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# Whither Criminology?: On The State of Criminology's Paradigm

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Whither Criminology?: On the State of Criminology's Paradigm

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My dad's example through his own quiet genius and voracious reading has inspired my pursuit of a doctorate. Exploring ideas simply for the sake of knowledge may not pay the bills but I am foolishly romantic enough to believe that these represent the only enduring contribution of humankind.

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## Abstract

*What is the condition of criminology's paradigm? The reply to this question has implications bearing on the profession's bona fides as a science as well as its sustainability as an independent academic enterprise. The work attempts to capture the elusive term through the use of five themes: theoretical consensus, methodological consensus, boundaries, the departure from sociology, and the current and future status of the field. In approaching this question the work presents an analysis of both qualitative and quantitative data. Semi-structured interviews were conducted with seventeen renowned criminologists. The centerpiece of the latter dataset was assembled and analyzed in prior research (Savelsberg et al. 2002). A content analysis of 2,109 peer-reviewed articles appearing in the field's top journals from 1951 to 2008 produced numerous findings. Criminology lacks a hegemonic theoretical orientation but a consensus is evident in the peer-reviewed publication data in terms of its methodology. The field defends its prerogative to draw from any tradition it sees fit to. A review of the content of the field's research and the debates discussed with the interviewees suggests a somewhat amorphous, yet still discernable, definition of the field's identity, one that is dedicated to the process of science. This can be seen in terms of the parameters of the seminal theoretical and empirical debates recounted by the interviewees. What is clear is that the field has successfully emancipated itself from the discipline of sociology both professionally and in terms of its content. Concerns were offered in terms of potential threats to the continued growth of the profession resulting from a reduction in funding and its becoming fractured and isolated organizationally but there are reasons for optimism in terms of the expansion of its research horizons into exploring state crime, overcoming the macro/micro divide and incorporating biological, international, cultural/anthropological, and power oriented themes. Discussion of the prospects for how the current work may come to inform a large scale research agenda conclude the work.*

## *Chapter I: Foundation and Framework*

Whither criminology? The established social sciences hold the field at bay. But within the professions and academy it blooms with vigor. Conventional indicators of success—enrollments, numbers of graduates, programs conferring degrees, professional journals with improved impact ratings, growing acceptance by a wider community of scholars and policymakers—verify that criminology and criminal justice (hereinafter “criminology” unless otherwise specified) is a field demonstrating all the indicators that promise of continued growth. Yet expansion cannot continue forever; the entire university will not be turned over to criminology. The growth in the profession will eventually moderate as that of its early sponsor, sociology, has done (Turner and Turner, 1990). Numerous other scientific endeavors have all experienced similar trajectories and will continue to (Price, 1965). Accepting this as the fundamental assumption, the present work poses a research question the field and its growing number of practitioners ought to consider: What is the state of criminology’s paradigm? That is, what is the degree of theoretical and methodological consensus within the field? The answer to this question has a bearing on the state of the field as well as its institutional well-being. The issue has recently been raised by a two of the field’s leading scholars (Laub, 2004; Rafter 2007, 2008a) but no systematic attempts have been made in addressing these concerns to date.

If it is not dismissed out-of-hand by the field the posing of such a question is likely to generate criticism. Indeed, it would be difficult to imagine a more inopportune moment at which to raise the issue. In a university environment increasingly attentive to markers of financial success the introduction of a criminology and criminal justice curriculum has been welcome. On the college campus it has managed to generate an interest on behalf of its primary constituency, undergraduate majors, while simultaneously attracting research funding from external resources. The field has flourished since the founding of the first independent School of Criminology at the University of California at Berkeley in 1950 (Morris, 1975) and offering 30 total courses



nationwide on the sociology of delinquent classes/ criminology in 1901 (Oberschall, 1972). As of 2001 the field had established near 25 doctoral, well over 100 master's level, and several hundred bachelor's programs (Clear, 2001). These programs graduated 82 doctoral students in 1999 and an estimated 2,300 master's students in 1997 (ibid). These figures, now nearly a decade old, are offered as indication of the field's having successfully ensconced itself in the university structure. Its boundaries have also expanded beyond the confines of police science to include such diverse interests as developmental taxonomies and bio-social influences in criminality to critical theories and a variety of sociologically influenced theorizing. The field has become accustomed to success. This holds the potential for encouraging a false sense of confidence and accomplishment. The field has established a record of professional success. The question of scholarly success is a different matter however.

