances, the consensus is a ‘thick’ agreement on more ambitious conclusions that are the product of extensive deliberation and contestation. Consensus taking this latter shape are more common across abductive and reflexive reports, which intervene to settle a political or scientific dispute.

Third, consensus varies in the extent to which it succeeds in forestalling critique. In significant part, this is attributable to criminological consensus’s differences in substance and form. Where interviewees shared that consensus had been easily bridled, that ease was attributed either to an assessment that the evidence the panel assembled pointed in only one direction, or that the composition of the panelists were harmoniously inclined toward the same conclusion. On the other hand, where consensus had been procured with more challenge, that challenge was attributed to a dispute over the evidentiary threshold at which the panel could justify issuing a policy-informing claim. In the former instance, critique was rare; in the latter, the report endured significantly more ferment. At no point, however, was dissent so pronounced as to contest the foundations of a CSR’s enterprise: where contest arose, it was either narrowly methodological in ways that bore little on the report’s conclusions [*e.g.*, MacCoun], or it was swiftly suppressed in the report’s backstage

[*e.g.*, Reuter; Sherman; Meares], or in its frontstage [*e.g.*, NAS 2005: Appendix A]. As a result, CSRs were able to preserve their performance of “a powerful narrative of credibility.” (Hilgartner 2000:54)

Fourth, CSRs display the settled rules with which criminologists make policy-informing claims. Just as the reports displayed little dissent in the substance of their findings, so too they neatly smoothed over epistemic fissures between positivist, progressive, and postmodern approaches to criminological knowledge production. Interviewees varied in their commitment to the value-free ideal, to the propriety of science advice in the policy sphere, to objectivity’s centrality to scientific progress, and to the evidentiary threshold of certainty upon which policy could defensibly rest. But those varying commitments never compromised the production of policy-informing criminological claims. Rather, variations in the interviewees’ commitments along each of these dimensions was ordered out of the final report—sometimes by partitioning the authorship of the report into sections (*e.g.*, consensus-as-deference), other times by minimizing the report’s argumentative thrust (*e.g.*, consensus-as-dilution).

The existence of the CLJ attests to the awareness of a late-1960s need to harness the legitimating force of the NAS to the production of criminological knowledge as an “imprimatur” [Nagin] for policy-informing criminological claims. The practical legitimacy (Suchman 1995; *see also* Abbott 2010) of shoring up a discipline’s identity in this way casts a shadow over the earlier disenchantments with criminologists’ capacity to know something useful about crime and justice that might grip policymakers’ attention. To that end, [Feeley]’s description of the CLJ’s early establishment speaks both to the demand to settle pressing matters of criminological dispute—such as the robustness and validity of Ehrlich’s observed finding of capital punishment’s deterrent effectiveness—and to the field-level energy to appraise the value of the newly-established National Institute of Law Enforcement and Criminal Justice housed within the LEAA.

Early CSRs were more predominantly inductive and intervened little to settle technical or substantive disputes. In fact, the overall pattern in Figure 3.1 suggests that CSRs as a whole were not commonplace until the 21st century, after which point the CLJ blossomed. The modest uses for which scientific consensus was deployed in inductive reports speaks to the same concerns of practical legitimacy that may have motivated criminology’s turn to the NAS in the first instance: what was needed was not so much a settlement of a methodological or programmatic dispute so much as a canonizing imprimatur on an already-established criminological wisdom—such as the fruitfulness of, say, developmental life course criminology and ‘criminal career’ research (NAS 1986a; 1986b).

CSRs thus represent a reversal of the epistemic disarray in which criminology found itself by the mid-1970s. As a precondition for producing policy-informing scientific claims, the research programs that were undergoing a process of ‘crystallization’ I observed in Chapter One were replaced with mutually agreed-upon norms about what criminologists knew, and how they came to know it. This then gave rise to the periods I have referred to as criminology’s ‘stratification’ from 1965 to 1995, and its subsequent ‘institutionalization’ from 1995 to the present. The recent prolificacy of both abductive and then also reflexive reports commenced once criminology’s stratification had drawn to a close and its institutionalization was well underway. The shared aspiration of both abductive and reflexive reports, namely to point in the direction that either policy or scientific research ought to take, presupposes that criminological questions of sufficient import lingered with the kind of urgency that demanded NAS intervention. This implicates a thirst for criminological authority to answer pressing policy-informing questions, which in turn presupposes

that criminology had repaired the fractures of the previous generation. The CLJ's shifting focus from inductive to abductive and reflexive reports therefore tracks the interviewees' assessments that criminology, like some other social sciences, had restored its centrality to providing an evidence-base for policy guidance (Haskins 2018; Morrell et al. 2015; Pielke & Klein 2010; *but see* Desch 2019).

The ascendancy of CSRs is joined at the hip with the emergence of evidence-based crime and justice research. Both speak to the rising centrality and prominence of rules that govern how to make persuasive policy-informing criminological claims. The accompanying stratification and institutionalization of criminological knowledge illustrates one important mechanism of Kuhnian (1962) "normal science," whereby knowledge is sorted and ordered into the relevant and the irrelevant. That mechanism, and in particular its enforcement, should be of special criminological interest. The scientific consensus at CSRs' core assigns authority by quieting suspicion about possible avenues of criminological dispute. Scientific consensus, like its political counterpart (Levin 2018), is thus a mechanism of enforcing discursive modes. For example, a consensus in favor of decarceration (*e.g.*, NAS 2014) appeals to a conflict-avoidant political stage that worries about the backlash against reforms that over-reach beyond the voters' horizons of preference. At the same time, consensus sets the limited scope of options within which viable and reasonable policy reforms might happen without disrupting an internal order.

By sorting acceptable and unacceptable ways of knowing about crime and justice, CSRs thus *stabilize* criminological knowledge. CSRs arise at the culmination of a long process of ordering dissent out, either during the backstage formation of consensus, or in the frontstage resources that can be marshaled to delegitimize disputes. Goodman, Page & Phelps (2017: 13) have argued that consensus in the penal field is axiomatically "illusory." Shifting from the study of criminological knowledge to criminal justice policy, they contend that struggle is axiomatically the "motor" that "produces" penal change, and it is accordingly where one must first turn to study how penal development unfolds. In the "agonistic perspective" they advocate, it is *struggle*, not agreement, that serves as the "motor force of criminal justice history" (p.xi) in the form of "conventional political behavior," "subversion," and "disruption" (p.11).

I have argued to the contrary (Koehler 2019), in ways that the CSRs reveal most clearly—both for criminal justice generally, and for criminological knowledge specifically. In particular, Goodman et al.'s notion that consensus is axiomatically illusory and that struggle is therefore constant presupposes some kind of stability, not least over the terms of agreement. Debate between advocates of two penal policies, just as between advocates of two scientific positions, depends on a commitment to how such a debate might proceed. The construction of criminological facts discussed here shows that this means a mutual commitment to the legitimacy of evidence-based criminal justice reform. In turn, this also means a commitment to take some forms of thinking about crime and justice off the table of serious scientific consideration. It means, in short, a process of stratifying and institutionalizing criminology.

There is, after all, consensus that some scientific positions will not get taken seriously in the CSR process. Hagan pointedly remarked about the consensus-formation process that "what is characterized as irrelevant is often the most important." [Hagan] That interviewee gave a forceful condemnation that others [Meares; Ratcliffe] also echoed about how the CSRs' assembly of relevant evidence forecloses important ways of knowing about crime and justice. In this sense, the CSRs must be understood as an ordering mechanism that advances more than just the assignment

of the NAS's legitimacy to a particular conclusion contained within a report. Moreover, CSRs sort the scientific field's acceptable modes of knowing about crime.

Thus, agonism may well explain struggle between proponents of competing interests who presuppose one another's legitimacy to engage in conflict over ideal states of affairs. Dispute may be fierce while still remaining agonistic. Yet there is also a whole field of complementary kinds of conflict—which democratic political theorists call *antagonistic* (Mouffe 2000; 2013)—between proponents of competing positions who treat one another as presumptively *illegitimate*. Placing antagonistic disputes alongside agonistic ones more comprehensively explains not just change, but also stability (Koehler 2019).

This enforcement of the relevant from the irrelevant, and of the viable from the unreasonable, gives texture to the Freudian Bind of criminological knowledge production: on one hand, consensus sets the boundaries between which normal criminological science advances, and it does so by legitimating accepted norms for how to make persuasive policy-informing claims. Dispute is readily apparent and constant—even if, following Lyotard, it is “a horizon” that “is never fully reached.” On the other hand, the key, as the CSRs reveal, is that permissible dispute may not rise to a level that sets the very terms of scientific agreement themselves in dispute. To accomplish this, scientific consensus relies on exclusion mechanisms that take alternative ways of knowing about crime and justice off the table.

The emergence of an ‘evidence-based’ movement in crime and justice research appears in the most authoritative texts that criminologists are capable of producing: Consensus Study Reports, published by the National Academies of Sciences’ Committee on Law and Justice. An examination of how those texts came to be reveals that the consensus at their core stabilizes important tensions between positivist, progressive, and postmodern approaches to scientific knowledge production. That consensus projects outward the impression that criminological knowledge is capable of offering settled answers to important policy questions. Moreover, CSRs lend needed credibility to how a field ‘knows’ about crime and justice, by presenting an air of univocality even in instances where an analysis of the backstage processes of report construction reveals that such consensus was in fact absent. Even fierce disputes are repackaged as trivial, and comfortably packaged as falling within the bounds of agonistic dispute befitting the advancements of ‘normal science.’ Consequently, the facts that CSRs issue present persuasive policy-informing claims on which scientifically supported criminal justice policy may be constructed.

CHAPTER FOUR: RATIONALITIES OF CRIMINAL JUSTICE REFORM

Abstract

Criminal justice reform presupposes both that agencies of state control are susceptible to change, and that the state is best positioned to enact that change. I focus on the institutions of policing and corrections to track both of those implications through the past half-century. I identify two rationalities of criminal justice reform: *administrative reform* and *systems reform*. The administrative rationality, which stemmed from insights in the emergent sociology of criminal justice and legal realism, provided heft for both the due process liberalism and law-and-order conservatism that followed. Administrative theory imagines criminal law as a resource that frontline workers deploy to advance desirable ends; it accordingly emphasizes the discretion that those frontline workers exercise as the proper target of policy intervention. The systems rationality sprang from movements beyond criminology once operations researchers transposed the metaphor of the system to the study of criminal justice; a functionalist endeavor, systems theory imagines criminal law as part of a dynamic self-regulating equilibrium, but for its vulnerability to exogenous shocks that puncture the system's homeostatic integrity. Both rationalities equipped reform efforts with different tools to understand and intervene upon the dislocations that followed once the carceral state's expansion acquired full velocity from the 1970s onward: administration counseled in favor of internalist programmatic refinements, whereas systems counseled that criminal justice should be insulated from external disruptions. Following the history of these two rationalities of reform across policing and corrections illuminates criminology's role in evidence-based criminal justice.

Keywords

Criminal justice reform; reform rationalities; administration; systems; evidence-based movement

For there I picked up on the heather
And there I put inside my breast
A moulted feather, an eagle-feather—
Well, I forget the rest.

—Robert Browning, *Memorabilia*

In Chapter Three, I analyzed the policy-informing criminological claim to historicize criminology’s ‘stratification’ from 1965 to 1995 and subsequent ‘institutionalization’ from 1995 until the present. In this chapter, I situate the emergence of those discursive rules for criminological knowledge claims in the wider penal field they inhabited. I explore how the relationships between criminological thought shaped, spurred, and stalled criminal justice policy.

One insight from Chapter One was that criminological thought splintered both into strata of prestige and into clusters of substantive focus. Hierarchically, criminological knowledge sorted research programs deemed to deserve scholarly attention from those deemed undeserving (*see* Figure 1.4); substantively, criminological knowledge splintered in ways that centralized research on policing and corrections and abandoned the study of criminal courts (*see* Figure 1.5). In this chapter, I therefore concentrate on American policing and corrections from 1965 to the present to show that during the past half-century, criminological science reflected, first, a prevailing skepticism about the state’s capacity to control its citizens, and then, a newfound optimism about the same, by reimagining both the police and the prison. The transition from penal frustration to fervor was overdetermined: ebbs and flows in national crime rates doubtless played a role, but shifts in the imagined functions and capacities of policing and imprisoning were likely to have occurred regardless.

To illustrate the shift in sentiment from frustration with criminal justice to fervor, I describe two overlapping ‘reform rationalities’ that each capture an ontological belief about the role that law, legal institutions, and social control play in American policing and imprisoning. The first reform rationality is an *administrative* approach to criminal justice, which imagines law as a resource to advance desirable ends. Pioneered by Frank Remington’s American Bar Foundation (“ABF”) Study of criminal justice, administrative reform reached its apogee in liberal legalism’s commitment to fairness, equality, and due process. It was steeped in the belief that, when properly deployed, the tools of law and social science could measure, contain, and even redirect frontline criminal justice workers’ discretion. It was through Remington’s stewardship of the ABF Study, and his recruitment of research advisors Lloyd Ohlin and Herman Goldstein, that criminal justice discretion was “discovered” (Walker 1993). It viewed agencies of criminal justice as resources that could be deployed to advance desirable ends, law-and-order reformism. The second reform rationality is a *systems* approach to criminal justice, which commits itself to the premises that criminal justice agencies were intelligible by reference to the same basic laws of operation as were found among the natural and human sciences, that they were broadly functionalist in purpose, and that reform was achievable through homeostatic processes of institutional self-regulation (Mayeux 2018).

From the mid-1960s to the present, efforts to reform criminal justice featured both administrative and systems rationalities. By the late-1980s, evidence-based criminal justice reform had inherited elements of both rationalities, albeit with a much stronger emphasis on systems-thinking than administration. But suffusing either rationality in efforts to reform criminal justice proceeded in different ways and at different paces between police departments and correctional agencies. Structural differences that distinguished locally-governed police departments from state-

governed prisons both helped and hindered reform efforts of different kinds, and as a result evidence-based policing differed in pronounced ways from evidence-based corrections. The local dispersion and paramilitary command-and-control structure of police departments contrasted with the central organization but civically governed structure of correctional agencies. As a result, the path that evidence-based policing took barely resembled the one on which evidence-based corrections trod.

A prodigious literature historicizes the carceral state's postwar expansion (*see* Chapter One). Much of that literature tracks the public's disfavor with the state as an engine of mass incarceration, and complicates some of the neat narratives I portray in this chapter that favor ideal-typical breadth over granular accuracy. For example, some of the ebbs and flows in skepticism surrounding the capacity of criminal justice to control crime display more "agonistic" conflict (Goodman et al. 2016) than the sweeping characterizations contained in this chapter might suggest. In this chapter, however, my ambition is to bring full circle the history of criminological thought from the foregoing three chapters in service of three conclusions. First, I pin the history of subsiding pessimism in the state's capacity to apply social control on one hand to changing favors articulated in criminological science on the other. I show that criminological wisdoms about the police and the prison seeped into cultural zeitgeists about the propriety of public reliance on either institution. Second, I use those cultural zeitgeists as windows into the promises and perils of evidence-based reform movements. Third, I track how the changing favors that criminal justice enjoyed or endured over fifty years dulled the ambitions and narrowed the scope of criminal justice reform—a trajectory I have described elsewhere as one of "penal antagonism" (Koehler 2019).

The literature on criminal justice reform efforts pays close attention to how differences in governance structures between justice agencies impedes or facilitates large-scale shifts in the operations of the police (*e.g.*, Rushin 2017; Sparrow 2016; Vitale 2017), the courts (*e.g.*, Bibas 2012; Feeley & Rubin 2000; Zimring et al. 2001), and corrections (*e.g.*, Lynch 2009; Schoenfeld 2018; Simon 2014). But direct comparison between justice agencies remains difficult because the different charges entrusted to each agency defeat tidy comparison of one technique in one agency with the same technique in the next. Evidence-based reform, on the other hand, speaks more generally to a rationality about criminal justice reform efforts than to a specific technique. Thus, because it is common even across unlike agencies, it allows comparison of like with like. Evidence-based reform therefore presents a useful window into the advantages and disadvantages of different agencies' receptivity to policy-informing criminological claims, at the same time that it provides a concrete application of beliefs about criminal justice capacities writ large.

1960S: RATIONALITIES

Administrative rationality

In the summer of 1956, Frank Remington, then a recently-hired professor of criminal law at the University of Wisconsin, had taken up his role as Director of Field Research for a study sponsored by the American Bar Foundation. Without a clear mandate or research question at its inception, but an overflowing purse courtesy of the Ford Foundation's ambitious program of postwar development, the ABF Study would eventually culminate as the benchmark project to understand the "administration" of criminal justice.

The ABF Study had been commissioned in response to a speech that Justice Robert Jackson delivered in 1953, in which he complained about the underequipped knowledge base concerning

American law enforcement. The ink was yet to dry on Jackson's dissent in *Shaughnessy* that made known his discontent with justice's irregularities in which he canonized the maxim that "procedural fairness and regularity are of the indispensable essence of liberty" (1953: 225). Jackson's request that the ABF survey American criminal justice spurred the Ford Foundation to inject massive funds for a three-state pilot project with the hope of a further nation-wide extension to study the administration of discretion at all decision points in the criminal justice apparatus.

The ABF Study's principal contribution was twofold: first, it dislodged the prevailing belief, dominant since the Wickersham commissions of earlier years (Zimring 2013; see Pound 1923/2018), that the ailments of criminal justice were primarily those of inexpert leadership and material resource constraints. The problems of policing and of imprisoning, so the dominant arguments went, were soluble with smarter personnel and more funding; case attrition, plea bargaining, street-side justice, and constitutional technicalities contaminated the purity of what ought to have been an efficient machine capable of processing *all* crime into *precise* and *correctly calibrated* punishment (Bibas 2012). The formalism that permeated Wickersham imagined any crime left without a conviction as cause to complain.

Not so for the contributors to the ABF Study. Instead, their second principal contribution was to replace the dominant belief with a new one that imagined law not as a perfectible organ for mechanically processing 'crime' through respective 'decision points'; instead, Remington, Ohlin, and Goldstein viewed discretion of all kinds as resources with which to advance desirable ends (Ohlin & Remington 1993). It was Remington's observation, made a few years earlier while conducting an ethnography of the Detroit criminal court, that prosecutors leveraged the (unexercised) threat of criminal prosecution to pressure businesspeople to retribute victims in the civil court down the corridor. Remington recognized that what would have appalled the progressive Wickershamite in the misuse of deliberately empty sanctions from one (criminal) legal code to enforce an end sought in another (civil) one was instead a standard exercise of justice administration. Law's exercise might well be hidden, so the ABF Study taught, but it was nonetheless still vigorously in action. That sensitivity to law's versatility—the bedrock of what would later develop into the legal realism movement at Wisconsin (Feeley 2018)—would serve as one of the ABF Study's guiding contributions to the administrative rationality in criminal justice.

Unlike the Wickersham Commissions from which the ABF Study inherited its dominant belief-set about criminal justice formalism, the ABF Study's field research in police departments, in courtrooms, and in the community injected the same three insights informed by the sociology of criminal justice into the administrative rationality that followed (Walker 1993): first, the criminal law was versatile in its application. Withholding threats could be as much at the core of the criminal justice function as their exercise. Second, the administration of criminal justice frequently proceeded untethered from the normative principles of criminal law. Order maintenance could gesture toward, without ever needing to exercise, a threatened sanction. This, too, upended the Vollmerian mantra (Vollmer & Parker 1937) that sanctions make codes into rules and that the legitimacy of those codes inheres in the extent to which those codes spring from the penal justifications that give them force. Third, the administration of criminal justice was a collection of decisions fraught with the discretion of frontline workers. Discretion was not to be celebrated as such, but without an appreciation of the role that discretion played in criminal justice administration, reform efforts would stall.

Developments outside of criminological circles affirmed the ABF Study's lesson that discretion was to be the lodestar of criminal justice reform efforts for the foreseeable future. By

January of 1967, shortly after Lyndon Johnson's State of the Union Speech called the nation to arms in an "all-out effort to combat crime" (Johnson 1967), the stage was set for the 1968 presidential election. Unlike the fraught contests of 1964 steeped in apoplectic Goldwaterisms, Johnson had made clear that he had no further designs for presidential office. Only Hubert Humphrey and Eugene McCarthy had announced their—altogether meager—bids for the presidency. It would still take more than a year before Bobby Kennedy would announce his bid in March of 1968, by which point he was still stepping into what appeared as an uncontested track.