And this is the point at which the problem begins. Much of the field's success is correlated with the growth of higher education. This makes the task of disentangling its success from that of its primary benefactor challenging. Earning revenue for the university and contributing to the accumulation of understanding through generating research findings are excellent attributes for an emerging social science to demonstrate. Through meeting these institutional goals the field of criminology has secured its immediate future on the college campus. The fact that independent departments are emerging indicates an institutional commitment on the part of universities in recognizing and fostering its unique contribution for years to come. With its short term future no longer in jeopardy it is time to begin an appraisal of its intellectual state. The university structure will support a field of endeavor for only a short duration—to satiate labor market forces for instance—without requiring it make a more sustained intellectual contribution. Ultimately, if the field is to secure an enduring presence it must demonstrate a capacity to compete in the market of ideas. What will help to ensure this is a coherent research agenda. This will offer the benefit of building a body of research findings which will produce a cumulative growth of knowledge by greasing the proverbial wheels of

discovery. Building cohesion around unified theoretical and methodological approaches will also serve to keep competing social sciences from imposing their perspectives on the field.

Metaphorically speaking, the field should be cognizant of the risks associated with being colonized by an imperialistically minded approach such as economics. A social science with a more resolute sense of its own purpose will be more successful in offering resistance, thereby extending its lifespan. Paradigm can be thought of as the field's bulwark against potential encroachments on its academic territory.

A cautionary tale can be seen in the unraveling of The American Social Science Association's 19<sup>th</sup> century broad mandate to eradicate poverty. The organization eventually splintered into political science and history, leaving the organization without an agenda sufficient to justify its continued existence (Haskell, 2000). Prior to engaging the task of building this consensus in hopes of avoiding a similar fate an evaluation of to what extent criminology may already demonstrate agreement is required.

#### *Statement of the Problem*

Reflections over criminology heading into social scientific discourse alone have been a growing theme amongst practitioners (Wellford, 1991; Akers, 1993; Savelsberg and Sampson, 2002), but has not lacked for historical antecedents (Wolfgang, 1963; Jeffery, 1978; Binder, 1987). Some contend that the field's effort at establishing an identity separate from its principle patron, sociology, offers professional and intellectual advantages. Having the autonomy to establish a research agenda uninhibited by and distinct from sociology is perhaps the foremost advantage mentioned. The thrust of the critique of this position is that criminology, after but a brief incubation within sociology, is not mature enough yet to sustain the rigors of academic debate without the benefit of a chaperone. Its lack of a more fully formed purpose and sense of identity will increase its susceptibility to intellectual whims. A critical part of the argument contends that as a policy centered exercise criminology is vulnerable to adopting the ideological

dispositions of funding sources. To the degree that it is shaped by these influences it cannot claim to be objectively pursuing truth.

The work of Joachim Savelsberg and colleagues (Savelsberg, King & Cleveland, 2002; Savelsberg, Cleveland & King, 2004) has demonstrated that while the content of findings of criminology are consistently generated through verifiably scientific methodology the research agenda—the topics it devotes resources to exploring—of the field demonstrates a correlation with prevailing political perspectives. Their review of the field’s literature appearing over a forty-two year span suggests that the choice of research question is influenced by the political agenda of funding sources. This effect is more visible when comparing agendas across disciplines. Research conducted by scholars affiliated with criminology or criminal justice programs tends to focus more on topics and theories advocated by state actors than their peers conducting studies within sociology departments (Savelsberg, King & Cleveland, 2002). This finding is suggestive that sociology is less prone to the influence of funding sources as a result of it being wedded to a more robust intellectual tradition. What makes these findings particularly problematic are the additive effects of the rise in governmental funding for criminological research over the last half century and “the relationship between a changing ideological climate and criminological knowledge [being] almost fully explained through funding and programming effects.” (Savelsberg, King & Cleveland, 2002, p. 327).

These criticisms (Savelsberg, King & Cleveland, 2002; Savelsberg, Cleveland & King, 2004) were generated through the careful collection and analysis of criminological publications appearing during the formative period of the field’s professionalization (1951-1993). The deference required by virtue of being subjected to the peer-review process was set aside in an editorial preface to a special edition of *Crime, Law, & Social Change* entitled “Mutual Engagement: Criminology and Sociology?” Here, Joachim Savelsberg and Harvard University’s Robert J. Sampson laid six critiques at the foot of criminology. Three of these are particularly relevant to the present inquiry. Firstly, it is alleged that criminology lacks an intellectual core

(i.e. “common assumptions, guiding insights, or an intellectual idea that animates” Savelsberg & Sampson, 2002, p. 101). Secondly, they argue that criminologists devote too much attention to earning disciplinary credentials, to the detriment of the development of intellectual ideas. Thirdly, reiterating the points raised in the paragraph above, they argue that as the field separates itself from sociology it is vulnerable to extra-scholarly influences. The caution suggested by the editors and voiced by others is that criminology would be well served to deliberate carefully before severing its institutional and intellectual ties to sociology (see also Short with Hughes, 2007). An evaluation of the field’s literature reveals that sociology doctorates still maintain a significant presence (Shutt and Barnes, 2008), indicating that there is still time to mend the divide without encountering lasting damage.

The field’s longstanding relationship with sociology is the legacy of one of its original theorists, Edwin H. Sutherland (Laub, 2006). Ironically, he may also be responsible for its intellectual diversity through the expansive definition of the field he provided, it being the most enduring and popularly cited. Criminology, he stated, is the systematic study of, “the process of making laws, of breaking laws, and reacting toward the breaking of laws” (Sutherland, 1960 p.3). The field is built largely around a focus on the explanation of variation in these three *separate* dependent variables. This tripartite agenda invites contributions from an open-ended range of research traditions; to that approach which can account for the most variation will go the rewards. The first piece (the process of making laws) implies the application of power, typically falling within the domain of political science, and to a lesser extent the sociology of law. Within criminology this question has traditionally drawn the attention of radical/critical criminologists (Turk, 1969; Quinney, 1970), in addition to rational choice (Beccaria 1775, [1983]) and labeling theorists (Becker, 1963). The second prong, pertaining to the breaking of laws, includes the research agenda of the bulk of the field’s practitioners. Conventional criminology addresses the question of etiology or motive. An abbreviated list of explanations falling under this heading include: control, social disorganization, anomie, strain, and differential association. Finally, the

reaction to the breaking of laws incorporates criminal justice, with its focus on the administrative apparatus of the justice system—police, courts, corrections. This interdisciplinarity provides the field with an abundance of intellectual flexibility. Additionally, this has also ensured the initial survival of the field and encouraged vigorous discussion.

However, this could also be the selfsame trait that is inhibiting its intellectual reputation. A leading scholar in the sociology of professions has offered the following observation on the state of criminology, “In the first place, interdisciplinarity has generally been problem driven, and problems. . . have their own life cycle. There is ample evidence that problem-oriented empirical work does not create enduring, self-reproducing communities like disciplines except in areas with stable and strongly institutionalized external clienteles like criminology. Even there, the status differences seem to keep the disciplines in superior power. Criminology departments hire from sociology departments, but seldom vice versa” (Abbott, 2001 p. 134). Two of the points raised merit additional discussion. Problem-driven intellectual pursuits, like those attempting to explain crime, are responsive to the entities responsible for defining and suppressing the problems (i.e., funding sources). By extension, the shape of the problem will demonstrate a corresponding impact on the shape of the interdisciplinary efforts; as the problem goes, so goes the field. The second point is that despite having the benefit of a “stable and strongly institutionalized external clientele” criminology lacks the intellectual or institutional capital required to have its graduates hired in the more established discipline of sociology. Arguably, this is an indication that sociology views criminology as an intellectual pursuit lacking in ideas, or at least those that cohere with and can potentially propel its agenda.