By early 1967, Kennedy's own political profile had been characterized by his elevation of the Attorney General's office from obscure hinterland to forward base in the executive's Great Society interventionism. Kennedy was a figurehead in combating public disaffection with the executive's capacity to confront public problems (Simon 2007). Within a short period either side of Kennedy's tenure, public regard had shifted from viewing America as a Nazi-thumping, moon-visiting, polio-eradicating paragon of governmental competence and humanity-promoting virtue to a Vietnam-losing, inequality-fomenting, failure racked by wiretapping scandals, urban unrest, and oil crises.

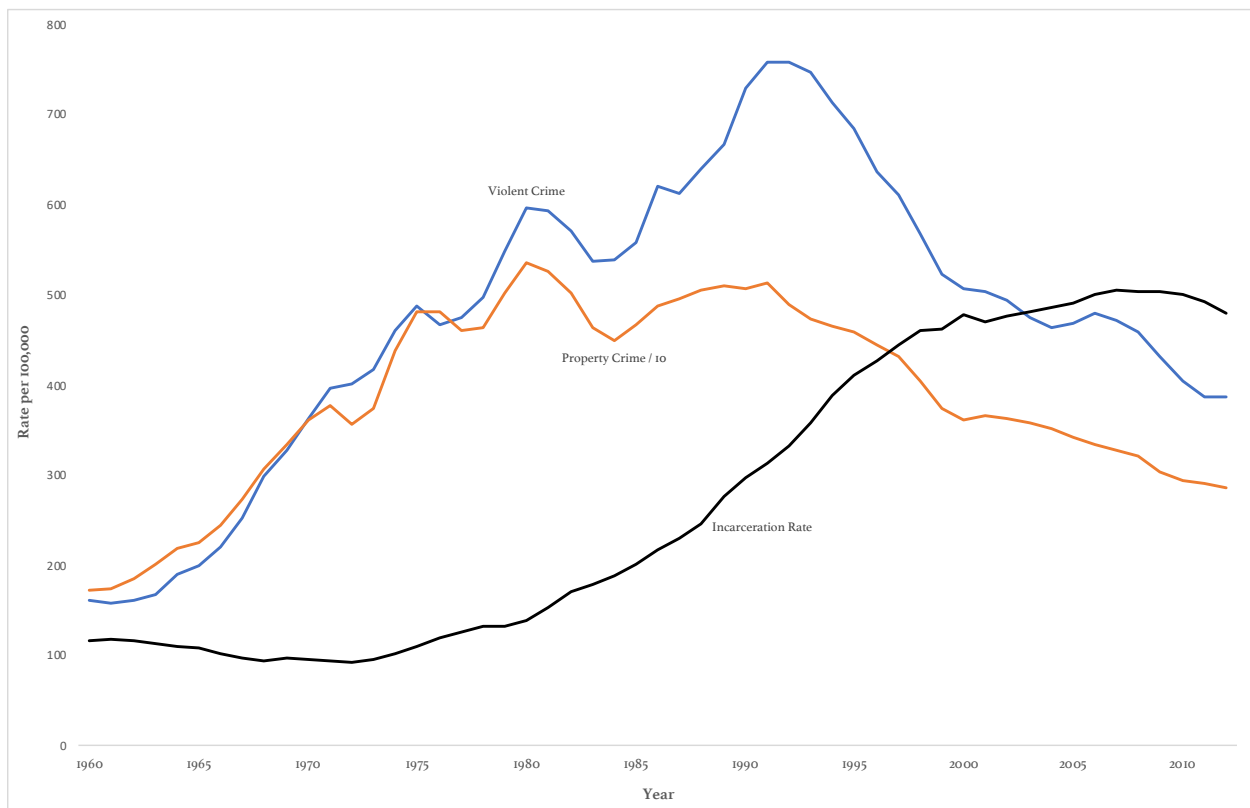
The Johnsonian crime agenda fell squarely in between that trajectory from public pride to pathos, and in considerable part served as one of its cornerstones. As preparation for Kennedy's presidential candidacy, he shared his proposed extensions to the Johnson administration's crime agenda in a speech he delivered to Columbia Law School within a fortnight of the 1967 State of the Union address. In that speech entitled *Crime in the Cities*, Kennedy rehearsed the same liberal reformisms that lent Johnson's Great Society interventionism such lingering favor. "For the 70 percent of Americans who live in cities," he worried without supporting citation, "freedom is threatened most of all by the incidence of crime." (Kennedy 1967: 142) But in the shadow of Johnson's similarly crime-focused oratory, Kennedy's reflections presaged two ironies of the administrative rationality in criminal justice reform for the half-century yet to come.

First, Kennedy's proposed approach to solving crime's problems coupled Great Society interventionism, which genuflected in the direction of targeting crime's root causes, with a programmatic platform that eschewed public welfare altogether in favor of policies designed to enhance state force. Amid florid sermons to strengthen community values and the promise of providing that "all of our young people are adequately educated and can obtain jobs," or the spiritual rehabilitation of "public attitudes, of our values, and our support for law enforcement," (Kennedy 1967: 153) the contemporary approach to administrative reform was reflexively material: investment was needed in technocratic solutions to retrieve the "true efficiency" of frontline agents of criminal justice, albeit without allowing efficiency to be "lost or converted into a substitute for fairness." (p.147)

Kennedy's embodiment of late-60s due process liberalism set an unmistakably administrative agenda for criminal justice reform. Following on the heels of *Mapp* (1961), *Gideon* (1963), and *Miranda* (1966), the police of the modern age were to embody Vollmer's request voiced a half-century earlier for a 'smart cop' capable of navigating the indeterminacies of street justice, albeit with an important wrinkle informed by the ABF Study's insights from the realist sociology of criminal justice. The mutual distaste between the liberal intelligentsia and the police had thrown policing's capacity to serve community ends in disrepute (Lipset 1969), and growing appreciation of policing's complexity thus reinforced Johnson's request—voiced only three years earlier in the War on Crime's earliest days—for a cadre of better-trained law enforcement brigade steeped in the "best techniques of modern science." (Johnson 1964)

Administrative modernization permeated calls for prison reform, too, in calls for better-trained correctional staff, requests to buttress custodial programming, and post-release employment services that in their modesty gestured toward the rehabilitative ideal's "decline" (Allen 1981). The LEAA served all these ends in name, and cemented the view that the federal government could play *some* role in criminal justice reform, but skepticism surrounding its effectiveness inspired caution about what that role ought to be (Feeley & Sarat 1980). This, then, was the first irony of Great Society liberalism's mark on administrative criminal justice reform: call first for large-scale reorganization, but then respond to dissatisfaction with the failures of those reorganizations by advocating programmatic reforms to reinforce the carceral infrastructure (Hinton 2016).

Figure 4.1: RATES OF VIOLENT CRIME, PROPERTY CRIME, AND INCARCERATION IN THE UNITED STATES, 1960–2014. Violent and property crime rates appear as blue and orange lines, respectively, and the incarceration rate appears in black. The property crime rate has been divided by ten for ease of legibility. *Source:* FBI Uniform Crime Reports; BJS Corrections Statistical Analysis Tool (2019).



The second irony of administrative criminal justice reform was that Kennedy's worries about crime as the paramount threat to American freedom were not altogether unfounded. Postwar investment in the criminal justice system had steadied, and national concern had not let this fact go unnoticed. For decades, American incarceration had unwaveringly hovered around a rate of 110 per 100,000, with a "stable" standard deviation of about 9 (Blumstein & Cohen 1973). Compared to other Western civilizations, American carceral appetites were high but by no means offensively so relative to other industrialized Western nations (Zimring 2019: Ch.1). Yet Figure 4.1 shows that crime, on the other hand, evinced a volatility that threw incarceration's stability into stark relief. FBI Uniform Crime Reports showed that both violent and property crime rates climbed precipitously throughout the past decade, and would climb more precipitously still until

the early-1990s. By 1970, nationwide violent crime had almost doubled in a single decade (from 161 to 364 per 100,000), and property crime fared similarly (from ~1,700 to ~3,600 per 100,000; the data have been divided by ten to ease legibility on the same axis).

The sharp increase in both violent and property crime throughout the 1960s and early 1970s during a time of penal “stability” strained the administrative rationality’s lassitude toward the discretionary attrition of crimes as they progressed through the organs of criminal justice. When Kennedy intoned that crime was *the* problem of the day, he was not only echoing and amplifying Johnsonian disquiet about crime; he was also acting as a proponent of the administrative rationality nonetheless vocalizing growing skepticism about the limits of that stance toward criminal justice reform.

The Johnsonian crime agenda that Kennedy extended was not altogether fruitless. But the advent of systems thinking would soon address the two ironies that administrative criminal justice reform left behind—one that queried the government’s role in criminal justice reform despite an inability to identify appropriately targeted interventions; and the other a belief in the stability of penal appetites in the face of a crime rate that was acquiring full velocity.

Systems rationality

The administrative rationality’s footprint loomed large over criminal justice reform efforts until at least the late-1960s, nowhere more so than in the pages of *The Challenge of Crime in a Free Society* (1967a). *Challenge* was unlike a plethora of other high-profile contemporary commissions with criminal-justice-adjacent charges that included the American Law Institute’s *Model Code of Pre-Arrest Procedure* in 1963 and completed in 1975, the National Advisory Commission on Civil Disorders produced its report on urban unrest in 1973, and the American Bar Association published its *Standards for Regulating the Police Function* in 1974 begun a decade earlier. Johnson had also commissioned a forgotten smaller profile *Report on Crime in the District of Columbia* that antedated *Challenge* by a year (1966). *The Challenge of Crime in a Free Society* not only represented the culmination of years of expert panels, high-profile meetings, white papers, and open solicitations since Johnson’s initial commission in 1965; it thus also entered a policy fray thirsty for clear answers about criminal justice’s role, capacity, and susceptibility for reform.

The commission produced an array of field studies on policing (1967b), courts (1967c), corrections (1967d), and juvenile delinquency (1967e), and it synthesized those findings and the policy recommendations that followed into a report published as a widely-circulated paperback (1967a). A final task force report (1967f) on Science and Technology was appended to the commission’s assembled field studies and integrated into the summary report (1967a). Bearing a strikingly different character than the other Task Force Reports or their associated summary chapters, *Science and Technology* was both a methodological and substantive departure from the administrative rationality that undergirded the rest of the Commission’s work. It injected into the understanding of criminal justice a technocratic managerialism honed in the classrooms of operations research and systems analysis rather than those of sociology, law, and political science.

Replete with policy recommendations borne of the ABF Study’s insights, the final report sought to administer the discretion that was replete throughout criminal justice with a blossoming industry of written policies that would enable bureaucrats to monitor, evaluate, and adjust the decision points of frontline agents of criminal justice. Inadequate diversity in police personnel was soluble with training; citizen complaints were soluble with the establishment of review boards; unconstitutional custodial conditions were soluble with grievance protocols; and above all, the

priorities of criminal justice were to be codified in departmental guidelines and mission statements. *The Challenge of Crime in a Free Society* was not only a paean to Democratic liberalism, it was also joined to a movement to take frontline worker discretion seriously as a point of a more visible federal intervention in criminal justice reform.

However, the report's closest resemblance to the ABF Study was not its substance but its form. *Challenge* disaggregated criminal justice into four modules—police, prosecution, courts, and corrections—that could be intelligibly integrated into a whole. Unlike the dominant motif of previous efforts to imagine criminal justice since Wickersham, *Challenge* imagined those discrete modules in close conversation with one another, with inputs at one end affecting the processing of outputs at another. This metaphorical conversation between constituent modules integrated into a systemic whole was the first of the features of *systems*-thinking that would go on to be the report's greatest triumph. By placing each module in conversation with one another, systems-thinking elevated postwar functionalism to its policymaking ideal. The scientist need but observe an agency to know its purpose, and one need but observe how surrounding agencies respond to the behaviors of the one to know how it is situated in relation to the other (Heyck 2015).

The second crucial contribution of systems thinking followed from the first. Functionalism's emphasis on imagining criminal justice agencies in conversation with one another also implicated a maxim of reform efforts that the system, properly construed, was homeostatic. If each agency reacted to one another, then the laws of dynamic equilibrium dictated that exogenous shocks could reasonably be contained within the system's self-regulating order (Isaac 2012). It was a simplifying and abstracting maneuver, but that was the point. The acme of that simplification and abstraction appeared in the report's earliest pages, when the first effort to visualize 'the' 'criminal justice' 'system' spread across a double-paged centerfold (see Figure 4.2).

Visualizations simplify and abstract; therein lies both their virtue and their vice (Healy & Moody 2014). When visualizations transpose complex information from one medium to another, they do so by inscribing a metaphor where one had previously been absent. And visualizing the criminal justice system in *The Challenge of Crime in a Free Society* was a powerful metaphor indeed: the muscular federal intervention that the report advocated resonated with a contemporary zeitgeist more propitious toward centralized reform efforts and the 'stable' incarceration rate harmonized with the premise of homeostasis in systems-based thinking (Mayeux 2018).

In one sense, the system visualized in *Challenge*'s opening pages affirmed one of the ABF Study's insights about the interplay between criminal justice agencies. A series of pipes transmitted the 'intake' at one end of the 'system' through its associated decision points, with attrition at each step signified by the diminishing diameter of pipes connecting one decision point with the next. Cases fell 'out of system' at the farthest end of the system's process, or along the way they were 'unsolved or not arrested' or 'dropped or dismissed' or 'released' depending on the preceding completed decision stage.

But the system also visualized criminal justice in a way that departed significantly from the administrative rationality. Unlike the ABF Study's lesson that case attrition was the product of frontline agents strategically and creatively deploying law as a resource to advance desirable ends, the systems approach imagined the criminal justice system as serving a function of perfecting crime's movement from one stage of criminal justice's discrete modules to the next. This was much more akin to the Wickersham ontology that Remingtonian administration sought to replace. The intake that kickstarted criminal justice processing was crime itself—whether “observed by” or “reported to” the police (p.8–9). Crime was found in the world, or if left unfound, it was measurable as an inefficiency to be monitored and reacted as the system corrected and perfected itself. Thus, the systems rationality did not conceptualize crime, as did the administrative rationality, as the product of discretionary judgments and interpretive games.

The criminal justice system's visualization therefore offered much less clarity about what crime was than what it was not. Crime and the criminal law were not resources susceptible to manipulation by frontline agents. Crime was manageable through scientifically-informed management of the process. The systems analysis toolkit borrowed much from the formalism that had underpinned Wickersham, and from which the rest of the commission was a departure. Skirting the ABF Study's insights that case attrition was a resource that frontline workers deployed to advance desirable ends, systems analysis instead reconstrued each decision point from one criminal justice actor to the next as vulnerable to ‘leaks’ to be plugged. The plumbing that flushed those widgets from one end of criminal justice to the other had leaks, and fixing those leaks produced a cleaner, more efficient system.

Nowhere was the systems rationality more conspicuous than in the *Task Force Report: Science and Technology*. Authored by Alfred Blumstein, by then already an accomplished operations researcher hired from the Institute for Defense Analyses, Blumstein openly professed a professional mission to align criminology and criminal justice reform thinking with systems analysis (Blumstein 2018). The conclusions that follow from the *Task Force Report* that he steered displayed the fullness of the systems rationality's reform repertoire: clearance rates were coterminous with error rates; the dark figure of unmeasured crime commission was the same; as were inconsistent sentences, and—outside a narrowly tolerable margin—overturned convictions on appeal were similarly an indication of lower court error. Although that much was explicit in the report, what was implicit would be the governing principle for the evidence-based age to follow: because the process began with crime and ended with crime, forces external to that process that shaped or constituted crime were parceled out of the process. Thus, unlike the administrative rationality's enthusiasm to identify and curb toxic influences on criminal justice discretion such as racism or misaligned political incentives (Bittner 1970; Skolnick 1966; Wilson 1968), the systems rationality instead construed whatever had been circumscribed as ‘external’ to the system as beyond the scope of criminal justice reform.

The systems rationality was thus a departure from the administrative rationality in both a modest and an ambitious sense. Modestly, systems produced a reform rationality that was numb to discretionary behaviors responsible for alternative inputs that registered as ‘crime,’ such as police misconduct resulting in arrest, fabricated testimony, wrongful conviction, and the like. Only what the systems-visualization could *make visual* was intelligible: crime was either ‘observed by’ or ‘reported to’ the police, and that was the only intake that triggered the system process. Ambitiously, systems ordered whole domains of criminological attention out of the reform conversation. Systems thinking rigidly circumscribed what ‘counted’ as part of the system and

what did not. Crime went in, dispositions and appeals and convicts and punished offenders came out. Nowhere in the metaphor was there space for the political process that produces a penal code. Nowhere in the metaphor was there consideration of larger structural forces—*systemic* ones, even—like race or exclusion or power (Mayeux 2018).

The Challenge of Crime in a Free Society thus juxtaposed two very different reform rationalities alongside one another: the administrative rationality at the report's front-end contrasted with the systems rationality at its back-end, and each one implicated different commitments for criminal justice reform. The years immediately to follow would reveal which was to be the governing template.

1970S: JEREMIAD

The Challenge of Crime in a Free Society enjoyed an auspicious early reception. Its principal recommendations formed the template for the Omnibus Crime Control and Safe Streets Act written the following year, of which the LEAA's establishment was one of its chief contributions. Contained within the LEAA was the National Institute of Law Enforcement and Criminal Justice, the nation's first centralized authority for disbursing funds to research the operations of criminal justice until the LEAA's dissolution and replacement with the NIJ. The overflowing monies for criminal justice science that the LEAA distributed exceeded the spending appetites of even the infrastructure of criminology and criminal justice departments at state colleges. Following the path dependencies first trodden during *Challenge's* composition, research funds for technical projects found a new home among the burgeoning industry of research institutes housed outside of universities (Cronin et al. 1981). One branch of that research funding included the beltway research institutes populated by Blumstein's fellow operations researchers responsible for strengthening the systems rationality in criminal justice research and beyond; the other branch included the Ford Foundation, which after Bobby Kennedy's assassination was populated by his technocratically-minded campaign staff—most notably McGeorge Bundy (Berman 1983).

McGeorge Bundy soon established the Police Foundation in 1970 as the nation's most visible figurehead in criminal justice research with an initial \$30 million grant to fund five years' worth of experiments and pilot programs (Ford Foundation 1970). The Kansas City Experiment was among its first forays. Begun in 1972, the experiment assigned five patrol beats to a "reactive" condition limiting police presence only to responses to citizen distress calls, another five beats to a "proactive" condition characterized by saturated police presence, and five control condition patrol beats that received ordinary police exposure (Kelling et al. 1974). The experiment gathered a prodigious array of survey, observational, ride-along, and official data to test differences in citizen perception of police presence, effects on reported crime rates, and citizen fear of crime.

The nomenclature of the experimental conditions was a telling insight into the experiment's intellectual and empirical objective. The distinction between "proactive" and "reactive" policing was of a recent provenance, first appearing only a few years earlier in Reiss's *The Police and the Public* (1971). There, Reiss had observed that the lion's share of police work—the "standard model"—was indistinguishable from petty hassle and the maintenance of racist and bourgeois values. Reiss was disappointed that police did not strategically deploy the criminal law as a tool to advance socially desirable ends as proponents of the administrative rationality had hoped they might. He therefore warned that the only solution consistent with liberal principles was to isolate the police function to reactive patrol, responding exclusively to citizen invitations to intervene on behalf of the community's self-defined order.

But at the time the Kansas City Experiment began, there had been no full-scale analysis of the police's basic functions. The administrators who oversaw the experiment on behalf of the Police Foundation sympathized with Reiss's cautions about the problems associated with preventive patrol. They included progressive members of policing's top brass such as Clarence Kelley (then the chief of police in Kansas City before his appointment as director of the FBI) and administratively-minded social scientists such as George Kelling (a seminarian, probation officer, and social work doctoral candidate before his later collaborations with James Q. Wilson upon the latter's appointment as the Police Foundation's chairmanship). All were bound by a shared commitment that the promise of preventive policing's standard model was both empirically overstated and normatively distasteful (Zimring 2007: Ch.2). The Kansas City Experiment's null hypothesis was thus its most chief ambition.

The results exceeded expectations, destructively so. Not only did the patrol beats assigned to the preventive experimental condition report no discernible improvements compared to the patrol beats assigned to the control condition in either citizens' perception of police presence, fear of crime, or crime itself; the same also held true for patrol beats assigned to the reactive experimental condition, too (Kelling et al. 1974). The Kansas City Experiment affirmed the empirical needlessness of preventive patrol's 'standard model' of policing, but in the process it undermined faith in the only other model going. The consequence was that an experiment ostensibly fitted to redirecting police policy from one path to another ultimately shredded the map and left policing without a path to follow of any kind.

The Police Foundation's subsequent high-profile experiments in policing echoed the catastrophic results observed in Kansas City. A follow-up experiment on team policing in Cincinnati proved attested to the difficulties in sustaining complex patrol strategies (Police Foundation 1977); another experiment on the effectiveness of foot patrol in Newark observed null results on crime but modest improvements in citizen fear of crime (Police Foundation 1981); another experiment reported null results for the effect of arrest on preventing shoplifting (1983). The Police Foundation in the 1970s, in advancing its institutional mandate following *The Challenge of Crime in a Free Society*, had instead become synonymous with the hollow promises of policing altogether.

The outlook for corrections was similarly bleak but for the opposite reason. Unlike the Police Foundation's thumb that pressed rather too excitedly down on the scale in favor of a null, the prevailing wisdom in penal science had been characterized by a rehabilitative "ideal" for the better part of a century (Allen 1981; Pifferi 2016; Rothman 2017). That ideal, in turn, had been grounded in a progressive faith both in the state's capacity to advance desirable ends, and in the moral virtues of those ends. It was during the same moments that the Police Foundation's null results inspired skepticism surrounding the police function that the same progressive faith was unraveling in corrections, too.

The rehabilitative ideal was under assault from the right and left alike. The right had voiced concern about government profligacy even before Great Society interventionism had become more than a whisper, and correctional institutions had always seemed a poor investment. The left objected to rehabilitation's unconstrained license to intervene and reshape people under the state's charge in its own image (American Friends Service Committee 1971). An acute source of the left's ire upon growing awareness of horrific prison conditions from San Quentin (Jackson 1970) to Attica (Thompson 2017) was the need to disabuse the public of the medical model's untrammelled authority to recast punishment as 'treatment' (Menninger 1968; *see also* Burgess 1962).

Robert Martinson was one committed liberal who had personally witnessed the horrors of America's carceral machine while he had been confined at Mississippi's infamous Parchman Penitentiary as a Freedom Rider (1962). As part of that experience, he bore an abiding contempt toward the authoritarian impulse to imagine custodial confinement as correctional treatment (Crimmins 2018). He therefore brought a progressive liberal zeal to his acceptance of an invitation by Douglas Lipton to collaborate on Nelson Rockefeller's newly-established Governor's Special Committee on Criminal Offenders. The Committee had been tasked with the mammoth charge of synthesizing all postwar anglophone research on the effectiveness of correctional rehabilitation.

The report's conclusions cemented 1974 as the "vintage year for the null hypothesis." (Zimring 2007: 31) But the team members interpreted the study's findings differently (Crimmins 2018). On one hand, Lipton—the Principal Investigator—cautioned against stumbling into a Type-II error. He warned that null effects do not provide affirmative evidence that intervention does not work; to the contrary, they provide negative evidence on the question of whether the intervention does work. It was a frustratingly shallow methodological argument to make on the back of years of exhaustive data gathering, and Lipton also knew that his interpretation would carry little political resonance, especially compared to Martinson's characteristic flair. He nonetheless imposed a tediously soporific methodological reserve in the 700-page final report that served as the team's official—and wholly unread—product (Lipton et al. 1975). Martinson, on the other hand, published an abbreviated précis under his own name—separate from the team's other members—in the widely-circulated *Public Interest*, where his exaggerated stylistic flourishes were much more welcome (Martinson 1974).

What followed was a jeremiad for the state's capacity to satisfy criminal justice ambitions that *Challenge* had promised (Simon 1993). Apostasy was uniform across both policing and corrections. At the same time that the 'standard model' of policing patrol came under intense scrutiny, so too did progressive hopes for the 'rehabilitative prison.' By the mid-1970s, the pages of *Science* magazine decried the "collapse" of prison rehabilitation (Holden 1975), and Norval Morris (1974) leant on Martinson's article in advocating the abolition of parole and a turn toward "limiting retributivism." A member of Congress complained in the pages of a leading criminology journal to lament that criminologists had fallen silent on important public policy questions (Conyers 1979). The passage of the Juvenile Justice and Delinquency Prevention Act (JJDPA) that same year deinstitutionalized status offenses, forbade the warehousing of youths in the same facilities as adults convicted of a crime, and cast suspicion over the use of custodial confinement for youths as a policy response of first resort.

Many imagine the criminal justice legacies post-*Challenge* as a set of broken promises—a Kuhnian "paradigm shift," no less (Zalman 1987) of justice reform's repudiation of due process liberalism and the administrative rationality that Vorenberg elevated in the report's front-end. Contesting the continuities between America's wars on poverty, then crime, then drugs, then terror and beyond (Hinton 2016; Huq & Muller 2008; Simon 2007; Wacquant 2009), *Challenge* is often cast as a damp squib of policy-making that was ill-timed for the scientific revanchism of the Police Foundation experiments and Martinson. Retrospectives commonly bemoan that *Challenge*'s key policy recommendations—especially those with more of a progressive flavor—fell to the side as mass incarceration accelerated (Reiss 1994; Skogan 2018; Spohn 2018; Tonry 2018). Like Bobby Kennedy's quixotic calls for a spiritual reckoning for America's soul while advocating modest programmatic refinements, *Challenge* grandly promised a criminal justice template that never materialized.

1980S: APOLOGIA

If the 1970s had been a decade during which neither administrative nor systems thinking countervailed against the value of criminal justice reform, then developments during the 1980s were instead a decade during which the need for criminal justice reform. Bipartisan skepticism in criminal justice continued, with the left worried about state over—reach, and the right with the state’s servile mawkishness. When Allen (1981) identified that a rehabilitative ideal presupposed both a “strong faith in the malleability of human behavior and human character” and “a sufficient consensus of values to permit practical agreement on what it means to be rehabilitated,” he attributed that ideal’s decline to a failure to meet either prong. Disputes that were fought *sub rosa* between proponents of the administrative and systems rationalities were adrift in indecision and disagreement about the Big Three Questions of Criminal Justice, pertaining to (1) whether the state was capable of changing people; (2) whether it deserved to; and (3) what people should be changed into.

During the 1980s, only the first of those Big Three Questions was given much oxygen. Both the Police Foundation experiments and the Martinson were retold as errors of method, and not of politics. As the 1970s drew to a close, scholars revisited both sets of null findings and found they had mis-interpreted the data. As would later be noted, the Kansas City Experiment was mired in the same insensitivity to Type-II errors that Lipton had cautioned against in Martinson’s reading of the data on correctional treatment effectiveness (Weisburd et al. 2012). The problem was one of dosage: spreading police presence over the totality of a patrol beat was far too diffuse and diluted to concentrate the deterrent effect police presence might have had. It should have come as no surprise that across control and experimental conditions, respondents neither perceived differences in exposure or fear of crime nor that there had been any appreciable impact on the frequency of arrests.

The Martinson report drew the same scrutiny, but this time it was Martinson himself who withdrew his earlier position. Writing in the far less visible *Hofstra Law Review* in 1978, Martinson re-analyzed his earlier data and concluded that “contrary to common belief, the rate of recidivism ... in this country is not high, it is quite low” and that “contrary to [his] previous position, some treatment programs *do* have an appreciable effect on recidivism.” (p. 244). The problem, as he noted, was that the vote count method of synthesizing large numbers of primary studies was vulnerable to underpowered studies—the very kind of problem that ordinarily plagued criminological research then as it does now. Tallying the non-significant results of a long list of evaluations of correctional treatment when none of those studies commanded sufficient power to reject the null was a recipe for the exact result he had drawn five years earlier. This time around, he had learned his mistake.

Neither of the methodological flaws that initiated what has since been termed the era of “nothing works” would receive as much attention in their negative refutation as they originally had in their affirmative error. But both the Police Foundation and Martinson stories established that the methodological errors of the initial effort had a significant role to play in making criminal justice reform reasonable or unreasonable. Empirical measurement and scientific legitimation had taken on a new valence that had theretofore been absent. As the 1980s continued, the Big Three Questions of Criminal Justice collapsed together, which in turn resulted in empirical questions about the state’s *capacity* to change people over-riding normative ones about whether it deserved to or into what image they should be changed. As the dust settled on the Police Foundation

experiments and the Martinson report, the progressive concerns of criminal justice reform had been eclipsed by the technocratic urge to identify *what works*.

Policing was at the vanguard of efforts to restore faith in criminal justice institutions in the early-1980s. A former captain of the Oakland Police department—James Stewart—had been installed as the new Director of the NIJ who promptly enlarged and streamlined the funding infrastructure for (especially experimental) police research (Sherman 2005). The victim's rights movement had joined with civil liberties organizations and criminal justice reform campaigners to elevate the profile of domestic violence as a core aspect of police work (Gottschalk 2006). Extant wisdom in the policing of domestic violence fell along three axes (Sherman et al. 1992): among the police, it was believed that domestic violence was too intractable a solution to be solved by any officer arriving at the scene of a report; among clinicians, it was believed that only technical mediation and anger-management could quell the behavioral bases of domestic violence perpetration; and among victims rights activists, it was believed that only separation of an aggressor, ideally through confinement, could provide the reprieve necessary to ensure safety. In Minneapolis, more than three-hundred misdemeanor domestic violence cases were randomly assigned to one of three experimental conditions that reflected each of those positions—temporary separation, officer-led mediation, or presumptive arrest. Despite slippage in treatment integrity, the results nonetheless clearly pointed in favor of the greatest reductions in domestic violence following the officer's presumptive arrest of the male aggressor upon arriving at the scene of a domestic violence incident.

The Minneapolis domestic violence experiment provoked four reactions. The first was the adoption—prematurely given the fragility of results from a single experiment, as the evaluators themselves affirm (Sherman et al. 1992)—of a presumptive arrest policy for all domestic violence callouts across many American cities. The second was a program of replication experiments in a handful of cities across the country, whose results complicated the crime-prevention narrative first observed in Minneapolis. The third was that the experiment spurred a program of further experimental collaborations between criminologists (led by Lawrence Sherman) and the Minneapolis Police Department. Shortly after the domestic violence experiment drew to a close, the Repeat Call Address Policing (RECAP) Experiment assigned a team of four patrol officers to devise solutions to problems at 250 addresses that generated repeat calls for service. Although evaluators were disappointed to observe no discernible reduction in calls for service from those addresses that received intensive police exposure, the experiment nonetheless produced a much more significant finding that would form the basis of evidence-based policing's biggest success: the observation that more than half of a city's calls for service emanated from less than three percent of its addresses—a finding that has since replicated wherever it has been tested (Weisburd 2015).

The fourth reaction to the Minneapolis domestic violence experiment was that, unlike the Kansas City and Newark experiments that took one vision of police efficacy off the table without placing a new one in its stead, Minneapolis proposed a new path forward. In so doing, it answered the angst that “there is a pervasive sense that older ways of thinking about crime have lost their usefulness and credibility; but no convincing alternatives have come forward to take their place.” (Currie 1985: 10) The Minneapolis apologia thus reversed the Kansas City story in two senses: first, police *can* reduce crime, so the experiment showed; second, experiments designed *well* would have proved as much. The experiment's results were as much a victory for the experimental method as they were for the police institution itself.

Change was afoot in corrections, too. Echoing one of the insights from Minneapolis, the RAND Corporation explored the practical applications of Wolfgang et al.'s (1972) observation that the majority of offending behaviors were concentrated among a minute subset of people within a population. The correctional response to that concentrated skewness was clear: as had been the case for Minneapolis RECAP, direct resources to the few problem units to which the greatest problems were attributed. In the years that followed, the logic of “selective incapacitation” most closely associated with correctional research thus became the blueprint for a full-scale reorganization of criminal justice governance.

Shortly after the RAND study, for example, the NAS published its third, fourth, fifth, and sixth Consensus Study Reports—all inductive, all chaired by Alfred Blumstein, and thus all steeped in systems-based wisdoms—between 1983 and 1986. All four CSRs schematically tied the allocation of scarce criminal justice resources to the “problem units” that vexed the Minneapolis police department. The program of research in developmental and life-course criminology to follow that had stably developed for a half century since the Gluecks exploded and became subsumed in the research program on *Concentration of Crime* that occupied the fourth-most central node in the network of criminological thought displayed in Figure 1.5. It appears as the purple node in the center of the Figure, serving as the sinew that connects the blue idea-region on policing research in the right hemisphere with the orange idea-region on correctional research in the left hemisphere. The observation of crime’s concentration therefore realizes one of the key ambition of systems thinking in criminal justice reform: uncoupled from any one particular substantive component of the criminal justice system and instead widely applicable to all of them, the *Concentration of Crime* presents a way of thinking about criminal justice—indeed, a rationality—rather than a parochial topic of any one criminal justice agency. The criminology of offending ‘careers’ was readily transposable from the developmental and life-course criminology in corrections as it was to the criminology of place that propelled policing research in the 1980s.

The observation that crime concentrated within a subset of the population was by no means an invention of the 1980s, but its identification as a platform of reorganizing criminal justice *as a system* certainly was. The same principles proliferated beyond policing and prisons and were shown to have important policy implications in schools (*e.g.*, Gottfredson 1987), parenting (*e.g.*, Olds et al. 1986), reentry (*e.g.*, Petersilia & Turner 1993), and beyond. Selective incapacitation was neatly compatible with the systems rationality: it imagined crime as a manipulable unit whose prevention could be efficiently managed through technical adjustments and resource allocations; it provided a coherent logic model that was easily transposable from one criminal justice agency (say, corrections) to the next (say, policing or courts or otherwise); and it demonstrated the crime preventive potential of targeting problems at the point of their control (namely their enforcement) rather than at their point of origin (namely their root causes).

Selective incapacitation’s embodiment of the systems rationality dominated criminal justice reform throughout the 1980s. It undergirded the Sentencing Reform Act and that Act’s attendant establishment of the Sentencing Commission in 1984, which—citing to the rehabilitative ideal’s decline as an “outmoded model” (Senate Report on the Crime Control Act of 1984, S. Rep. #98–225 1983)—left only deterrence, retribution, and incapacitation intact among the remaining complement of permissible penal rationales. Justice Scalia’s dissenting opinion in *Mistretta* (1989) complained about the systems rationality’s tendency to hide how “the decisions made by the Commission are far from technical, but are heavily laden (or ought to be) with value judgments and policy assessments.” (p. 414)

Selective incapacitation thus elevated the *management* of problem *units* as the systems rationality's key scientific contribution in the 1980s. No longer was the efficacy of criminal justice agencies the concern of first order as had plagued the preceding decade; instead, criminal justice reform was a matter of identifying which units in the system were the most susceptible to state intervention.

1990S: REVEILLE

In many corners of criminal justice reform, the pallor of skepticism that had gripped criminology twenty years earlier still lingered. David Bayley, for example, plaintively worried that

The police do not prevent crime. This is one of the best kept secrets of modern life. Experts know it, the police know it, but the public does not know it. Yet the police pretend that they are society's best defense against crime and continually argue that if they are given more resources, especially personnel, they will be able to protect communities against crime. This is a myth.

(Bayley 1994: 3)

Yet despite those misgivings, the successes of 1980s experimentation sounded reveille for the "use and usefulness of criminology" (Sherman 2005) in the 1990s. Key to that growing sense of criminology's promise to criminal justice policy reform was that, unlike the skepticisms of the 1970s, it was *both* the case that agencies of criminal justice could appreciably influence crime, *and* that criminologists were equipped to identify those influences (Simon 1993).

In policing, the observation that crime concentrated in a minute subset of a city's space became the basis of one of the most successful experiments in police research. Over the course of a year, fifty-five experimental 'hot spots' that attracted an oversized share of police calls for service were exposed to three hours of saturated police presence a day, and fifty-five control hot spots received the same police presence as usual. The experimenters observed a modest reduction in crime calls for service and in disorder at the experimental sites (Sherman & Weisburd 1995)—reductions that have proved much more considerable during subsequent replication experiments that share an emphasis on policing concentrated "micro-places" of crime (Weisburd et al. 2012).

The Minneapolis hot spots experiment had been framed explicitly in contest against the findings of the Police Foundation experiments from two decades before. The hot spots policing experiments did not go unanswered. Critiques abound concerning the dysfunctions of over-policing, of the elision of moral and political stakes, the discursive shift from race to space, and of the ontological shift of the penal subject from persons to places (*e.g.*, Harcourt 2008; Rios 2011; Rosenbaum 2006; Thacher 2019). But in the decades to follow, the positive effects that Sherman & Weisburd observed upended once and for all the belief espoused in the opening pages of David Bayley's *Police and the Future*, and charted a program of reform pinned to the experimental method. The research agenda that the Minneapolis hot spots experiment inaugurated would be heralded as one of the most robust and promising findings in both policing research specifically (Lum & Koper 2017) and in policy-informing criminology generally (Clear 2010).

Like Sherman and Weisburd's answer to the question that the Police Foundation experiments posed about the police's capacity to control crime, correctional research witnessed a similar answer to the question that its intellectual forbear posed. A team of Canadian criminologists synthesized 154 quasi-experimental that tested the effectiveness of correctional treatment, and found that a differentiated view of the underlying research pointed toward types of programming that

successfully reduced recidivism (Andrews et al. 1990). Meta-analysis in this instance solved the problem that Lipton, Martinson, and Wilks's vote-count strategy had posed, which tallied the non-significant effects of under-powered studies. Meta-analysis, instead, allowed the researchers to uncover moderator effects that differentiated effective programs from ineffective ones, and from this emerged the 'Risk-Need-Responsivity' model of selecting treatment recipients based on their likelihood of reoffending. People at lowest risk of recidivating, it was observed, tended to display iatrogenic effects from correctional treatment, and people at highest risk of recidivating reported the most significant reductions in crime at follow-up after treatment (Bonta & Andrews 2016).

The ontological shift that selective incapacitation encouraged was thus complete: for policing, the error of the Police Foundation experiments was one of diluted dose across experimental conditions that was soluble by saturating the micro-places where crime concentrated with police presence; for corrections, the error of the Martinson report was one of methodologically inadequate quantitative synthesis techniques whose solution showed that targeting people at highest risk of recidivating would produce considerable reductions in future crime. The logic of criminal justice reform, joined at the hip with the systems rationality, was identical across both solutions: find the problem units in the population and concentrate resources.

By the late-1990s, the crime control capacities of criminal justice had recovered entirely from the apostasies of the previous generation. Nowhere was this emblemized better than when Congress commissioned a team of criminologists at the University of Maryland to assemble evidence on the efficacy of programs in crime and justice. The resulting "Maryland Report" (Sherman et al. 1997) served as the late bookend to the shelf that had opened with Martinson (LaFree 2018). Sorting the program evaluation literature into evidence of "what works," "what does not," and "what's promising" to prevent crime in policing, the family, schools, corrections, labor markets, and places.

The report's chief contribution, however, was not its ratification of the wisdom that crime prevention was indeed possible. Rather, its chief contribution was the "Maryland Scientific Methods Scale" (Sherman et al. 1997: Ch.1) that ranked the quality of crime prevention program evaluations on a score from One (indicating a correlation between an intervention and a crime outcome) to Five (indicating a randomized controlled trial). The Maryland Scientific Methods Scale (SMS) became a shibboleth for demonstrating whether and how well crime prevention programs meet their stated end (Hope 2005). By casting problems of criminal justice efficacy in the causal language of program evaluation, criminology's 'experimental turn' took shape only a decade after its counterpart gripped other social sciences (Sampson 2010).

The consequences of the SMS and criminology's associated experimental turn bring the systems rationality's full limits into view. On one hand, the program evaluation framing imagines crime as a problem to be managed through the effective organization and refinement of criminal justice agencies. The rationality's chief vulnerability lies in its susceptibility to ruptures in criminal justice's self-regulating 'order.' On the other hand, the errors of crime control are reconstrued as programmatic as opposed to political. This is, after all, the systems rationality's core premise: what is external to the system is beyond consideration. If the system's inputs are limited to "reported" or "observed" crime alone (See Figure 4.2), then the politics of penal policy play no role—or, at best, a distorting one (Barkow 2019; Pratt 2007).