What would give the field leverage both professionally and intellectually, while simultaneously offering it the prospect of maturing into a discipline, is an overarching idea that sustains the research enterprise. Fundamental agreement on a unifying theoretical and methodological perspective would help to ensure criminology’s continued presence in the marketplace of ideas by facilitating a more efficient allocation of limited resources. Those

disciplines or fields of study with a greater degree of technical certainty and consensus evidence shorter review periods for peer-evaluated publication, more effect on pay resulting from research productivity, greater success competing for external funding/university resources, and more likelihood of having research results cited by lower consensus fields than vice-versa (Pfeffer, 1993). Professional consensus produces these advantages through reducing the amount of “task uncertainty” (Fuchs, 1992; Whitley, 2000). Researchers in the community are apprised of what questions require attention, the accepted manner in which to approach them, and how to interpret the results. As a result, there is relatively little effort expended in pursuing answers to questions which the field is indifferent to. Building from an established body of findings offers the advantage of encouraging a cumulative and linear accumulation of knowledge. The practical implication is that consensus offers the benefit of having a group of scholars recognize a limited number of findings as exemplars. In a sense, this serves the purpose of canalizing the research efforts of the field. The question of the value of criminology’s scientific credentials is a sensitive one. In order to approach it the field must be resolute enough to heed an honest answer. As a fledgling social science the posing of such a critical question would have been ill-considered; its mere survival was of utmost importance.

The field of criminology required some time between its founding and flowering to assure itself of its immediate future prior to asking self-critical questions. Having secured the appropriate measure of self-assurance the time for a systematic account of the field’s bona fides has arrived. Several luminaries have pointed the field in the direction of engaging the issue. During his 2005 Sutherland award address to the American Society of Criminology (ASC) John Laub (2006) promoted the argument that the idea of paradigm (“the soul of criminology” or “systematizing the concepts and problems of a given domain of inquiry in compact form” fn. 5) should be problematized. Two years hence, in his Sutherland address Franklin Zimring offered a similar sentiment, “Indeed, one threshold question is whether the only organizing principle of the discipline is its diversity—perhaps we could consider a name change to the American Society of

People Who Study Crime? Perhaps all methods and all perspectives are created equal in this postmodern era of ours” (Zimring, 2008, p. 257). The forum of the messages—the Sutherland Address being tantamount to a lifetime achievement award bestowed upon criminology’s most esteemed scholars—and the gravitas of its messengers notwithstanding, the notion of revisiting the idea of paradigm is likely to meet with resistance for two reasons. Firstly, criminology has managed to not just survive but thrive while lacking an organizing framework to this point. Many would ask, what would be the point of altering a program that has produced this much success? Secondly, there are longstanding theoretical debates with irreconcilable assumptions (Hirschi, 1979) and level of measurement issues (Short Jr., 1979) that cannot be simply ignored. This presents the practical difficulty of generating consensus even if the field wished to approach it through integrating traditions. Despite its professional reluctance to fix what may not be broken and the state of theoretical incongruity it behooves the group of scholars who identify as criminologists to engage this issue because of its existential importance.

These challenges to the field are not unprecedented. In many ways they are reminiscent of the Michael-Adler report which raised similar concerns early in the 20<sup>th</sup> Century (Michael and Adler, 1933). The substance of the damning points raised against the enterprise of criminology at that early point perturbed a young Edwin H. Sutherland. In response to its criticism that the field lacked a unifying framework, scientifically falsifiable hypotheses, and the incoherence he saw in the Gluecks’ research (Laub and Sampson, 1991) he formulated one of the field’s early dominant theoretical perspectives, differential association. In a similar vein, it is hoped that an appraisal of the points raised by the contemporary critics of the field can provide the foundation for another equally compelling reply. The first step in this process will be determining how valid these criticisms are. With over a century’s worth of accumulated research the field is in a much better position to both evaluate and withstand these critiques. Whether or not it will choose to mobilize resources in dealing with it will be up to the criminological community however.

### *Framework for the Study*

The touchstone of the current effort is Thomas Kuhn's (1970) account of the history of scientific progress, *The Structure of Scientific Revolution* (SSR, Kuhn, 1970). This work depicts the evolution of the scientific enterprise from inchoate fact gathering to sophisticated theory testing. Pre-paradigmatic sciences find practitioners toiling away at the task of accumulating and documenting observations. Little training, if any, is necessary at this early point to be part of the scientific effort; an interest in documenting the phenomenon will suffice. At some later point attempts are made to draw inferences that account for variations in the collected data. Competing theoretical perspectives are eventually formulated which purport to define and explain the given phenomena. At the outset each is compelled to simultaneously engage both the competing perspectives and the data. Because the competing perspectives use an independent explanatory framework in accounting for the events each largely speaks past the others by focusing attention on differing aspects of the problem. The absence of agreement on the terms of the debate induces stalemate. What is needed to overcome this discord is agreement on the fundamentals of debate and a methodology with which to approach the question.