Across both policing and corrections over the course of 1965 to 1995, criminological thought had 'stratified' into ways of studying crime and criminal justice (*see* Chapter One), ways of

thinking about crime and criminal justice (*see* Chapter Three), and thus ways of framing the “use and usefulness of criminology” itself (Sherman 2005). The history of criminological thought corrects the misprision of criminology’s salvific effect on “saving” rehabilitation by situating the systems rationality that underpinned *Challenge*’s back-end as that report’s greatest success. The buildup of the carceral state that followed *Challenge* was less an abdication of the due process liberalism that that characterized *Challenge*’s nod to administration than it was an embrace of systems-thinking. The vintage year for the null hypothesis cast a pallor over the ambitious reform platforms of both the administrative and systems rationalities, and as a result, neither reform platform had been given a full opportunity to be tested.

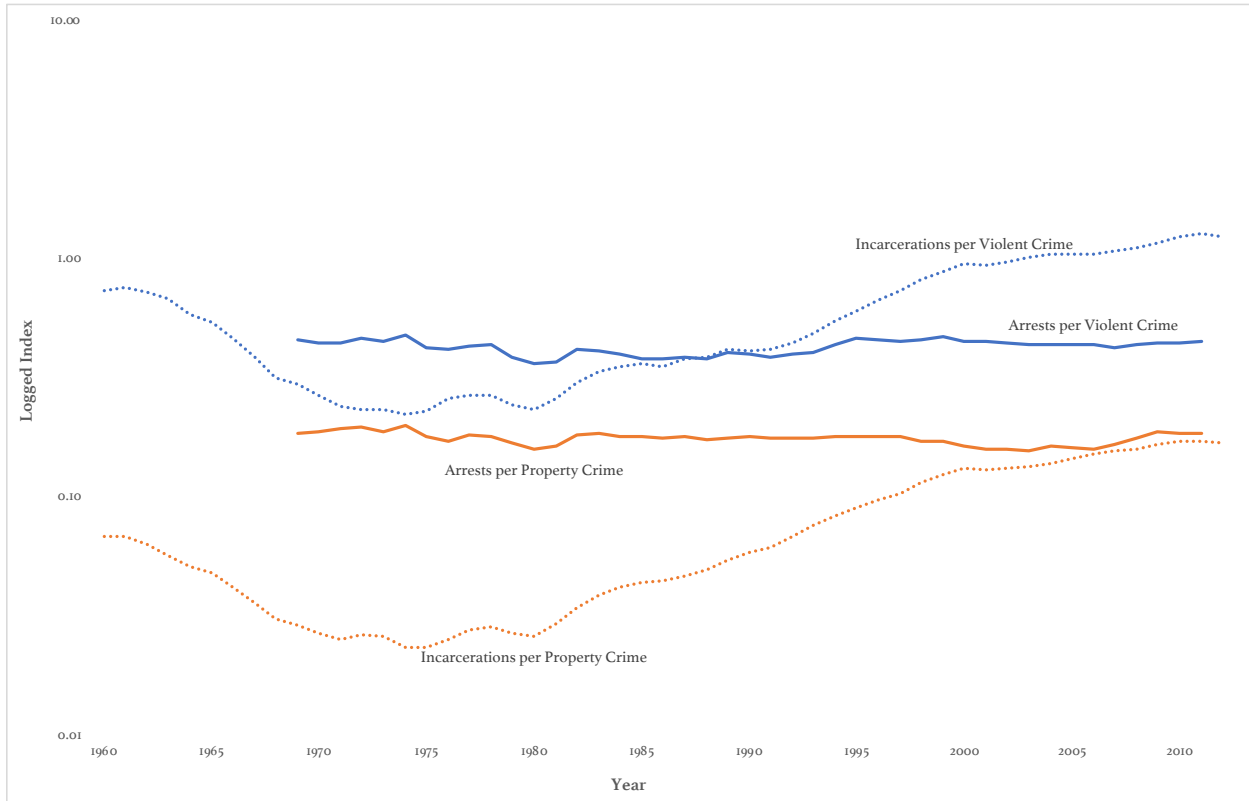
2000s: REFLECTIONS

The historical narrative surrounding the vintage year of the null hypothesis is that of a widespread retreat from faith in criminal justice institutions to meet their progressive goals. This is conventionally historicized as though American criminal justice and adjacent institutions were “losing legitimacy” (*e.g.*, LaFree 2018; *see also* Kohler-Hausmann 2019). Perhaps precipitously rising crime during a period of unresponsively stable incarceration jolted the nation’s penal appetites. Crude estimates of American punitivity represented as the ratio of incarcerations per crime certainly suggest that throughout the 1960s, American imprisoning belied Blumstein & Cohen’s (1973) assertion that such penal appetites were indeed stable after all. For example, Enns’s (2016) punitivity index has been reproduced as the dotted lines in Figure 4.3, albeit with the minor modification that I use a logged *y*-axis to display more easily the rate of change over time. Throughout the 1960s, national incarcerations per crime consistently dropped, indicating a leniency that would reverse only in 1974. Figure 4.3 more clearly displays the accelerating drive to incarcerate proportional to the crime rate during the 1990s crime drop that Figure 4.1 obscured. Note in particular that the logged linear trend from 1974 onward reflects an accelerating rate of punitivity as opposed to a constant one.

Comparison of Figure 4.1 with Figure 4.3 clarifies three distinct periods in the relationship between rates of crime and incarceration. The first, extending from the early-1960s to the mid-1970s, is one of declining punitivity. But that decline is a function of stable incarceration while violent crime rapidly increased. The lock-step distance between between the orange and blue dotted lines in Figure 4.3 shows that the same relationship held true for property crime as well. Single-minded attention to the numerator (incarceration) that ignores the denominator (crime) thus misses that change in the punitivity index during those years reflected constancy in the policy infrastructure of imprisonment while crime dramatically changed. To characterize this period as one of ‘leniency’ misses the point: the 1960s to the early-1970s were instead characterized by a steadfast commitment to the ideal of dynamic equilibrium—a dogmatic insistence that even as crime rates change, punishment need not.

A wealth of literature untethers the policy presumption that crime rates predict incarceration. That literature univocally affirms that incarceration is a function of policy choices, only some of which might be determined by crime—and even those tend to bear a modest and indirect connection to crime (NAS 2014; Western & Muller 2013). But large-scale structural dislocation in the crime-incarceration connection, of the kind that ought to have punctured belief in the premise of dynamic equilibrium—a premise at the very core of systems thinking—nonetheless did not do so. Instead, the systems rationality vibrantly continued.

Figure 4.3: ARREST AND INCARCERATION AS A LOGGED FUNCTION OF CRIME RATES. Solid lines display logged indices of changes in arrest rates pinned to property (orange) and violent (blue) crime rates; dotted lines reproduce Enns' (2016: 11) measure of changes in incarceration rates pinned to the same. *Source:* FBI Uniform Crime Reports; BJS Corrections Statistical Analysis Tool; BJS Arrest Data Analysis Tool (2019).



Two interpretations made the persistence of the systems rationality tenable. The first is that the period following the mid-1970s was one of self-correcting equilibrium. The punitivity index reversed course and took a sharp upswing for both incarcerations per violent crime (the dotted orange line in Figure 4.3) and per property crime (the dotted blue line in Figure 4.3). Homeostasis can take a while, so the proponent of systems thinking might have counseled, but come it eventually will. After a decade and a half of ‘leniency,’ the decade and a half to follow was not so much an instance of ‘punitivity’ as it was a regression to the mean.

The second interpretation for why the systems rationality persisted is that the two trends that constituted the punitivity index during those years represented *not* a return of crime rates to a level that fell within the acceptable bounds of carceral capacity as might arbitrarily have been the case in the early-1960s. Rather, the restored values found toward the end of the 1980s resulted from a dramatic expansion in incarceration to correct for the underlying crime rates. In that sense, during a climate of Martinson-inspired worry about the state’s capacity to control crime, carceral expansion from the mid-1970s onward might have looked like a viable systems-based correction to the leniency that had prevailed since 1960.

But setting the policies governing policing against those governing corrections complicates the narrative that the Police Foundation experiments and the Martinson report eroded faith in progressive criminal justice writ large, and that carceral expansion intuitively presented itself as a

systems-based reaction. In the first instance, whatever leniency characterized the years preceding the mid-1970s was ‘corrected’ by the end of the 1980s, at which point crime rates plateaued and then fell, initiating Figure 4.3’s third period (from the early-1990s to the present). During that last period, incarcerations per violent crime and per property crime continued to increase, this time as a function not of an increase in both the numerator (incarcerations) and the denominator (crimes), but rather as a function of increases in the numerator but a *decrease* in the denominator. If the system had regressed to the mean by the late-1980s, then whatever correction dynamic equilibrium called for ought to have impelled a concomitant discontinuation of the phenomenon impelling incarceration to rise in the middle of the 1970s. The implication on this score is that the system was emphatically *not* self-regulating. Homeostasis itself was untenable.

A second development should have shattered the systems rationality yet failed to do so. Solid lines in Figure 4.3 represent the analogue to Enns’s punitivity score, but instead of representing incarcerations per (violent or property) crime, I have instead calculated arrests per (violent or property) crime as far back as the FBI Uniform Crime Reports allow. Unlike Enns’s punitivity index, we might consider this ratio an ‘apprehension index’ reflecting the extent to which policing tracked crime. The pattern here is incontrovertible: the ratio of arrests to crime across both property and violent crimes has been consistently flat since 1969. If Martinson’s null findings spurred a retreat away from the progressive ideals of the rehabilitative prison and toward an incapacitative one, why didn’t the Police Foundation Experiments prompt a similar shift in the policy orientation of the police?

The answer must be that skepticism in the capacity of criminal justice agencies to control crime plays at most a contingent and indeterminate role in the formulation of criminal justice policy. As Marie Gottschalk (2015) notes, the notion that carceral expansion from the 1970s onward was fostered by or even propelled by ubiquitous null findings in criminological research inflates the role that science plays in what are fundamentally *political* choices. On this, the proponent of systems rationality agrees that mass incarceration is attributable to the displacement of crime politics from experts internal to the system to populist politics external to it (Mayeux 2018: 88). This allergy to populist politics, privileging instead the expertise that criminologists have to play, inscribes a policy stance that would invite further engagement with criminologists as the solution to penal policy problems (Sherman 2009; Tonry 2013).

What, then, of the administrative rationality? Systems thinking doubtless played the more prominent role in criminology’s experimental turn and the evidence-based movement that followed, but the administrative preoccupation with law’s capacity to advance desirable ends did not disappear altogether. Indeed, President Obama’s paeon to evidence-based criminal justice reform charted a platform of primarily administrative- rather than systems-based successes during the preceding eight years. Indeed, the proudest criminal justice accomplishment of his administration had been the establishment of a panoply of centralized oversight boards and inter-agency coordination protocols, most notably as part of a program of federal intervention in criminal justice that hearkened to the administrative recommendations redolent of the late-1960s (Obama 2017; *see also* President’s Task Force on 21st Century Policing 2015).

But there is no question that the administrative rationality has played a much less visible role in the past half-century of criminal justice reform than its systems-based counterpart. Consequently, the evidence-based platform has successfully portrayed carceral expansion as a product of bad or misguided research rather than perverse policy choices, and as a corollary has positioned technical—in particular, criminological—refinements as the solution. The problem is

not simply that the evidence-based movement pins criminal justice reform to crime prevention, leaving those same reforms vulnerable to moments when crime might increase without reference to crime control policy. The problem is also much deeper. Crime control is about so much more than whether or how much crime is suppressed by any given policy; it is also about *how* the state goes about its crime control function. When the question concerning the state's *capacity* to advance a stated end over-rides questions about the *method* with which it does so, as Allen (1981) noted had contributed to the rehabilitative ideal's decline, then penal policy takes on an avowedly scientific and technocratic flavor from which political considerations are hard to rescue.

WORKS CITED

- Abbott, A. 1995. Things of boundaries. *Social Research*, 62(4): 857–882.
- Abbott, A. 2001. *Chaos of disciplines*. Chicago UP.
- Abel, R. L. 2010. Law and society: Project and practice. *Annual Review of Law and Social Science*, 6: 1–23.
- Adams, R. 1976. Criminal justice: An emerging academic profession and discipline. *Journal of Criminal Justice*, 4: 303–314.
- Adler, F. 1997. The ASC and women: One generation without, one generation with. *The Criminologist*, May/June: 1, 3–5.
- Akers, R. L. 1979. Theory and ideology in Marxist criminology. *Criminology*, 16: 527–544.
- Allen, F. A. 1981. *The decline of the rehabilitative ideal: Penal policy and social purpose*. Yale UP.
- Amara, N., & R. Landry. 2012. Counting citations in the field of business and management: why use Google Scholar rather than the Web of Science. *Scientometrics*, 93(3): 553–581.
- American Friends Service Committee. 1971. *Struggle for justice: A report on crime and punishment in America*. Hill & Wang.
- Andrews, D. A., Zinger, I., Hoge, R. D., Bonta, J., Gendreau, P., & Cullen, F. T. 1990. Does correctional treatment work? A clinically relevant and psychologically informed meta-analysis. *Criminology*, 28(3): 369–404.
- Baliatti, S., M. Mäs, & D. Helbing. 2015. On disciplinary fragmentation and scientific progress. *PLoS One*, 10(3): e0118747.
- Barker, V. 2009. *The politics of imprisonment: How the democratic process shapes the way America punishes offenders*. Oxford UP.
- Barkow, R. E. 2019. *Prisoners of politics: Breaking the cycle of mass incarceration*. Belknap/Harvard UP.
- Barnes, B. 1977. *Interests and the growth of knowledge*. Routledge.
- Bayley, D. H. 1994. *Police for the Future*. Oxford UP.
- Beirne, P. 1987. Adolphe Quetelet and the origins of positivist criminology. *American Journal of Sociology*, 92(5): 1140–1169.
- Beirne, P. 1993. *Inventing criminology: Essays on the rise of 'homo criminalis.'* SUNY Press.
- Berger, D. 2014. *Captive nation: Black prison organizing in the Civil Rights Era*. North Carolina UP.
- Berger, P. L., & Luckmann, T. 1991. *The social construction of reality: A treatise in the sociology of knowledge*. Penguin.
- Bergin T. 2013. *The evidence enigma: Correctional boot camps and other failures in evidence-based policymaking*. Routledge.

- Berkeley, G. (1734) *The analyst: A discourse addressed to an infidel Mathematician*. Library of Alexandria.
- Bibas, S. 2012. *The machinery of criminal justice*. Oxford UP.
- Bittner, E. 1970. *The functions of the police in modern society: A review of background factors, current practices, and possible role models*. National Institute of Mental Health.
- Black, D. J. 1972. The boundaries of legal sociology. *Yale Law Journal*, 81:1086-1100.
- Black Panther Intercommunal News Service*. June 8, 1974. Vol IX, no. 24.
- Blomberg, T., Mestre, J., & Mann, K. 2013. Seeking causality in a world of contingency: Criminology, research, and public policy. *Criminology & Public Policy*, 12(4): 571–584.
- Blumstein, A. 1994. Interaction of criminological research and policy. *Journal of Quantitative Criminology*, 12(4): 349–361.
- Blumstein, A. 2018. Science and technology and the President's Crime Commission: Past and future. *Criminology & Public Policy*, 17(2): 271–282.
- Blumstein, A., & Cohen, J. 1973. A theory of the stability of punishment. *The Journal of Criminal Law and Criminology*, 64(2): 198–207.
- Boffey, P. 1975. *The brain bank of America: An inquiry into the politics of science*. New York: McGraw-Hill.
- Bonta, J., & Andrews, D. A. 2016. *The psychology of criminal conduct*. Routledge.
- Börner, K., C. Chen, & K.W. Boyack. 2005. Visualizing knowledge domains. *Annual Review of Information Science and Technology*, 37(1): 179–255.
- Bosworth, M., & C. Hoyle, eds. 2011. *What is criminology?* Oxford UP.
- Boyack, K., K. Börner, & R. Klavans. 2008. Mapping the structure and evolution of chemistry research. *Scientometrics*, 79(1): 45–60.
- Bourdieu, P. 1975. The specificity of the scientific field and the social conditions of the progress of reason. *Social Science Information*, 14(6): 19–47.
- Blomberg, Thomas G. 2010. Introduction. In *Becoming deviant*, 2nd ed. David Matza, Englewood Cliffs, NJ: Prentice Hall.
- Bourdieu, Pierre. 1988. *Homo academicus*. Palo Alto, CA: Stanford University Press.
- Bourdieu, Pierre. 2004. *Science of science and reflexivity*. Chicago, IL: University of Chicago Press.
- Bright, L.K. 2018. Du Bois' democratic defense of the value-free ideal. *Synthese*, 195(5): 2227–2245.
- Bulletin of the Society for the Advancement of Criminology*. December 6, 1954. Vol. 1, no. 7.
- Burgess, A. 1962. *A clockwork orange*. William Heinemann.
- Camic, C. 1995. Three departments in search of a discipline: Localism and interdisciplinary interaction in American sociology, 1890–1940. *Social Research*, 62: 1003–1033.
- Campbell, D. T. 1969. Reforms as experiments. *American Psychologist*, 24(4): 409–429.

- Campbell, D. T. 1973. The social scientist as methodological servant of the experimenting society. *Policy Studies Journal*, 2(1): 72–75.
- Campbell, D.T. 1991. Methods for the experimenting society. *Evaluation Practice*, 12(3): 223–260.
- Cartwright, N., & Hardie, J. 2012. *Evidence-based policy: A practical guide to doing it better*. Oxford UP.
- Cheliotis, L. 2006. How iron is the iron cage of new penology?: The role of human agency in the implementation of criminal justice policy. *Punishment & Society*, 8(3): 313–340.
- Clear, T. R. 2010. Policy and evidence: The challenge to the American Society of Criminology: 2009 presidential address to the American Society of Criminology. *Criminology*, 48(1): 1–25.
- Cohen, R. 2009. *Freedom's orator: Mario Savio and the radical legacy of the 1960s*. Oxford UP.
- Cohen, S. 1988. *Against criminology*. Transaction.
- Cohn, E.G. 2011. Changes in scholarly influence in major American criminology and criminal justice journals between 1986 and 2005. *Journal of Criminal Justice Education*, 22(4): 493–525.
- Cohn, E.G., & D.P. Farrington. 1990. Differences between British and American criminology: An analysis of citations. *British Journal of Criminology*, 30(4): 467–482.
- Cohn, E.G., & D.P. Farrington. 2006. Assessing the quality of American doctoral program faculty in criminology and criminal justice, 1991–1995. *Journal of Criminal Justice Education*, 9(2): 187–210.
- Cohn, E.G., D.P. Farrington, & A. Iratzoqui. 2014. *Most-cited scholars in criminology and criminal justice, 1986–2010*. Springer.
- Cohn, E.G., D.P. Farrington, & R.A. Wright. 1998. *Evaluating criminology and criminal justice*. Greenwood.
- Cohn, E.G., A. Iratzoqui, D.P. Farrington, A.R. Piquero, & Z.A. Powell. 2018. Most-cited articles and authors in Crime and Justice, 1979–2015. *Crime and Justice*, 47(1): 475–508.
- Cole, S. 1992. *Making science: Between nature and society*. Harvard UP.
- Conyers, J. 1979. Criminology, economics, and public policy. *Crime & Delinquency*, 25(2): 137–144.
- Crimmins, T. 2018. Incarceration as incapacitation: An intellectual history. *American Affairs*, 2(3): 144–166.
- Cronin, T. E., Cronin, T. Z., & Milakovich, M. E. 1981. *US v. Crime in the Streets*. Indiana UP.
- Cullen, F. T. 2005. The twelve people who saved rehabilitation: how the science of criminology made a difference. *Criminology*, 43(1): 1–42.
- Cullen, F. T., & Messner, S. 2007. The making of criminology revisited: An oral history of Merton's anomie paradigm. *Theoretical Criminology*, 1: 5–37.
- Currie, E. 1985. *Confronting crime: An American challenge*. Pantheon.