There are two primary means by which a community of investigators comes to adopt a common understanding or paradigm. A pre-paradigmatic community must create one anew and a community with one on occasion will exchange an established understanding for a revolutionary approach. The most straightforward manner by which the former occurs is through a near universal recognition by the community that the new explanatory framework offers a more satisfying account of the observation than its competitors. The outcome can be established by a simple appeal to the logic of those confronting the problem. Kuhn also states that sociological shifts can account for the succession of one paradigm by another. If a conceptual apparatus fails to earn a sufficient number of adherents amongst subsequent generations—if students no longer cite it, that is—it is no longer deemed relevant. Alternatively, the revolutionary approach, that which displaces the older framework, is that which is deemed more relevant by the scientific



community (see also Collins, 1998). The history of science is filled with examples such as the paradigmatic thinking of Newton giving way to more recent contribution formulated by Einstein within the discipline of physics. Within criminology this can be seen in the understanding of Beccaria giving way to Lombroso and then Sutherland. The essential value of paradigmatic insights offered by these figures is that it provides the fundamental assumptions upon which to pursue additional understanding of the phenomena in question.

What the paradigm allows for is a state of affairs characterized as “normal science”. The “mopping-up” (the task of exploring the numerous implications of the paradigm) occurs within the boundaries established by these paradigmatic figures. The value that these unprecedented contributions grant is that they inform the process of puzzle solving. Paradigm offers a comprehensive understanding of the issue at hand and it delimits the tasks of the community. It defines the problem, offers guidance on a systematic approach, and suggests a perspective with which to interpret observations. The process of revolution begins anew with the accumulation of anomalies or unanticipated results. When a critical mass of *ad hoc* explanations are brought to bear in accounting for the anomalies the community begins the process of working itself out of the crisis through discrediting the existing paradigm and agitating for an improved approach. At this point the process of redefining the problem and seeking alternative solutions begins anew; it is a cyclical routine whereby a discipline works its way through a succession of normal sciences.

#### *Limitations and Criticisms of Kuhn*

The argument articulated in SSR, although integral to the work, does not constitute the framework in its entirety. There exist several worthy critiques of the work. The most significant of these in light of the current exercise is his almost exclusive reliance on examples drawn from the natural sciences. As a result, Thomas Kuhn’s work has been largely appropriated by philosophy and history, although the work is not completely lacking in sociological applications (Freidrichs, 1970; Nisbet, 1966; Ritzer, 1975). Kuhn insists that his theory of scientific progression can apply to the entire range of traditions aimed at understanding the human

condition, up to and including artistic expression (see Chapter 13, Kuhn, 1970). However, elsewhere in the text there is indication that his position is less than absolute: “On the contrary, the really pressing problems, e.g., a cure for cancer or the design of a lasting peace, are often not puzzles at all, largely because they may not have any solution” (Kuhn, 1970 p. 36-7). Here it is revealed that his characterization of the scientific approach may not be entirely inclusive of the social sciences. In addition, the quote indicates that his framework is not completely teleological. Broadly conceived, economics, political science, and criminology alike are each aimed at achieving “a lasting peace” through informing us of the nature of human interaction. Criminology hopes to achieve this end through determining what is responsible for criminality and recommending ameliorative measures. Applying a framework principally designed to explain the evolution of thought within the natural sciences, as Kuhn’s is, without modification to the social sciences would be imprudent. The primary difficulty is that the sciences are founded upon differing assumptions. Namely, natural science is driven to find *one* answer. For example, as a point of empirical fact DNA can assume only one structure but a lasting peace can manifest itself in numerous forms (compare Rawls, 1971 with Nozick, 1974). This will present a problem in applying the framework in the event that criminology is addressing itself to a problem which lacks a singular solution, or any solution for that matter. The first piece of Sutherland’s three part definition of the field (the making of laws) seems to introduce the notion of ideology into the field. Here, one can see the axiomatic difference between conflict and consensus theorists on how to achieve a lasting peace. This fact may represent a fundamental impediment to achieving theoretical or methodological agreement from the field’s practitioners. The intertwining of ideology and science has led some to argue that social scientists cannot claim to be scientists in the purest sense of the term (Hayek, 1955).