- Daems, T. 2009. *Making sense of penal change*. Oxford UP.
- De Winter, J.C., A.A. Zadpoor, & D. Dodou. 2014. The expansion of Google Scholar versus Web of Science: A longitudinal study. *Scientometrics*, 98(2): 1547–1565.
- Deaton, A., & Cartwright, N. 2018. Understanding and misunderstanding randomized controlled trials. *Social Science & Medicine*, 210: 2–21.
- Desch, M. 2019. *The cult of the irrelevant: The waning influence of social science on national security*. Princeton studies in international history and politics. Princeton UP.
- DiMaggio, P. J., & Powell, W. W. 1983. The iron cage revisited: Institutional isomorphism and collective rationality in organizational fields. *American Sociological Review*, 48: 147–160.
- Dooley, B. 2018. Whither criminology? The search for a paradigm over the last half-century. *American Sociologist*, 49(2): 258–279.
- Dooley, B. 2010. *Whither criminology? On the state of criminology's paradigm*. Unpublished doctoral dissertation, University of Missouri—St. Louis.
- Douglas, H. 2009. *Science, policy, and the value-free ideal*. Pittsburgh UP.
- Douglass, J. 2007. *The California idea and American higher education: 1850 to the 1960 Master Plan*. Stanford UP.
- Dunn, W.H., (ed.). 1998. *The experimenting society: Essays in honor of Donald T. Campbell*. Transaction.
- Enns, P. K. 2016. *Incarceration nation: How the United States became the most punitive democracy in the world*. Cambridge UP.
- Farrington, D.P., E.G. Cohn, & A. Iratzoqui. 2019. Who are the most-cited scholars in Asian criminology compared with Australia, New Zealand, North America, and Europe? *Asian Journal of Criminology*, 14(1): 61–76.
- Feeley, M. M. 2018. How to think about criminal court reform. *Boston University Law Review*, 98(3): 673–730.
- Feeley, M. M., & Rubin, E. L. 2000. *Judicial policy making and the modern state: How the courts reformed America's prisons*. Cambridge UP.
- Feeley, M., & A. Sarat. 1981. *The policy dilemma: Federal crime policy and the Law Enforcement Assistance Administration, 1968–1978*. Minnesota UP.
- Feyerabend, P. 1993. *Against method*. Verso.
- Foner, E. 2011. *Reconstruction: America's unfinished revolution, 1863-1877*. Harper Collins.
- Ford Foundation. 1970. *A more effective arm: A report on a Police Development Fund, newly established by the Ford Foundation*. Ford Foundation Press.
- Franceschet, M. 2009. A comparison of bibliometric indicators for computer science scholars and journals on Web of Science and Google Scholar. *Scientometrics*, 83(1): 243–258.
- Frickel, S., & Gross, N. 2005. A general theory of scientific/intellectual movements. *American Sociological Review*, 70(2): 204–232.

- Friedland, R., & Alford, R. R. 1991. Bringing society back in: Symbols, practices, and institutional contradictions. In *The new institutionalism in organizational analysis*, eds. Walter W. Powell and Paul J. DiMaggio. Chicago UP.
- Friedman, Lawrence M. 1986. The law and society movement. *Stanford Law Review*, 38: 763–780.
- Frost, N. A., & Clear, T. 2007. Doctoral education in criminology and criminal justice. *Journal of Criminal Justice Education*, 18: 35–52.
- Gabbidon, S.L., & R. Collins. 2012. Using Google Scholar to determine the most-cited criminology and criminal justice-related books. *American Journal of Criminal Justice*, 37(1): 33–45.
- Galliher, J. F. 1978. The life and death of liberal criminology. *Contemporary Crises*, 2: 245–263.
- Galliher, J. F. 1979. Government research funding and purchased virtue: Some examples from criminology. *Crime and Social Justice*, 11: 44–50.
- Garland, D. 1992. Criminological knowledge and its relation to power. Foucault’s genealogy and criminology today. *British Journal of Criminology*, 32(4): 403–422.
- Garland, D. 2002. Of crimes and criminals: The development of criminology in Britain. *The Oxford handbook of criminology*, 3, 7–50.
- Garland, D., & R. Sparks. 2000. Criminology, social theory and the challenge of our times. *The British Journal of Criminology*, 40(2): 189–204.
- Garth, B., & Sterling, J. 1998. From legal realism to law and society: Reshaping law for the last stages of the social activist state. *Law & Society Review*, 32: 409–472.
- Geis, G. 1995. The limits of academic tolerance: The discontinuance of the School of Criminology at Berkeley.” In *Punishment and social control: Essays in honor of Sheldon Messinger*, eds. Thomas G. Blomberg and Stanley Cohen. De Gruyter.
- Gilmore, R.W. 2007. *Golden gulag: Prisons, surplus, crisis, and opposition in globalizing California*. California UP.
- Goodman, P., J. Page, & M. Phelps. 2017. *Breaking the pendulum: The long struggle over criminal justice*. Oxford UP.
- Gottfredson, D. 1987. An evaluation of an organization development approach to reducing school disorder. *Evaluation Review*, 11(6): 739–763.
- Gottschalk, M. 2006. *The prison and the gallows: The politics of mass incarceration in America*. Cambridge UP.
- Gottschalk, M. 2015. *Caught: The prison state and the lockdown of American politics*. Princeton UP.
- Green, G.S. 1981. A test of the Ortega Hypothesis in criminology. *Criminology*, 19(1): 45–52.
- Greenhalgh, T., & Russell, J. 2009. Evidence-based policymaking: a critique. *Perspectives in biology and medicine*, 52(2): 304–318.
- Greenwood, P. W., & Abrahamse, A. F. 1982. *Selective incapacitation*. Rand.

- Groves, W.B., & Sampson, R.J. 1987. Traditional contributions to radical criminology. *Journal of Research in Crime and Delinquency*, 24: 181–214.
- Gutting, G. 1989. *Michel Foucault's archaeology of scientific reason*. Cambridge UP.
- Hagan, J. 2012. *Who are the criminals? The politics of crime policy from the Age of Roosevelt to the Age of Reagan*. Princeton UP.
- Haggerty, K.D. 2004. Displaced expertise: Three constraints on the policy-relevance of criminological thought. *Theoretical Criminology*, 8(2): 211–231.
- Harcourt, B. E. 2008. *Against prediction: Profiling, policing, and punishing in an actuarial age*. Chicago UP.
- Harcourt, B.E. 2011. *The illusion of free markets: Punishment and the myth of natural order*. Harvard UP.
- Harcourt, B. E. 2018. The systems fallacy: A genealogy and critique of public policy and cost-benefit analysis. *The Journal of Legal Studies*, 47(2): 419–447.
- Harzing, A.-W., & S. Alakangas. 2016. Google Scholar, Scopus and the Web of Science: A longitudinal and cross-disciplinary comparison. *Scientometrics*, 106(2): 787–804.
- Haskins, R. 2018. Evidence-based policy: The promise and challenges of a movement. *Annals of the AAPSS*, 678(1): 8–205.
- Healy, K., & Moody, J. 2014. Data visualization in sociology. *Annual Review of Sociology*, 40: 105–128.
- Heidt, J. M. 2011. *The evolution of criminological theories*. Unpublished doctoral dissertation, Simon Fraser University.
- Hendricks, J., & Breckinridge Peters, C. 1973. The ideal type and sociological theory. *Acta Sociologica*, 16(1): 31–40.
- Heyck, H. 2015. *Age of system: Understanding the development of modern social science*. Johns Hopkins UP.
- Hinton, E. 2016. *From the War on Poverty to the War on Crime: The making of mass incarceration in America*. Harvard UP.
- Holdaway, S., & P. Rock. 1998. *Thinking about criminology*. UCL Press.
- Holden, C. 1975. Prisons: Faith in 'rehabilitation' is suffering a collapse. *Science*, 188: 815–817.
- Hope, T. 2005. Pretend it doesn't work: The 'anti-social' bias in the Maryland Scientific Methods Scale. *European Journal on Criminal Policy and Research*, 11(3-4): 275–296.
- Huq, A. Z., & Muller, C. 2008. The war on crime as precursor to the war on terror. *International journal of law, crime and justice*, 36(4): 215–229.
- Isaac, J. 2012. *Working knowledge: Making the human sciences from Parsons to Kuhn*. Harvard UP.
- ISLEC; Institute for the Study of Labor and Economic Crisis. 1975. *The iron fist and the velvet glove: An analysis of the US police*. Crime and Social Justice Associates.
- Jackson, G. 1970. *Soledad brother: The prison letters of George Jackson*. Chicago Review Press.

- Jasanoff, S. 2009. *The fifth branch: Science advisers as policymakers*. Harvard UP.
- Jenness, V., & R. Grattet. 2001. *Making hate a crime: From social movement to law enforcement*. Russell Sage Foundation.
- Johnson, Lyndon B. 1964, January 8. *State of the Union address to Congress*.
- Johnson, Lyndon B. 1967, January 10. *State of the Union address to Congress*.
- Kaufman, L., & P.J. Rousseeuw. 2005. *Finding groups in data: An introduction to cluster analysis*. *Wiley series in probability and statistics*. Wiley.
- Kelling, G., Pate, A., Dickman, D., & Brown, C. 1974. The Kansas City preventive patrol experiment: A technical report. Police Foundation.
- Khurana, R. 2007. *From higher aims to hired hands: The social transformation of American business schools and the unfulfilled promise of management as a profession*. Princeton UP.
- Kleiman, M.A.R. 2008. *When brute force fails: How to have less crime and less punishment*. Princeton UP.
- Klockars, C. B. 1979. The contemporary crises of Marxist criminology. *Criminology*, 16: 477–515.
- Koehler, J. 2015. Development and fracture of a discipline: Legacies of the School of Criminology at Berkeley. *Criminology*, 53(4): 513–544.
- Koehler, J. 2019. Penal (Ant)Agonism. *Law & Social Inquiry*, 44(3): 799–805.
- Kohler-Hausmann, J. 2019. *Getting tough: Welfare and imprisonment in 1970s America*. Princeton UP.
- Krisberg, B. 1974. Teaching radical criminology. *Crime and Social Justice*, 1: 64–66.
- Kuhn, T. S. 1962. *The structure of scientific revolutions*, 50th anniversary ed. Chicago UP.
- Lacey, N. 2008. *The prisoner's dilemma: Political economy and punishment in contemporary democracies*. Cambridge UP.
- LaFree, G. (2018). *Losing legitimacy: Street crime and the decline of social institutions in America*. Routledge.
- Lamont, M. 2001. Three questions for a big book: Collins's *The Sociology of Philosophies*. *Sociological Theory*, 19: 86–91.
- Lamont, M., & Molnár, V. 2002. The study of boundaries in the social sciences. *Annual review of sociology*, 28(1): 167–195.
- Latour, B. 1988. *Science in action: How to follow scientists and engineers through society*. Harvard UP.
- Latour, B. 2005. *Reassembling the social: An introduction to Actor-Network-Theory*. Oxford UP.
- Latour, B., & Woolgar, S. 2013. *Laboratory life: The construction of scientific facts*. Princeton UP.
- Laub, J. H. 1983. *Criminology in the making: An oral history*. Northeastern UP.

- Laub, J. H. 1984. Talking about crime: Oral history in criminology and criminal justice. *Oral History Review*, 12: 29–42.
- Laub, J. H., & Frisch, N. E. 2016. Translational criminology: A new path forward. In T.G. Blomberg, J. Mestre Brancale, K.M. Beaver, & W.D. Bales., (eds.) *Advancing criminology and criminal justice policy* (pp. 78-88). Routledge.
- Laub, J. H, & Sampson, R. J. 1991. The Sutherland-Glueck Debate: On the sociology of criminological knowledge. *American Journal of Sociology*, 96: 1402–1440.
- Laudan, L. 1984. *Science and values: The aims of science and their role in scientific debate*. California UP.
- Leon, C. 2011. *Sex fiends, perverts, and pedophiles*. NYUP.
- Levin, B. 2018. The consensus myth in criminal justice reform. *Michigan Law Review*, 117(2): 259–318.
- Li, J., J.F. Burnham, T. Lemley, & R.M. Britton. 2010. Citation analysis: Comparison of Web of Science, Scopus, SciFinder, and Google Scholar. *Journal of Electronic Resources in Medical Libraries*, 7(3): 196–217.
- Lipset, S. M. 1969. Why cops hate liberals and vice versa, in *The Police and the Rest of Us: An Atlantic Supplement*. *The Atlantic Monthly*, 223(3): 76–83.
- Lipton, D. S., Martinson, R., & Wilks, J. 1975. *The effectiveness of correctional treatment: A survey of treatment evaluation studies*. Praeger.
- Loader, I., & R. Sparks. 2010. *Public criminology?* Routledge.
- Lum, C., & Koper, C. 2017. *Evidence-based policing: Translating research into practice*. Oxford UP.
- Lumet, S. 1957. *Twelve Angry Men*. United Artists.
- Lyons, W., & S. Scheingold. 2000. The politics of crime and punishment. *Criminal justice*, 1(1): 103–148.
- MacDonald, A. 1893. *Criminology*. Funk & Wagnalls.
- Mannheim, H. 1960. *Pioneers in criminology*. Quadrangle.
- Mannheim, K. 1929. *Ideologie und Utopie* [Ideology and utopia]. Cohen.
- Manski, C. 2007. *Identification for prediction and decision*. Harvard UP.
- Manski, C. 2013. *Public policy in an uncertain world: Analysis and decisions*. Harvard UP.
- Martini, C., & Boumans, M. (Eds.). 2014. *Experts and consensus in social science* (Vol. 50). Springer.
- Martinson, R. 1962, January 6. Prison notes of a Freedom Rider. *The Nation*.
- Martinson, R. 1974. What works? Questions and answers about prison reform. *The public interest*, 35: 22–54.
- Martinson, R. 1979. New findings, new views: A note of caution regarding sentencing reform. *Hofstra Law Review*, 7(2): 243–258.

- Mayeux, S. 2018. The idea of ‘the criminal justice system.’ *American Journal of Criminal Law*, 45(1): 55–94.
- McCord, J. 2003. Cures that harm: Unanticipated outcomes of crime prevention programs. *The Annals of the American Academy of Political and Social Science*, 587(1): 16–30.
- McLennan, R.M. 2008. *The crisis of imprisonment: Protest, politics, and the making of the American penal state, 1776–1941*. Cambridge UP.
- McPherson, C. M., & Sauder, M. 2013. Logics in action: Managing institutional complexity in a drug court. *Administrative Science Quarterly*, 58: 165–196.
- Meho, L.I., & K. Yang. 2007. Impact of data sources on citation counts and rankings of LIS faculty: Web of science versus Scopus and Google Scholar. *Journal of the American Society for Information Science and Technology*, 58(13): 2105–2125.
- Melo-Martín, I. de, & K. Intemann. 2014. Who’s afraid of dissent? Addressing concerns about undermining scientific consensus in public policy developments. *Perspectives on Science*, 22(4): 593–615.
- Menninger, K. 1968. *The crime of punishment*. Viking.
- Merton, R. K. 1938. Science, technology and society in seventeenth century England. *Osiris*, 4: 360–632.
- Merton, R. 1957. *Social theory and social structure: Toward the codification of theory and research*, rev. ed. Free Press
- Merton, R. K. 1942. A note on science and democracy. *Journal of Legal and Political Sociology*, 1: 115–127.
- Monk, R. C. 1988. Scientific research programs, theory and schools interregnum in criminology and criminal justice. *Journal of Contemporary Criminal Justice*, 4: 1–19.
- Moody, J., & R. Light. 2006. A view from above: The evolving sociological landscape. *American Sociologist*, 37(2): 67–86.
- Morn, F. 1995. *Academic politics and the history of criminal justice education*. Contributions in criminology and penology, 46. Greenwood.
- Morrell, K., Learmonth, M., & Heracleous, L. 2015. An archaeological critique of ‘evidence-based management’: One digression after another. *British Journal of Management*, 26(3): 529–543.
- Morris, A. 1975. The American Society of Criminology: A history, 1941-1974. *Criminology*, 13: 123–167.
- Morris, N. 1974. *The future of imprisonment*. Chicago UP.
- Mouffe, C. 2000. *The democratic paradox*. Verso.
- Mouffe, C. 2013. *Agonistics: Thinking the world politically*. Verso.
- Muhammad, K.G. 2010. *The condemnation of blackness: Race, crime, and the making of modern urban America*. Harvard UP.
- Murakawa, N. 2014. *The first civil right: How liberals built prison America*. Oxford UP.

- Murphy, J. 1986. The voice of memory: History, autobiography and oral memory. *Historical Studies*, 22: 157–175.
- Myrdal, G. 1969. *Objectivity in social research*. Random House.
- Nagin, D. S., & Weisburd, D. 2013. Evidence and public policy: The example of evaluation research in policing. *Criminology & Public Policy*, 12(4): 651–679.
- NAS (National Academies of Sciences, Engineering, and Medicine). 2005, revised ed. *Getting to know the committee process*. Washington, DC: National Academies Press. Accessed 16th May, 2019: <https://perma.cc/55WV-8RXY>.
- NAS (National Academies of Sciences, Engineering, and Medicine). 2019. *About the Committee on Law and Justice*. Accessed May 16th, 2019: <https://perma.cc/CUX8-YPBC>.
- Obama, B. H. 2017. The President's role in advancing criminal justice reform. *Harvard Law Review*, 130(3): 1–56.
- Ohlin, L., & Remington, F. J., (eds.). 1993. *Discretion in criminal justice: The tension between individualization and uniformity*. SUNY Press.
- Olds, D. L., Henderson, C. R., Chamberlin, R., & Tatelbaum, R. 1986. Preventing child abuse and neglect: A randomized trial of nurse home visitation. *Pediatrics*, 78(1): 65–78.
- Parmelee, M. 1918. *Criminology*. Macmillan.
- Pawson, R. 2006. *Evidence-based policy: A realist perspective*. Sage.
- Perlstein, R. 2008. *Nixonland: The rise of a president and the fracturing of America*. Scribner.
- Petersilia J. 2003. *When prisoners come home: Parole and prisoner reentry*. Oxford UP.
- Petersilia, J., & Turner, S. 1993. *Evaluating intensive supervision probation/parole: Results of a nationwide experiment*. US DOJ/NIJ.
- Petrosino, A., Boruch, R.F., Soydan, H., Duggan, L., & Sanchez-Meca, J. 2001. Meeting the challenges of evidence-based policy: The Campbell Collaboration. *Annals of the AAPSS*, 578: 14–34.
- Petty, E.A. 1959. Historical perspectives on the American Society of Criminology. Unpublished manuscript, available at: <https://perma.cc/45LZ-7RDM>, accessed 12 August, 2019.
- Phillips, C., & B. Bowling. 2003. Racism, ethnicity and criminology: Developing minority perspectives. *British Journal of Criminology*, 43(2): 269–290.
- Pielke, R., & Klein, R. A. (Eds.). 2010. *Presidential science advisors: Perspectives and reflections on science, policy and politics*. Springer.
- Pifferi, M. 2016. *Reinventing punishment. A comparative history of criminology and penology in the nineteenth and twentieth centuries*. Oxford UP.
- Pilkington, A., & C. Liston-Heyes. 1999. Is production and operations management a discipline? A citation/ co-citation study. *International Journal of Operations and Production Management*, 19(1): 7–20.
- Piquero, A.R., Farrington, D.P., & A. Blumstein. 2003. The criminal career paradigm. *Crime and Justice*, 30: 359–506.

- Platt, T. 1988. If we know, then we must fight: The origins of radical criminology in the U.S. *Critical Sociology*, 15: 127–138.
- Police Foundation. 1977. *The Cincinnati team policing experiment: A summary report*. Police Foundation.
- Police Foundation. 1981. *The Newark foot patrol experiment*. Police Foundation.
- Police Foundation. 1983. *Arresting shoplifters*. Police Foundation
- Poole, E.D., & R.M. Regoli. 1981. Periodical prestige in criminology and criminal justice: A comment. *Criminology*, 19(3): 470–478.
- Poovey, M. 1998. *A history of the modern fact: Problems of knowledge in the sciences of wealth and society*. Chicago UP.
- Pound, R. 1923/2018. *Criminal Justice in America*. Routledge.
- Pratt, J. 2007. *Penal populism*. Routledge.
- President's Commission on Law Enforcement and Administration of Justice. 1967a. *The challenge of crime in a free society*. US Government Printing Office.
- President's Commission on Law Enforcement and Administration of Justice. 1967b. *Task Force Report: The Police*. US Government Printing Office.
- President's Commission on Law Enforcement and Administration of Justice. 1967c. *Task Force Report: The Courts*. US Government Printing Office
- President's Commission on Law Enforcement and Administration of Justice. 1967d. *Task Force Report: Corrections*. US Government Printing Office.
- President's Commission on Law Enforcement and Administration of Justice. 1967e. *Task Force Report: Juvenile Delinquency and Youth Crime*. US Government Printing Office.
- President's Commission on Law Enforcement and Administration of Justice. 1967f. *Task Force Report: Science and Technology*. US Government Printing Office.
- President's Task Force on 21st Century Policing. 2015. *Final report of the President's task force on 21st century policing*. Office of Justice Programs.
- Quinney, R. 1974. *Critique of legal order: Crime control in capitalist society*. Little, Brown & Co.
- Quinney, R. 1979. The production of criminology. *Criminology*, 16: 445–458.
- Rabinow, P., & Rose, N. 2006. Biopower today. *BioSocieties*, 1(2): 195–217.
- Rafter, N. 2007. Somatotyping, antimodernism, and the production of criminological knowledge. *Criminology*, 45(4): 805–833.
- Rafter, Nicole. 2010. Silence and memory in criminology: The American Society of Criminology 2009 Sutherland Address. *Criminology*, 48(2): 339–355.
- Reiner, R. 1988. British criminology and the state. *British Journal of Criminology*, 28(2): 138–158.