Thomas Kuhn’s work proceeds upon the assumption that science has an internal logic of its own; it is an introspective assessment of the processes within a self-contained endeavor. The argument generally ignores considerations proffered by the sociology of science and the

sociology of knowledge which contend that exogenous factors exert influence on the internal machinations of an investigatory framework. These literatures are resplendent with examples of how sources operating at a remove from science such as governmental funding (Stokes, 1997), cohort effects (Savelsberg & Flood, 2004), scholarly networks (Collins, 1998; Crane, 1975; Mullins, 1973; Zuckerman, 1976), professional competition (Abbott, 1988), and the waxing and waning of religious doctrine (Fleck, 1979; Merton, 1970)—just to name a few—contribute to the development and acceptance of scientific fact. The present work seeks to overcome this bias through devoting attention to cultural factors that shape the contours of criminology.

A related bias is that Kuhn implies a set of characteristics a community of scholars must demonstrate prior to earning the distinction of being a science. On the whole it seems to be the case that Kuhn recognizes the paradigmatic state of a field as being the singular hallmark of a scientific approach but draws back from this position in part: “there can be a sort of scientific research without paradigms, or at least without any so unequivocal and so binding as the ones named above”—Aristotle’s *Physica*, Ptolemy’s *Almagest*, Newton’s *Principia* and *Optics*, Franklin’s *Electricity*, Lavoisier’s *Chemistry*, and Lyell’s *Geology* (Kuhn, 1970 p. 11).

Systematic approaches to accumulating understanding should be thought to differ in scale, not in kind; paradigm is therefore argued to exist on a continuum. With the introduction of Lyell’s *Geology*, for instance, the science demonstrated more coherence than it had before. What is proposed is an evaluation of the state of paradigm with the recognition that the absence of a single marker of the maturity of its paradigm does not nullify its consideration as a science. Paradigm is used as a crude approximation of the field’s development. If criminology relies almost exclusively upon a single theoretical construct and methodological approach it can be considered as more developed than those enterprises engaged in simple data collection; making any more fine grained analysis than this significantly increases the difficulty of the work. However, the absence of several of the hallmarks of paradigm likely will make offering this determination easier.

Another criticism raised in opposition to the Kuhnian perspective rests on philosophical grounds. Karl Popper (1959, 1963) insists that the *sine quo non* of the scientific approach is falsification. A theory cannot be proved; it can only be disproved. The objection leveled is that Kuhn's conceptualization of science allows for, indeed even encourages, *ad hoc* explanations to account for anomalous findings. The problem is that these are not necessarily incorporated into the larger theory in any meaningful sense; they are simply appended without being embedded. Rather than dismiss an existing paradigm its advocates are inclined to develop piecemeal explanations. Only when an inordinate number of these accumulate does a crisis develop and a need for a revolutionary approach emerge. Yet even when a revolution succeeds its victory is often not total or complete. SSR documents that displaced paradigms are often maintained contemporaneously with revolutionary paradigms, as they frequently manage to answer lingering questions more satisfactorily than the newer formulations. Revolutionary paradigms are not necessarily more comprehensive than their antecedents. Popper argues this is inexcusable; theories that fail to account for phenomena that they purport to merit outright rejection (for a similarly provocative statement see also Feyerabend, 1975). Fundamentally, this is a disagreement over the defining characteristic of science. Ultimately, Popper's critique misses the point. Kuhn is engaged in explaining the machination of science not in making normative claims.