- Reiner, R. 2002. The first cut is the deepest: Criminological texts and the return of the repressed. In I.K. McKenzie & R. Bull, (eds.), *Criminal justice research: Inspiration, influence and ideation*. Routledge. Pp. 3–29.
- Reiss, A. J. 1994. An evaluation and assessment of the impact of the Task Force Report: Crime and its Impact—An Assessment. In Conley, J.A., (ed.), *The 1967 President's Crime Commission Report: Its impact 25 years later*. Anderson. Pp. 1–20.
- Reiss, A. J. 1971. *The police and the public*. Yale UP.
- Rosenberg, A. 2012. *Philosophy of science: A contemporary introduction*, 3rd ed. Routledge.
- Rosenfeld, Seth. 2012. *Subversives: The FBI's war on student radicals and Reagan's rise to power*. New York, NY: Farrar, Straus and Giroux.
- Ross, Dorothy. 1991. *The origins of American social science*. Cambridge, UK: Cambridge University Press.
- Rice, S.K., K.J. Terry, H.V. Miller, & A. Ackerman. 2007. Research trajectories of female scholars in criminology and criminal justice. *Journal of Criminal Justice Education*, 18(3): 360–384.
- Rice, S.K., M.J. Hickman, & P. Reynolds. 2011. A preliminary assessment of small world scholarship networks in criminology and criminal justice. *Journal of Criminal Justice Education*, 22(1): 67–83.
- Rios, V. M. 2011. *Punished: Policing the lives of Black and Latino boys*. NYU Press.
- Rock, P., (ed.). 1988. *A history of British criminology*. Clarendon Press.
- Rock, P. 2005. Chronocentrism and British criminology. *British Journal of Sociology*, 56(3): 473–491.
- Rock, P. 2011. 'What have we done?' Trends in criminological theorising. *Acta Criminologica: Southern African Journal of Criminology*, 24(1): 19–43.
- Rosenbaum, D. P. 2006. The limits of hot spots policing. In Weisburd, D., & Braga, A., (eds.), *Police innovation: Contrasting perspectives*. Cambridge UP. Pp. 245–263
- Ross, D. *The origins of American social science*. Cambridge UP.
- Rothman, D. J. 2017. *Conscience and convenience: The asylum and its alternatives in progressive America*. Routledge.
- Rothschild-Elyassi, G., J. Koehler, & J. Simon. 2019. Actuarial justice. In M. Deflem, ed., *The Handbook of Social Control*. Wiley-Blackwell. Pp. 194–206.
- Rubin, A. 2019. *The deviant prison: Eastern State Penitentiary and the advantage of difference*. Cambridge UP.
- Rubin, A.T., & K. Reiter. 2018. Continuity in the face of penal innovation: revisiting the history of American solitary confinement. *Law & Social Inquiry*, 43(4): 1604–1632.
- Rushin, S. 2017. *Federal intervention in American police departments*. Cambridge UP.
- Sampson, R. J. 2010. Gold standard myths: Observations on the experimental turn in quantitative criminology. *Journal of quantitative criminology*, 26(4): 489–500.

- Sampson, R. J., Winship, C., & Knight, C. 2013. Translating causal claims: Principles and strategies for policy-relevant criminology. *Criminology & Public Policy*, 12(4): 587–616.
- Savelsberg, J.J. 1994. Knowledge, domination, and criminal punishment. *American Journal of Sociology*, 99(4): 911–943.
- Savelsberg, J.J., L.L. Cleveland, & R.D. King. 2004. Institutional environment and scholarly work: American criminology, 1951–1993. *Social Forces*, 82(4): 1275–1302.
- Savelsberg, J.J., & S. Flood. 2004. Criminological knowledge: Period and cohort effects in scholarship. *Criminology*, 42(4): 1009–1041.
- Savelsberg, J.J., & S. Flood. 2011. American criminology meets Collins: Global theory of intellectual change and a policy-oriented field. *Sociological Forum*, 26(1): 21–44.
- Savelsberg, J.J., & R.J. Sampson. 2002. Mutual engagement: Criminology and sociology? *Crime, Law and Social Change*, 37(2): 99–105.
- Schoenfeld, H. 2018. *Building the prison state: Race and the politics of mass incarceration*. Chicago UP.
- Scheingold, S. 1998. Constructing the new political criminology: Power, authority, and the post-liberal state. *Law & Social Inquiry*, 23(4): 857–895.
- Schwendinger, H., & Schwendinger, J. 1970. Defenders of order or guardians of human rights? *Issues in Criminology*, 5: 123–157.
- Schwendinger, H., & Schwendinger, J. 2014. *Who killed the Berkeley School? Struggles over radical criminology*. Thought Crimes Press.
- Selznick, P. *TVA and the grassroots*. University of California Press.
- Shank, G. 1999. Looking back: Radical criminology and social movements. *Social Justice*, 26: 114–134.
- Sherman, L. W. 2005. The use and usefulness of criminology, 1751-2005: Enlightened justice and its failures. *The Annals of the American Academy of Political and Social Science*, 600(1): 115–135.
- Sherman, L. W. 2009. Evidence and liberty: The promise of experimental criminology. *Criminology & Criminal Justice*, 9(1): 5–28.
- Sherman, L.W., & Cohn, E.G. 1989. The impact of research on legal policy: The Minneapolis Domestic Violence Experiment. *Law & Society Review*, 23(1): 117–144.
- Sherman, L. W., Gottfredson, D. C., MacKenzie, D. L., Eck, J., Reuter, P., & Bushway, S. 1997. *Preventing crime: What works, what doesn't, what's promising: A report to the United States Congress*. US DOJ.
- Sherman, L. W., Schmidt, J. D., & Rogan, D. P. 1992. *Policing domestic violence: Experiments and dilemmas*. Free Press.
- Sherman, L. W., & Weisburd, D. 1995. General deterrent effects of police patrol in crime ‘hot spots: A randomized, controlled trial. *Justice Quarterly*, 12(4): 625–648.
- Short, J. F., & Hughes, L. A. 2007. Criminology, criminologists, and the sociological enterprise. In *Sociology in America: The ASA centennial history*, ed. Craig C. Calhoun. Chicago UP.

- Shwed, U, & P. Bearman. 2010. The temporal structure of scientific consensus formation. *American Sociological Review*, 75(6): 817–840.
- Simon, J. 1993. *Poor discipline: Parole and the social control of the underclass, 1890–1990*. Chicago UP.
- Simon, J. 2007. *Governing through crime: How the war on crime transformed American democracy and created a culture of fear*. Oxford UP.
- Simon, J. 2010. Janus-faced leviathan: California’s prisons and universities as faces of state power. *Carceral Notebooks*, 6: 151–174.
- Sklansky, D. A. 2008. *Democracy and the police*. Stanford UP.
- Skogan, W. G. 2018. The commission and the police. *Criminology & Public Policy*, 17(2): 379–396.
- Skolnick, J. 1966. *Justice without trial: Law enforcement in a democratic society*. Wiley.
- Skolnick, J. 1969. *The politics of protest*. Simon & Schuster.
- Skolnick, J. 2012. Legacies of legal realism: The sociology of criminal law and criminal justice. *Annual Review of Law and Social Science*, 8: 1–10.
- Small, H. 1973. Co-citation in the scientific literature: A new measure of the relationship between two documents. *Journal of the American Society for Information Science*, 24(4): 265–269.
- Smelser, N. J. 2010. *Reflections on the University of California: From the Free Speech Movement to the global university*. California UP.
- Snodgrass, J. 1972. *The American criminological tradition: Portraits of the men and ideology in a discipline*. Unpublished doctoral dissertation, University of Pennsylvania.
- Sorensen, J. R., Patterson, A. L., & Widmayer, A. 1992. Publication productivity of faculty members in criminology and criminal justice doctoral programs. *Journal of Criminal Justice Education*, 3: 1–33.
- Sparrow, M.K. 2011. *Governing science: New perspectives in policing*. Washington, DC: Department of Justice, National Institute of Justice.
- Sparrow, M. K. 2016. *Handcuffed: What holds policing back, and the keys to reform*. Brookings Institution.
- Spohn, C. 2018. Reflections on the exercise of prosecutorial discretion 50 years after publication of *The Challenge of Crime in a Free Society*. *Criminology & Public Policy*, 17(2): 321–340.
- Suchman, M. C. 1995. Managing legitimacy: Strategic and institutional approaches. *Academy of Management Review*, 20(3): 571–610.
- Sunstein, C. R. 2014. *Why nudge? The politics of libertarian paternalism*. Yale UP.
- Sykes, G. 1974. The rise of critical criminology. *Journal of Criminal Law & Criminology*, 65(2): 206–213.
- Takagi, P. 1979. LEAA’s research solicitation: Police use of deadly force. *Crime and Social Justice*, 11: 51–61.

- Thacher, D. 2015. Perils of value neutrality. In *Institutions and Ideals: Philip Selznick's Legacy for Organizational Studies* (pp. 317-352). Emerald.
- Thacher, D. 2019. The aspiration of scientific policing. *Law & Social Inquiry*, 44(1): 273–297.
- Thacher, D., & Rein, M. 2004. Managing value conflict in public policy. *Governance*, 17(4): 457–486.
- Thaler, R. H. 2015. *Misbehaving: The making of behavioral economics*. WW Norton & Company.
- Thompson, H. A. 2017. *Blood in the water: The Attica prison uprising of 1971 and its legacy*. Vintage.
- Thornton, P. H., & Ocasio, W. 2008. Institutional logics. In *The Sage handbook of organizational institutionalism*, eds. Royston Greenwood, Christine Oliver, Roy Suddaby, and Kerstin Sahlin-Andersson. Sage.
- Thornton, P. H., Ocasio, W., & Lounsbury, M. 2012. *The institutional logics perspective: A new approach to culture, structure, and process*. Oxford UP.
- Timmermans, S., & Tavory, I. 2012. Theory construction in qualitative research: From grounded theory to abductive analysis. *Sociological Theory*, 30(3): 167–186.
- Toby, J. 1979. The new criminology is the old sentimentality. *Criminology*, 16: 516–526.
- Tomlins, C. 2000. Framing the field of law's disciplinary encounters: A historical narrative. *Law and Society Review*, 34: 911–972.
- Tonry, M. 1995. *Malign neglect: Race, crime, and punishment in America*. Oxford UP.
- Tonry, M. 2013. Evidence, Ideology, and politics in the making of American criminal justice policy. *Crime & Justice*, 42(1): 1–18.
- Tonry, M. 2018. The President's Commission and sentencing, then and now. *Criminology & Public Policy*, 17(2): 341–354.
- Townley, B. 1997. The institutional logic of performance appraisal. *Organization Studies*, 18: 261–285.
- Triplett, R., (ed.). 2017. *The handbook of the history and philosophy of criminology*. Wiley-Blackwell.
- Trujillo, C.M., & T.M. Long. 2018. Document co-citation analysis to enhance transdisciplinary research. *Science Advances*, 4(e1701130): 1–10.
- Turk, A. T. 1979. Analyzing official deviance: For nonpartisan conflict analyses in criminology. *Criminology*, 16: 459–476.
- University of California Bulletin. 1950. Announcement of the School of Criminology. Volume 45, 1.
- Vaughan, D. 2006. The social shaping of commission reports. *Sociological Forum*, 21(2): 291–306.
- Vaughan, D. 2016. *The Challenger launch decision: Risky technology, culture, and deviance at NASA*. Chicago UP.
- Vitale, A. S. 2017. *The end of policing*. Verso.

- Vollmer, A. 1930. The scientific policeman. *American Journal of Police Science*, 1: 8–12.
- Vollmer, A. 1933. Police progress in the past twenty-five years. *Journal of Criminal Law and Criminology*, 24: 161–175.
- Vollmer, A., & Parker, A. E. 1937. *Crime, Crooks and Cops*. Funk & Wagnalls.
- Vollmer, A., & Schneider, A. 1916. The school for police as planned at Berkeley. *Journal of the American Institute of Criminal Law and Criminology*, 7: 877–898.
- Wacquant, L. 2009. *Punishing the poor: The neoliberal government of social insecurity*. Duke UP.
- Wacquant, L. 2011. From ‘public criminology’ to the reflexive sociology of criminological production and consumption. *British Journal of Sociology*, 51(2): 438–448.
- Walker, S. 1993. *Taming the system: The control of discretion in criminal justice, 1950–1990*. Oxford UP.
- Walker, S. 1994. Between two worlds: The president’s crime commission and the police, 1967–1992. In J.A. Conley, (ed.), *The 1967 President’s Crime Commission Report: Its Impact 25 Years Later*. Anderson. Pp. 21–35.
- Weber, M. 1949. ‘Objectivity’ in social science and social policy. In E.A. Shils & H.A. Finch, (eds.), *Max Weber on the methodology of the social sciences*. Glencoe, IL: Free Press. pp.50-112.
- Weingart, S. 2015. Finding the history and philosophy of science. *Erkenntnis*, 80(1): 201–213.
- Weisburd, D. 2003. Ethical practice and evaluation of interventions in crime and justice: The moral imperative for randomized trials. *Evaluation Review*, 27(3): 336–354.
- Weisburd, D. 2015. The law of crime concentration and the criminology of place. *Criminology*, 53(2): 133–157.
- Weisburd, D., Groff, E. R., & Yang, S. M. 2012. *The criminology of place: Street segments and our understanding of the crime problem*. Oxford UP.
- Western, B., & C. Muller. 2013. Mass incarceration, macrosociology, and the poor. *Annals of the American Academy of Political and Social Science*, 647: 166–189.
- Wheeler, S. 1994. The commitment to social science: A case study of organizational innovation. In *Social science in the making: Essays on the Russell Sage Foundation, 1907 – 1972*, eds. David C. Hammack and Stanton Wheeler. Russell Sage.
- White, H.D., & K.W. McCain. 1998. Visualizing a discipline: An author co-citation analysis of information science, 1972–1995. *Journal of the American Society for Information Science*, 49(4): 327–355.
- White, S. O. 1972. A perspective on police professionalization. *Law and Society Review*, 7: 61–86.
- Wiebe, R. H. 1967. *The search for order, 1877-1920*. Hill & Wang.
- Willis, J.J., & S.D. Mastrofski. 2011. Innovations in policing: Meanings, structures, and processes. *Annual Review of Law and Social Science*, 7(1): 309–334.
- Willrich, M. 2003. *City of courts: Socializing justice in Progressive Era Chicago*. Cambridge UP.

- Wilson, J. Q. 1968. Dilemmas of police administration. *Public Administration Review*, 28(5): 407–417.
- Wilson, J. Q. 1978. *Varieties of police behavior*. Harvard UP.
- Wilson, J.Q. 1983. *Thinking about crime*, rev. ed. Basic.
- Wolfgang, M. E., Figlio, R. M., & Sellin, T. 1987. *Delinquency in a birth cohort*. Chicago UP.
- Wright, J.P., & M. DeLisi. 2016. *Conservative criminology: A call to restore balance in the social sciences*. Routledge.
- Wright, R.A. 2000. Recent changes in the most-cited scholars in criminology: A comparison of textbooks and journals. *Journal of Criminal Justice*, 28(2): 117–128.
- Wright, R.A., & D.O. Friedrichs. 2006. The most-cited scholars and works in critical criminology. *Journal of Criminal Justice Education*, 9(2): 211–231.
- Wright, R.A., M. Malia, & C.W. Johnson. 2012. Invisible influence: A citation analysis of crime and justice articles published in leading sociology journals. *Journal of Crime and Justice*, 22(2): 147–169.
- Wright, R.A., & J.M. Miller. 1998. The most-cited works and scholars in police studies. *Policing: An International Journal*, 21(2): 240–254.
- Wolfgang, M. E., Figlio, R. M., & Sellin, T. 1972. *Delinquency in a birth cohort*. Chicago UP.
- Wolfgang, M.E., R. Figlio, & T. Thornberry. 1978. *Evaluating criminology*. Elsevier.
- Xenakis, S., & L. Cheliotis. 2018. Whither neoliberal penalty? The past, present and future of imprisonment in the US. *Punishment & Society*, 21(2): 187–206.
- Xie, Y., & A. Killewald. 2012. *Is American science in decline?* Harvard UP.
- Young, J. 2011. *The criminological imagination*. Polity Press.
- Zalman, M. 1987. Sentencing in a free society: The failure of the President's Crime Commission to influence sentencing policy. *Justice Quarterly*, 4(4): 545–569.
- Zhang, Y. 2017. Scholarly influence in criminology and criminal justice of China. *Asian Journal of Criminology*, 12(2): 107–125.
- Zimring, F.E. 2006. *The great American crime decline*. Oxford UP.
- Zimring, F.E. 2011. *The city that became safe: New York's lessons for urban crime and its control*. Oxford UP.
- Zimring, F. E. 2013. The accidental crime commission: Its legacies and lessons. *Marquette Law Review*, 96(4): 995–1011.
- Zimring, F.E. 2017. *When police kill*. Harvard UP.
- Zimring, F.E. 2019. *The insidious momentum of mass incarceration*.
- Zimring, F. E., & Hawkins, G. 1993. *The scale of imprisonment*. Chicago UP.
- Zimring, F.E., & G. Hawkins. 1997. *Incapacitation: Penal confinement and the restraint of crime*. Oxford UP.

Zimring, F. E., Hawkins, G., & Kamin. S. 2001. *Punishment and democracy: Three strikes and you're out in California*. Oxford UP.

CASES CITED

Gideon v. Wainwright, 372 U.S. 335 (1963).

Gregg v. Georgia, 428 U.S. 153 (1976).

Mapp v. Ohio, 367 U.S. 643 (1961).

Lombardo v. Handler, 546 F.2d 1043 (D.C. Cir. 1976).

Miranda v. Arizona, 384 U.S. 436 (1966).

Mistretta v. United States, 488 U.S. 361 (1989).

Shaughnessy v. United States ex rel Mezei, 345 U.S. 206 (1953).

APPENDICES

APPENDIX 1.A: ID CODESHEET

Rank	Idea-Cluster Label	Foremost Within-Cluster Texts		
		Code	Value	
		1	2	3
1	OffRehab Offender	Andrews, Zinger, et al. (1990). Does correctional treatment work? A clinically relevant and psychologically informed meta-analysis. <i>Criminology</i> , 28(3), 369-404.	Andrews, D. A., & Bonta, J. (2014). <i>The psychology of criminal conduct</i> .	Gendreau, P., Little, T., & Goggin, C. (1996). A meta-analysis of the predictors of adult offender recidivism: What works!. <i>Criminology</i> , 34(4), 575-608.
2	FamViol Family violence	Straus, M. A., et al. (1996). The revised conflict tactics scales (CTS2) development and preliminary psychometric data. <i>Journal of family issues</i> , 17(3), 283-316.	Straus, M. A. (1979). Measuring Intrafamily Conflict and Violence: The Conflict Tactics (CT) Scales. <i>Journal of Marriage and the Family</i> , 41(1), 75-88.	Archer, J. (2000). Sex differences in aggression between heterosexual partners: a meta-analytic review. <i>Psychological bulletin</i> , 126(5), 651.
3	DSM Diagnostic Stat	American Psychiatry Association. (1994). Diagnostic and statistical manual of mental disorders (DSM-IV).	American Psychiatric Association, & American Psychiatric Association. (2000). Diagnostic and statistical manual of mental disorders (revised 4th ed.).	American Psychiatric Association. (2013). <i>Diagnostic and statistical manual of mental disorders (DSM-5®)</i> .
4	Concentrn Concentration	Cohen, L. E., & Felson, M. (1979). Social Change and Crime Rate Trends: A Routine Activity Approach. <i>American Sociological Review</i> , 44(4), 588-608.	Sherman, L. W., Gartin, P. R., & Buerger, M. E. (1989). Hot spots of predatory crime: Routine activities and the criminology of place. <i>Criminology</i> , 27(1), 27-56.	Hindelang, M. J., et al. (1978). <i>Victims of personal crime: An empirical foundation for a theory of personal victimization</i> . Cambridge, MA: Ballinger.
5	OffTheor Offending	Gottfredson, M. R., & Hirschi, T. (1990). <i>A general theory of crime</i> . Stanford UP.	Hirschi, T. (1969). <i>Causes of Delinquency</i> .	Sykes, G. M., & Matza, D. (1957). Techniques of neutralization: A theory of delinquency. <i>American sociological review</i> , 22(6), 664-670.
6	CritCrim Critical	David, G. (2001). The culture of control: Crime and social order in contemporary society.	Feeley, M. M., & Simon, J. (1992). The new penology: Notes on the emerging strategy of corrections and its implications. <i>Criminology</i> , 30(4), 449-474.	Garland, D. (1996). Limits of the Sovereign State: Strategies of Crime Control in Contemporary Society. <i>British Journal of Criminology</i> , 36(4), 445-471.

7	Policing	Policing	Wilson, J. Q. (1968). Varieties of Police Behavior.	Goldstein, H. (1990). Problem-oriented policing.	Reiss, A. J. (1971). The Police and the Public.
8	LifeCourse	Life-course	Moffitt, T. E. (1993). Adolescence-limited and life-course-persistent antisocial behavior: a developmental taxonomy. <i>Psychological review</i> , 100(4), 674-701.	Sampson, R. J., & Laub, J. H. (1993). Crime in the making: Pathways and turning points through life.	Wolfgang, M., Figlio, R., & Sellin, T. (1972). Delinquency in a Birth Cohort.
9	OffPatter	Offending	Sampson, R. J., Raudenbush, S. W., & Earls, F. (1997). Neighborhoods and violent crime: A multilevel study of collective efficacy. <i>Science</i> , 277(5328), 918-924.	Sampson, R. J., & Groves, W. B. (1989). Community structure and crime: Testing social-disorganization theory. <i>American journal of sociology</i> , 94(4), 774-802.	Shaw, C. R., & McKay, H. D. (1942). Juvenile delinquency and urban areas.
10	Strain	Strain theory	Agnew, R. (1992). Foundation for a general strain theory of crime and delinquency. <i>Criminology</i> , 30(1), 47-88.	Baron, & Kenny. (1986). The moderator–mediator variable distinction in social psychological research: Conceptual, strategic, and statistical considerations. <i>Journal of personality & social psychology</i> , 51(6), 1173.	Agnew, R. (2001). Building on the foundation of general strain theory: Specifying the types of strain most likely to lead to crime and delinquency. <i>Journal of research in crime and delinquency</i> , 38(4), 319-361.
11	RxSentenDis	Racial sentencing	Steffensmeier, D., et al. (1998). The interaction of race, gender, and age in criminal sentencing: The punishment cost of being young, black, and male. <i>Criminology</i> , 36(4), 763-798.	Quinney, R. (1970). <i>The social reality of crime</i> . Transaction publishers.	Albonetti, C. A. (1991). An integration of theories to explain judicial discretion. <i>Social Problems</i> , 38(2), 247-266.
12	PrisEthnog	Prison	Sykes, G. M. (1958). The society of captives: A study of a maximum security prison.	Irwin, J., & Cressey, D. R. (1962). Thieves, convicts and the inmate culture. <i>Social problems</i> , 10(2), 142-155.	Glaser, D. (1964). The effectiveness of a prison and parole system.
13	SxVictimzn	Sex victimization	Koss, et al. (1987). The scope of rape: incidence and prevalence of sexual aggression and victimization in a national sample of higher education students. <i>Journal of consulting and clinical psychology</i> , 55(2), 162.	Koss, M. P., & Oros, C. J. (1982). Sexual experiences survey: a research instrument investigating sexual aggression and victimization. <i>Journal of consulting and clinical psychology</i> , 50(3), 455.	Koss, et al. (1987). The scope of rape: incidence and prevalence of sexual aggression and victimization in a national sample of higher education students. <i>Journal of consulting and clinical psychology</i> , 55(2), 162.

14	ProcJust	Procedural justice	Sunshine, J., & Tyler, T. R. (2003). The role of procedural justice and legitimacy in shaping public support for policing. <i>Law & society review</i> , 37(3), 513-548.	Sampson & Bartusch. (1998). Legal cynicism and (subcultural?) tolerance of deviance: The neighborhood context of racial differences. <i>Law and society review</i> , 777-804.	Tyler, T. R. (1990). Why people obey the law.
15	YthGang	Youth gangs	Thornberry, T. P., et al. (2003). <i>Gangs and delinquency in developmental perspective</i> .	Decker, S. H., & Van Winkle, B. (1996). <i>Life in the gang: Family, friends, and violence</i> .	Esbensen, F. A., & Huizinga, D. (1993). Gangs, drugs, and delinquency in a survey of urban youth. <i>Criminology</i> , 31(4), 565-589.
16	Desist	Desistance	Laub, J. H. and Sampson, R., J. (2003). Shared beginnings, divergent lives. Delinquent boys to age 70.	Giordano, P. C., Cernkovich, S. A., & Rudolph, J. L. (2002). Gender, crime, and desistance: Toward a theory of cognitive transformation. <i>American journal of sociology</i> , 107(4), 990-1064.	Maruna, S. (2001). Making Good: How Ex-Convicts Reform and Rebuild Their Lives.
17	RapeMyth	Rape myths	Burt, M. R. (1980). Cultural myths and supports for rape. <i>Journal of personality and social psychology</i> , 38(2), 217.	Amir, Menachem. 1971. <i>Patterns in forcible rape</i> . Chicago: University of Chicago Press.	Lonsway, K. A., & Fitzgerald, L. F. (1994). Rape myths. In review. <i>Psychology of women quarterly</i> , 18(2), 133-164.
18	ReintShame	Reintegrativ	Braithwaite, J. (1989). <i>Crime, shame and reintegration</i> . Cambridge University Press.	Braithwaite, J. (1999). <i>Crime, shame and reintegration</i> . Cambridge University Press.	Christie, N. (1977). Conflicts as property. <i>The British journal of criminology</i> , 17(1), 1-15.
19	SxOffen	Sex offending	Hanson, R. K., & Bussiere, M. T. (1998). Predicting relapse: a meta-analysis of sexual offender recidivism studies. <i>Journal of consulting and clinical psychology</i> , 66(2), 348.	Hanson, R. K., & Morton-Bourgon, K. E. (2005). The characteristics of persistent sexual offenders: a meta-analysis of recidivism studies. <i>Journal of consulting and clinical psychology</i> , 73(6), 1154.	Finkelhor, D. (1984). Child sexual abuse. <i>New York</i> .
20	SelfContr	Self control	Gottfredson, M. R., & Hirschi, T. (1990). <i>A general theory of crime</i> .	Grasmick, H. G., et al. (1993). Testing the core empirical implications of Gottfredson & Hirschi's general theory. <i>Journal of research in crime & delinquency</i> , 30(1), 5-29.	Pratt, T. C., & Cullen, F. T. (2000). The empirical status of Gottfredson and Hirschi's general theory of crime: A meta-analysis. <i>Criminology</i> , 38(3), 931-964.
21	Deter	Deterrence	Becker, G. S. (1968). Crime and punishment: an economic approach. <i>Journal of political economy</i> , 76(2), 169-217.	Nagin, D. S., & Paternoster, R. (1993). Enduring individual differences and rational choice theories of crime. <i>Law and Society Review</i> , 467-496.	Packer, H. L. (1968). The Limits Criminal of Criminal Sanctions.

22	BullyAggr	Bullying &	Nansel, et al. (2001). Bullying behaviors among US youth: Prevalence and association with psychosocial adjustment. <i>Jama</i> , 285(16), 2094-2100.	Olweus, D. (1993). Bullying at school: What we know and what can we do. <i>Malden, MA: Blackwell</i> .	Crick, N. R., & Grotpeter, J. K. (1995). Relational aggression, gender, and social-psychological adjustment. <i>Child development</i> , 710-722.
23	COStress	Correctional	Cullen, F. T., et al. (1985). The social dimensions of correctional officer stress. <i>Justice Quarterly</i> , 2(4), 505-533.	Toch, H., & Klofas, J. (1982). Alienation and desire for job enrichment among correction officers. <i>Fed. Probation</i> , 46, 35.	Cheek, F., & Miller, M. (1983). The experience of stress for correction officers: A double-bind theory of correctional stress. <i>Journal of Criminal Justice</i> , 11(2), 105-120.
24	FearCrime	Fear of crime	Skogan, W. G., & Maxfield, M. G. (1981). Coping with crime: Individual and neighborhood differences. <i>Beverly Hills: Sage</i> .	Hale, C. (1996). Fear of crime: A review of the literature. <i>International review of Victimology</i> , 4(2), 79-150.	Ferraro, K. F., & Grange, R. L. (1987). The measurement of fear of crime. <i>Sociological inquiry</i> , 57(1), 70-97.
25	PubOpin	Public opinion	Cullen, F. T., Fisher, B. S., & Applegate, B. K. (2000). Public opinion about punishment and corrections. <i>Crime and justice</i> , 27, 1-79.	Tyler, T., & Boeckmann, R. (1997). Three strikes and you are out, but why? The psychology of public support for punishing rule breakers. <i>Law and Society Review</i> , 237-265.	Cullen, et al. (1985). Attribution, salience, and attitudes toward criminal sanctioning. <i>Criminal Justice and Behavior</i> , 12(3), 305-331.
26	OrgnzdCri	Organized	Morselli, C. (2009). <i>Inside criminal networks</i> (Vol. 8). New York: Springer.	Granovetter, M. (1985). Economic action and social structure: The problem of embeddedness. <i>American journal of sociology</i> , 91(3), 481-510.	Cressey, D. R. (1969). Theft of a Nation: The Structure and Operation of Organized Crime in the United States.
27	Aggressn	Aggression	Crick, N. R., & Dodge, K. A. (1994). A review and reformulation of social information-processing mechanisms in children's social adjustment. <i>Psychological bulletin</i> , 115(1), 74.	Anderson, C. A., & Bushman, B. J. (2002). Human Aggression. <i>Annual Review of Psychology</i> , 53, 27-51.	Berkowitz, L. (1993). <i>Aggression: Its causes, consequences, and control</i> . McGraw-Hill Book Company.
28	Deceptn	Deception	DePaulo, B. M., Lindsay, J. J., Malone, B. E., Muhlenbruck, L., Charlton, K., & Cooper, H. (2003). Cues to deception. <i>Psychological bulletin</i> , 129(1), 74.	Vrij, A. (2008). <i>Detecting lies and deceit: Pitfalls and opportunities</i> . John Wiley & Sons.	Bond Jr, C. F., & DePaulo, B. M. (2006). Accuracy of deception judgments. <i>Personality and social psychology Review</i> , 10(3), 214-234.
29	KidTestimon	Child Testimony	Ceci, S. J., & Bruck, M. (1993). Suggestibility of the child witness: A historical review and synthesis. <i>Psychological bulletin</i> , 113(3), 403.	Orbach, et al. (2000). Assessing the value of structured protocols for forensic interviews of alleged child abuse victims. <i>Child abuse & neglect</i> , 24(6), 733-752.	Powell, M. B., Fisher, R. P., & Wright, R. (2005). Investigative interviewing. <i>Psychology and law: An empirical perspective</i> , 11-42.

30	CopStress	Police officer	Lazarus, R. S., & Folkman, S. (1984). Stress, appraisal, and coping.	Derogatis, L. R., & Melisaratos, N. (1983). The brief symptom inventory: an introductory report. <i>Psychological medicine</i> , 13(3), 595-605.	Brown, J. M., & Campbell, E. A. (1990). Sources of occupational stress in the police. <i>Work & stress</i> , 4(4), 305-318.
31	IntlHomic	Comparative	Messner & Rosenfeld. (1997). Political restraint of the market and levels of criminal homicide: A cross-national application of institutional-anomie theory. <i>Social Forces</i> , 75(4), 1393-1416.	Gartner, R. (1990). The victims of homicide: A temporal and cross-national comparison. <i>American sociological review</i> , 92-106.	Pratt, T. C., & Godsey, T. W. (2003). Social support, inequality, and homicide: A cross-national test of an integrated theoretical model. <i>Criminology</i> , 41(3), 611-644.
32	PrivPolic	Private	Bayley, D. H., & Shearing, C. D. (1996). The future of policing. <i>Law and society review</i> , 585-606.	Johnston, L., & Shearing, C. (2003). Governing security: explorations in policing and justice.	Loader, I. (2000). Plural policing and democratic governance. <i>Social & legal studies</i> , 9(3), 323-345.
33	WhiteColl	White collar	Sutherland, E. (1949). <i>White Collar Crime</i> .	Sutherland, E. H. (1940). White-collar criminality. <i>American Sociological Review</i> , 5(1), 13-18.	Clinard, M., & Yeager, P. (1980). Corporate crime: The first comprehensive account of illegal practices among America's top corporations.
34	GreenCrim	Green	White, R. (2011). Transnational Environmental Crime: Toward an eco-global criminology.	Hillyard, P. (2004). <i>Beyond criminology: Taking harm seriously</i> . London: Pluto Press; Black Point, NS: Fernwood Pub.	Lynch & Stretsky (2003). The meaning of green: Contrasting criminological perspectives. <i>Theoretical Criminology</i> , 7(2), 217-238.
35	SxHomic	Sex offender	Ressler, R. K., Burgess, A. W., & Douglas, J. E. (1988). <i>Sexual homicide: Patterns and motives</i> . Simon and Schuster.	Douglas, J. E., Burgess, A. W., Burgess, A. G., & Ressler, R. K. (Eds.). (1992). <i>Crime Classification Manual: A Standard System for Investigating and Classifying Violent Crimes</i> .	Groth, A. N., Burgess, W., & Holmstrom, L. L. (1977). Rape: power, anger, and sexuality. <i>The American journal of psychiatry</i> , 134(11), 1239-1243.
36	Eyewitness	Eyewitness	Wells et al.(1998). Eyewitness identification procedures: Recommendations for lineups and photospreads. <i>Law and Human behavior</i> , 22(6), 603.	Sporer et al.(1995). Choosing, confidence, and accuracy: A meta-analysis of the confidence-accuracy relation in eyewitness identification studies. <i>Psychological Bulletin</i> , 118(3), 315.	Cutler, B. L., & Penrod, S. D. (1995). <i>Mistaken identification: The eyewitness, psychology and the law</i> . Cambridge University Press.
37	SxRiskAss	Sex offender	Hanson, R. K., & Thornton, D. (2000). Improving risk assessments for sex offenders: A comparison of three actuarial scales. <i>Law and Human behavior</i> , 24(1), 119-136.	Hanson, R. K., & Morton-Bourgon, K. E. (2009). The accuracy of recidivism risk assessments for sexual offenders: a meta-analysis of 118 prediction studies. <i>Psychological assessment</i> , 21(1), 1.	Barbaree, et al. (2001). Evaluating the predictive accuracy of six risk assessment instruments for adult sex offenders. <i>Criminal Justice and Behavior</i> , 28(4), 490-521.

38	JuvCourt	Juvenile court	Schur, E. M., & Maher, V. (1973). <i>Radical nonintervention: Rethinking the delinquency problem</i> . Englewood Cliffs, NJ: Prentice-Hall.	Cicourel A., 1968, THE SOCIAL ORGANIZATION OF JUVENILE JUSTICE.	Dannefer & Schutt. (1982). Race and juvenile justice processing in court and police agencies. <i>American J of Sociology</i> , 87(5), 1113-1132.
39	Stalking	Stalking	Tjaden, P. G., & Thoennes, N. (1998). Stalking in America: Findings from the national violence against women survey.	Mustaine, E. E., & Tewksbury, R. (1999). A routine activity theory explanation for women's stalking victimizations. <i>Violence Against Women</i> , 5(1), 43-62.	Pathé, M., & Mullen, P. E. (1997). The impact of stalkers on their victims. <i>The British Journal of Psychiatry</i> , 170(1), 12-17.
40	MigratnCrim	Migration & crime	Aas, K. F. (2011). 'Crimmigrant' bodies and bona fide travelers: Surveillance, citizenship and global governance. <i>Theoretical criminology</i> , 15(3), 331-346.	Stumpf, J. (2006). The The Crimmigration Crisis: Immigrants, Crime, and Sovereign Power. <i>Am. UL Rev.</i> , 56, 367.	Bosworth, M. (2012). Subjectivity and identity in detention: Punishment and society in a global age. <i>Theoretical Criminology</i> , 16(2), 123-140.
41	TeenSxOff	Adolescent sex	Seto, M., & Lalumière, M. (2010). What is so special about male adolescent sexual offending? A review and test of explanations through meta-analysis. <i>Psychological bulletin</i> , 136(4), 526.	Worling & Curwen. (2000). Adolescent sexual offender recidivism: success of specialized treatment and implications for risk prediction1. <i>Child abuse & neglect</i> , 24(7), 965-982.	Fehrenbach et al.(1986). Adolescent sexual offenders: Offender and offense characteristics. <i>American journal of Orthopsychiatry</i> , 56(2), 225-233.
42	CogniDistor	Cognitive	Marshall, W. L., Anderson, D., Fernandez, Y., & Mulloy, R. (1999). <i>Cognitive behavioural treatment of sexual offenders</i> .	Maruna, S., & Mann, R. E. (2006). A fundamental attribution error? Rethinking cognitive distortions. <i>Legal and Criminological Psychology</i> , 11(2), 155-177.	Ward, T., & Keenan, T. (1999). Child molesters' implicit theories. <i>Journal of interpersonal violence</i> , 14(8), 821-838.
43	KidSxAbuse	Child sex abuse	Browne, A., & Finkelhor, D. (1986). Impact of child sexual abuse: A review of the research. <i>Psychological bulletin</i> , 99(1), 66.	Kendall-Tackett, et al. (1993). Impact of sexual abuse on children: a review and synthesis of recent empirical studies. <i>Psychological bulletin</i> , 113(1), 164.	Finkelhor, D. (1979). Sexually Victimized Children.
44	TrustInCops	Trust in policing	Reisig, & Parks. (2000). Experience, quality of life, and neighborhood context: A hierarchical analysis of satisfaction with police. <i>Justice quarterly</i> , 17(3), 607-630.	Cao, L., Frank, J., & Cullen, F. T. (1996). Race, community context and confidence in the police. <i>American journal of police</i> , 15(1), 3-22.	Brandl, S. G., et al. (1994). Global and specific attitudes toward the police: Disentangling the relationship. <i>Justice quarterly</i> , 11(1), 119-134.

45	SxORRegistry	Sex offender	Tewksbury, R. (2005). Collateral consequences of sex offender registration. <i>Journal of Contemporary Criminal Justice</i> , 21(1), 67-81.	Levenson, J. S., & Cotter, L. P. (2005). The effect of Megan's Law on sex offender reintegration. <i>Journal of Contemporary Criminal Justice</i> , 21(1), 49-66.	Levenson & Cotter 2005. The impact of sex offender residence restrictions: 1,000 feet from danger or one step from absurd?. <i>Int'nl J Offender Therapy & Comparative Criminology</i> , 49(2), 168-178.
46	Profiling	Actuarial	Salfati, C. G. (2000). The nature of expressiveness and instrumentality in homicide: Implications for offender profiling. <i>Homicide Studies</i> , 4(3), 265-293.	Canter, D. (2000). Offender profiling and criminal differentiation. <i>Legal and Criminological Psychology</i> , 5(1), 23-46.	Canter, et al. (2004). The organized/disorganized typology of serial murder: Myth or model?. <i>Psychology, Public Policy, and Law</i> , 10(3), 293.
47	YouthPTSD	Youth	Teplin, et al. (2002). Psychiatric disorders in youth in juvenile detention. <i>Archives of general psychiatry</i> , 59(12), 1133-1143.	Abram, et al. (2004). Posttraumatic stress disorder and trauma in youth in juvenile detention. <i>Archives of general psychiatry</i> , 61(4), 403-410.	Abram, et al. (2003). Comorbid psychiatric disorders in youth in juvenile detention. <i>Archives of general psychiatry</i> , 60(11), 1097-1108.
48	PolicingDV	Policing domestic	Sherman, L., & Berk, R. (1984). The specific deterrent effects of arrest for domestic assault. <i>American sociological review</i> , 261-272.	Sherman, et al. (1992). Crime, punishment, and stake in conformity: Legal and informal control of domestic violence. <i>American Sociological Review</i> , 680-690.	Sherman, L. W., Schmidt, J. D., & Rogan, D. P. (1992). <i>Policing domestic violence: Experiments and dilemmas</i> .
49	AdolOff	Adolescent	Sickmund, M., & Snyder, H. N. (1999). Juvenile offenders and victims: 1999 national report.	Snyder, H. N., & Sickmund, M. (2006). Juvenile offenders and victims: 2006 national report. <i>Office of juvenile justice and delinquency prevention</i> .	Cauffman, E., & Steinberg, L. (2000). (Im) maturity of judgment in adolescence: Why adolescents may be less culpable than adults. <i>Behavioral sciences & the law</i> , 18(6), 741-760.
50	Interrogatn	Interrogatio	Milne, B., & Bull, R. (1999). Investigative interviewing: psychology and practice.	Gudjonsson, G. H. (2003). <i>The psychology of interrogations and confessions: A handbook</i> .	Leo, R. A. (1995). Inside the interrogation room. <i>J. Crim. L. & Criminology</i> , 86, 266.

APPENDIX 2.A ORAL HISTORY SOURCE MATERIALS

Citations to interviews are denoted throughout the text by square brackets. The corresponding identifiers are provided below. A brief biographical sketch to clarify the interviewees' connection to the School's history appears online as supplementary material. The article draws from two sets of oral histories: The first set comprises primary interviews conducted by the author specifically for the purposes of this research; the second set represents secondary interviews conducted as part of ongoing efforts to archive the histories of criminology, law and society, or the University of California.

Primary Oral History Sources

[Blomberg, 2013] Blomberg, Thomas G. November 22nd, 2013.

Blomberg was an undergraduate student in the sociology department at Berkeley from 1965 to 1969. He then enrolled in the doctoral program at the Criminology School under the supervision of Sheldon Messinger, where he self-identified as a member of the law and society scholars. He graduated in 1973.

[Currie, 2013] Currie, Elliott. February 22nd, 2013.

Currie joined the Center for the Study of Law and Society in 1967 while still a doctoral student in Berkeley's sociology department. He co-taught courses in the Criminology School. Although he self-identified principally as a radical, he maintained ties with the law and society scholars throughout the final years of the School.

[Kadish, 2013] Kadish, Sanford. January 30th, 2013.

Kadish was Dean of the Law School at Berkeley from 1975 to 1982. He co-founded the Jurisprudence and Social Policy Program housed at Boalt Hall once the School of Criminology was disestablished, and was instrumental in recruiting many of the School's former faculty into the new program.

[Krisberg, 2013] Krisberg, Barry. January 23rd, 2013.

Krisberg was hired to a full-time position as an Assistant Professor within the School's faculty in 1971, with the purpose of teaching methodology classes. He remained at the School until its disestablishment.

[Kruttschnitt, 2013] Kruttschnitt, Candace. November 21st, 2013.

Kruttschnitt transferred into the undergraduate program at the School from UCLA, and graduated in 1973. Upon graduating with a Bachelor's from the School, She left Berkeley to pursue doctoral study at Yale.

[Mahabir, 2014] Mahabir, Cynthia. April 17th, 2014.

Mahabir was a member of one of the School's final cohorts of doctoral students, and she graduated in 1978. Barry Krisberg chaired her dissertation, but she never identified as a radical.

[Omi, 2014] Omi, Michael. April 7th, 2014.

Omi was an undergraduate at Berkeley between 1969 and 1973, and he graduated with a B.A. in Sociology. After arriving as a self-identified “apolitical” student, Omi engaged in activism off-campus through the School, and he became a radical by the time he graduated.

[Platt, 2013] Platt, Tony M. January 29th, 2013.

Platt arrived at the School to pursue doctoral study, and graduated in 1966. After holding a post-doctoral position at the University of Chicago, Platt returned to Berkeley as an Assistant Professor at the School in 1968 until he was denied tenure under calamitous and highly public circumstances in 1976.

[Skolnick, 2013] Skolnick, Jerome. January 31st, 2013.

Skolnick came to the Criminology School in 1970 from the Sociology department. He remained at the School until its disestablishment in 1976, at which point he moved to the Law School to join the new program in Jurisprudence and Social Policy.

[Speiglmán, 2014] Speiglmán, Richard. April 5th, 2014.

Speiglmán arrived in Berkeley as a Research Associate at the Center for the Study of Law and Society in 1968, and enrolled in doctoral study in 1971. He completed the D.Crim. in 1976, under the supervision of Herman Schwendinger.

Secondary Oral History Sources

[Bowker, 1991] Bowker, Albert. Interviewed by Harriet Nathan for the Regional Oral History Office’s University History Series, Bancroft Archives, UC Berkeley. 1991, date omitted.

Bowker was the Chancellor of UC Berkeley from 1971-1980, and presided over the dissolution of the School of Criminology.

[Frederick, 1984] Frederick, Walter. Interviewed by Suzanne Riess for the Regional Oral History Office, Bancroft Archives, UC Berkeley. 1984, date omitted.

Frederick was a publicist with close ties to the University administration throughout the latter half of the twentieth century.

[Heyns, 1986] Heyns, Roger W. Interviewed by Harriet Nathan for the Regional Oral History Office’s University History Series, Bancroft Archives, UC Berkeley. 1986, date omitted.

Heyns was the Chancellor of UC Berkeley from 1965-1971, during the People’s Park protests, the conclusion of Lohman’s tenure at the School, and its burgeoning profile as a center of radical criminology.

[Holstrom, 1971] Holstrom, John. Interviewed by Jane Howard for the Regional Oral History Office’s August Vollmer Historical Project, Bancroft Archives, UC Berkeley. June 29th, 1971.

Holstrom served as the Chief of the Berkeley Police Department from 1944 to 1960. This position was attached to a faculty appointment, by courtesy, to UC Berkeley’s Political Science department and then the School of Criminology.

[Kadish, 2012] Kadish, Sanford. Interview conducted by David Lieberman for the Center for the Study of Law and Society's Conversations in Law and Society. October 5th, 2012.

See biographical sketch above.

[MacNamara, 1996] Macnamara, Donal. Interviewed by Barbara Raffle Price for the American Society of Criminology Oral History Archives Project. March 15th, 1996.

MacNamara was invited by O.W. Wilson to Berkeley to meet with Vollmer to discuss the prospects of changing the Association of Police Science Instructors into a broader criminological and criminal justice society. MacNamara served as president of the resultant American Society of Criminology from 1960 to 1963.

[Morris, 1996] Morris, Norval. Interviewed by Michael Tonry for the American Society of Criminology Oral History Archives Project. November 21st, 1996.

Morris was invited to Berkeley in 1971 to interview for the position of Dean of the School of Criminology. He was met with opprobrium from the radical students for a perceived centrism in his scholarship, and he subsequently withdrew his candidacy.

[Nader, 2011] Nader, Laura. Interviewed by Calvin Morrill for the Center for the Study of Law and Society Society's Conversations in Law and Society. March 11th, 2011.

Nader arrived at Berkeley in 1960 to teach in the anthropology department. She was a member of the 1975 Selznick committee that proposed displacing the Criminology School to a new program housed within the Law School, although she dissented with the committee's proposal.

[Skolnick, 2012] Skolnick, Jerome. Interviewed by Calvin Morrill for the Center for the Study of Law and Society Society's Conversations in Law and Society. February 10th, 2012.

See biographical sketch above.

[Sproul, 1984] Sproul, Robert Gordon. Interviewed by Suzanne Riess for the Regional Oral History Office, Bancroft Archives, UC Berkeley. 1984, date omitted.

Sproul served as President of the University of California from 1930 to 1958, and was responsible for the inauguration of the School of Criminology.

[Wilson, 1972] Wilson, Orlando W. Interviewed by Jane Howard Robinson and John Holstrom for the Regional Oral History Office's August Vollmer Historical Project, Bancroft Archives, UC Berkeley. July 2nd, 1972.

Wilson was hired as a Professor of Police Administration at Berkeley in 1939. He was the School of Criminology's first Dean, from 1950 to 1960.

[Wolfgang, 1997] Wolfgang, Marvin. Interviewed by Freda Adler for the American Society of Criminology Oral History Archives Project. November 14th, 1997.

Wolfgang chaired the 1971 external committee tasked with evaluating the School's research profile. It reported favorably on the School's intellectual contributions.

APPENDIX 2.B ARCHIVAL SOURCE MATERIALS

Citations to archival sources are denoted throughout this article by square brackets. Such documents are filed irregularly, which necessitated devising an imperfect reference scheme. Where available, the Bancroft Archive Call Number appears next to the corresponding identifier. However, some sources are not accessible in this manner, or they have not been assigned a Call Number. Readers are invited to contact the author for further direction, if necessary.

[Letter#1] *Letter to Richard McGee, Chairman of the Advisory Council of the School of Criminology, Dept. of Corrections, from Herbert Blumer.* November 16, 1960. CU33 1:27

[Letter#2] *Letter to students and alumni of the School of Criminology from Dean Sheldon Messinger.* August 21, 1973. CU33 1:28

[Letter#3] *Letter to Herbert Blumer from Joseph Lohman.* September 9, 1960. CU33 1:27

[Memo#1] *Memorandum: to Chancellor Clark Kerr, re: Provision for Increased Criminological Research. Letter to Doctor Jenkins, Chairman of the Subcommittee on Educational Policy.* February 20, 1958. CU33 1:25

[Memo#2] *Memorandum to Professors Kelley, Kirk, MacCormick, and O'Neill, from O.W. Wilson, concerning Departmental Expectations.* Circa 1955. CU33 1:30

[Memo#3] *Memorandum to Vice Chancellor Kerr from Arthur Sherry, Acting Dean of the School of Criminology about Approval of the Recommendations of the Committee on the Reorganization of the Instructional Program of the School of Criminology.* January 31, 1961

[Memo#4] *Memorandum to students and staff of the Institute of Criminology, University of Cambridge, from Sheldon Messinger.* January 29, 1975. Radzinowicz Library

[Minutes#1] *Minutes of the Academic Advisory Committee Conference on the Schools of Criminology, Law, and Social Welfare.* February 27, 1957. CU33 1:25

[Minutes#2] *Minutes of Meeting of Academic Advisory Council.* October 27, 1959. CU33 1:25

[Pamphlet#1] *Careers in Criminology.* Pamphlet prepared by the Student Subcommittee on Careers of the Student-Faculty Educational Committee of the School of Criminology. 1968. CU33 4: undocumented folder.

[Pamphlet#2] *Newsletter of the Union of Radical Criminologists, vol. 1, no. 1: Statement of principles.* 1973. Platt Papers.

[Pamphlet#3] *Defend the Criminology School newsletter.* Committee to Save Criminology. Platt Papers.

[Report#1] *Report of the first meeting of the National Association of College Police Training Officials for the purpose of furthering college police training and standardizing police training curricula.* December 30, 1941. Available at:

http://www.asc41.com/Board_Minutes/1941/BoardMin_Dec_30_1941.pdf, accessed February 3rd, 2013.

[Report#2] *Reports of the School of Criminology.* The folder is filled with miscellany relating to the period before the School's establishment, including letters written by professionals from

throughout the country to the University administration in support of the School's establishment. CU33 1:25

[Report#3] *Report of Lynch Sub-Committee of Graduate Council Appointed to Study the proposed doctor of criminology degree.* March 17, 1954. CU33 1:25

[Report#4] *Social Science Departmental Expectations Summary.* April 1955.

[Monograph#1] Graves, Richard P. (1932) *The operating cost of the administration of criminal justice in seven California counties.* Unpublished Master's thesis, UC Berkeley.

[Monograph#2] Wise, Paul G. (1933) *Defects in the administration of criminal justice with particular reference to murder.* Unpublished Master's thesis, UC Berkeley.

[Monograph#3] Lohman, Joseph D. (1966) *An Experimental High School Project in Cultural Diversity.* In cooperation with the Oakland Public School System. CU33 4:1

[Monograph#4] Authorship undefined. (1966) *An Assessment of Training Programs in Public Agencies Dealing with Juvenile Delinquency.* Dealing with agencies throughout the Bay Area. CU33 4:2

[Monograph#5] Lohman, Joseph D., R. Carter, and Paul Takagi. (1966) *A Training Program for Agents of the Bureau of Drug Abuse Control.* In cooperation with the Bureau of Drug Abuse Control, US Food and Drug Administration, and Department of Health, Education, and Welfare. CU33 4:5

[Monograph#6] Lohman, Joseph D. (1967) *A Manual for Teachers, Counselors, and Administrators.* CU33 4:8

[Monograph#7] Parker, B., and J. Peterson. (1970) *Physical Evidence Utilization in the Administration of Criminal Justice.* Assisted by the Berkeley Police Department. CU33 4:13

[Monograph#8] Smith, Ronald. (1961), *An evaluation of programming methods for the distribution of police manpower.* Unpublished Master's thesis, UC Berkeley.

[Monograph#9] Platt, Anthony. (1965), *The criminal responsibility of the mentally ill in England 1100-1843.* Unpublished Master's thesis, UC Berkeley.

[Monograph#10] Powell, Gary. (1966), *Violence: A function of social isolation.* UC Berkeley.

APPENDIX 3.A: CONSENSUS STUDY REPORTS BY THE COMMITTEE ON LAW AND JUSTICE

Italicized titles were excluded from the sampling frame.

- 2018 Modernizing Crime Statistics, vol. II: New Systems for Measuring Crime
- 2018 Proactive Policing: Effects on Crime and Communities
- 2016 *Review of Proposals for Research on Statistical Methodologies for Assessing Variables in Eyewitness Performance*
- 2016 Preventing Bullying Through Science, Policy, and Practice
- 2016 Modernizing Crime Statistics, vol. I: Defining and Classifying Crime
- 2015 *Support for Forensic Science Research: Improving the Scientific Role of the National Institute of Justice*
- 2015 Understanding the U.S. Illicit Tobacco Market: Characteristics, Policy Context, and Lessons from International Experiences
- 2014 *Identifying the Culprit: Assessing Eyewitness Identification*
- 2014 Implementing Juvenile Justice Reform: The Federal Role
- 2014 *Science Needs for Microbial Forensics: Developing Initial International Research Priorities*
- 2014 The Growth of Incarceration in the United States: Exploring Causes and Consequences
- 2014 Estimating the Incidence of Rape and Sexual Assault
- 2013 Confronting Commercial Sexual Exploitation and Sex Trafficking of Minors in the United States
- 2013 Priorities for Research to Reduce the Threat of Firearm-Related Violence
- 2013 Reforming Juvenile Justice: A Developmental Approach
- 2012 Deterrence and the Death Penalty
- 2011 Budgeting for Immigration Enforcement: A Path to Better Performance
- 2011 *Reference Manual on Scientific Evidence: Third Edition*
- 2010 Strengthening the National Institute of Justice
- 2010 Understanding the Demand for Illegal Drugs
- 2009 Ensuring the Quality, Credibility, and Relevance of U.S. Justice Statistics
- 2009 *Strengthening Forensic Science in the United States: A Path Forward*
- 2009 *Evaluation of the Reference Manual on Scientific Evidence: Letter Report*
- 2008 *Protecting Individual Privacy in the Struggle Against Terrorists: A Framework for Program Assessment*
- 2008 *Ballistic Imaging*

- 2008 Surveying Victims: Options for Conducting the National Crime Victimization Survey
- 2008 Parole, Desistance from Crime, and Community Integration
- 2007 *Engaging Privacy and Information Technology in a Digital Age*
- 2005 Improving Evaluation of Anticrime Programs
- 2005 Firearms and Violence: A Critical Review
- 2004 Fairness and Effectiveness in Policing: The Evidence
- 2004 *Forensic Analysis: Weighing Bullet Lead Evidence*
- 2003 *Guatemala: Human Rights and the Myrna Mack Case*
- 2003 *The Polygraph and Lie Detection*
- 2001 Informing America's Policy on Illegal Drugs: What We Don't Know Keeps Hurting Us
- 2001 Juvenile Crime, Juvenile Justice
- 2001 *Issues for Science and Engineering Researchers in the Digital Age*
- 2000 *LC21: A Digital Strategy for the Library of Congress*
- 2000 *The Digital Dilemma: Intellectual Property in the Information Age*
- 1996 *The Evaluation of Forensic DNA Evidence*
- 1994 Violence in Urban America: Mobilizing a Response
- 1986 Criminal Careers and "Career Criminals," vol. I
- 1986 Criminal Careers and "Career Criminals," vol. II
- 1983 Research on Sentencing: The Search for Reform, vol. I
- 1983 Research on Sentencing: The Search for Reform, vol. II
- 1982 *Report of the Committee on Ballistic Acoustics*
- 1978 Deterrence and Incapacitation: Estimating the Effects of Criminal Sanctions on Crime Rates
- 1977 Understanding Crime: An Evaluation of the National Institute of Law Enforcement and Criminal Justice

APPENDIX 3.B: CONSENSUS STUDY REPORT INTERVIEWEES

Each interviewee served as a co-authorial member of the Consensus Study Reports listed below their name. Some interviewees also served on reports not listed below their name in other capacities, for instance as a presenter or reviewer.

Philip Cook. 14th November, 2018.

- Inductive:** Understanding the U.S. Illicit Tobacco Market: Characteristics, Policy Context, and Lessons from International Experiences (2015).
- Abductive:** Proactive Policing: Effects on Crime and Communities (2018).
Deterrence and the Death Penalty (2012).

Malcolm Feeley. 18th January, 2019.

- Inductive:** Understanding Crime: An Evaluation of the National Institute of Law Enforcement and Criminal Justice (1977).

John Hagan. 1st June, 2019.

- Inductive:** Juvenile Crime, Juvenile Justice (2001).
- Reflexive:** Strengthening the National Institute of Justice (2010).

Janet Lauritsen. 16th November, 2018.

- Abductive:** Fairness and Effectiveness in Policing: The Evidence (2004).
- Reflexive:** Modernizing Crime Statistics, vol. I: New Systems for Measuring Crime (2018).
Modernizing Crime Statistics, vol. II: Defining and Classifying Crime (2016).
Ensuring the Quality, Credibility, and Relevance of U.S. Justice Statistics (2009).
Surveying Victims: Options for Conducting the National Crime Victimization Survey (2008).

Mark Lipsey. 14th November, 2018.

- Reflexive:** Improving Evaluation of Anticrime Programs (2005).

Rob MacCoun. 25th January, 2019.

- Abductive:** Informing America's Policy on Illegal Drugs: What We Don't Know Keeps Hurting Us (2001).

Tracey Meares. 31st May, 2019.

- Abductive:** Proactive Policing: Effects on Crime and Communities (2018).
Fairness and Effectiveness in Policing: The Evidence (2004).
- Reflexive:** Strengthening the National Institute of Justice (2010).

Daniel Nagin. 16th November 2018.

- Inductive:** The Growth of Incarceration in the United States: Exploring Causes and Consequences (2014).
- Abductive:** Proactive Policing: Effects on Crime and Communities (2018).
Deterrence and the Death Penalty (2012).
- Reflexive:** Strengthening the National Institute of Justice (2010).

Jerry Ratcliffe. 17th November 2018.

- Abductive:** Proactive Policing: Effects on Crime and Communities (2018).

Peter Reuter. 16th November 2018.

- Inductive:** Understanding the U.S. Illicit Tobacco Market: Characteristics, Policy Context, and Lessons from International Experiences (2015).
Understanding the Demand for Illegal Drugs (2010).

Firearms and Violence: A Critical Review (2005).

Abductive: Budgeting for Immigration Enforcement: A Path to Better Performance (2011).

Lawrence Sherman. 15th November 2018.

Abductive: Fairness and Effectiveness in Policing: The Evidence (2004).