Nevertheless, Popper's criticism has been applied to criminology by Thomas Bernard (1990). He argues that the principal problem of the field is in its accumulation of theoretical perspectives that lack testability. What the field should focus on after its twenty years of testing theories is on falsifying perspectives. Emphasis should be placed on purging the field of theories that fail to enhance our understanding of the phenomena in question. In order to accomplish this end he states that scholars proffering theory should be required to offer falsifiable hypotheses. Furthermore, theoreticians should be obligated to grant advice on how to properly evaluate the predictions they put forth. A dilemma presents itself when thinking of executing this mandate. As indicated earlier, within criminology there are theoretical perspectives which hold

assumptions of human nature that are unalterably opposed (e.g. strain v. control, deterrence v. labeling). Evaluating the merits of their fundamental assumptions with objective certainty is an impossible task; the content of man's character has perplexed humanity for eons. Despite this obstacle, Bernard goads the field in the direction of being more sensitive to creating falsifiable theory. This can be accomplished through altering the structure in which research is conducted. Graduate training and tenure requirements should be adjusted in order to reflect an awareness of the problem. Perhaps by reducing the number of theoretical contenders (pretenders?) through eliminating theoretical constructs that are untestable the field can achieve a unified perspective or at the very least reach tentative agreement as to what fails to contribute to an explanation.

Yet another philosopher raises a pragmatic criticism. Throughout SSR Kuhn uses the term "paradigm" with nuances too numerous for it to have a reliable meaning. With twenty-one different shades of meaning (Masterman, 1970) in the central conceptual tool of the present work it is important that another, more consistent, definition be used here. The definition employed by a leading sociologist in his survey of the state of sociology's paradigm is generally consistent with that of Kuhn's without the ambiguity: "A paradigm is a fundamental image of the subject matter within a science. It serves to define what should be studied, what questions should be asked, how they should be asked, and what rules should be followed in interpreting the answers obtained. The paradigm is the broadest unit of consensus within a science and serves to differentiate one scientific community (or subcommunity) from another. It subsumes, defines, and interrelates the exemplars, theories, and methods and instruments that exist within it" (Ritzer, 1975). This definition is comprehensive and contains at least four fundamental elements bearing on the current inquiry. The first point is that the paradigm limits the scope of investigation and offers accepted guidelines for generating appropriate questions, as well as protocol for approaching and interpreting data. This results from the second point: these guidelines are created by a community of investigators—agreement on these issues exists at "the broadest unit of consensus". Thirdly, this consensus is used to distinguish this group from other communities.

It serves the sociological purpose of creating a marker of identity. Lastly, this consensus is built upon a common understanding derived from the application of an interrelation of the exemplars, theories, methods, and instruments it avails itself of. That is, there is a tradition which the group recognizes as being foundational. Against this standard a determination on the state of criminology's paradigm will be made. This process will begin with a review of the work conducted to date in gauging the extent of consensus within the field.

### *Prior Research*

Although the idea of paradigm has appeared within the literature of criminology recently (Laub, 2004; Rafter 2007, 2008a) there have been no explicit attempts to assess its state as yet. This is not meant to suggest that there is a total absence of research informing the current investigation. The research efforts have progressed along disparate lines and little effort has been expended in combining them to gain an enriched perspective. The concatenation of these fragmented research efforts yields an outline of what the field's understanding of its broadest unit of consensus is. What follows is a discussion of the contributions made through professional accounts given by leading intellects in the field, literature reviews, and surveys on the popularity of explanatory frameworks and criminogenic factors.

Arguably the most popular body of work informing an evaluation of the paradigmatic state of the field can be found in the retrospective accounts offered by the leading intellectuals in the field. Accounts from either the primary scholars themselves or others mentored by them have an established history in both criminology (Mannheim, 1960) and sociology (Horowitz, 1969). These accounts generally offer readers a first-person account of the origins of theoretical and methodological contributions. Some of these innovations resulted from exposure to training or reading outside the field, indicating intellectual debts the field owes to parent disciplines. Often included in these recollections are thoughts as to the factors influencing the success of specific contributions. Popularity of a given idea bears witness to the thought that a community recognizes as more or less central to its approach. More recent contributions have tended to rely

on in-depth interviews to account for the development of both careers and the field (Laub, 1983, 2002; Adler, 2002; Savelsberg and Flood, in press, Cullen and Messner, 2007, Cullen et al., forthcoming), but have not neglected the first hand written accounts (Short, 1969; Chambliss, 1987; Bursik, 1998; Merton, 2000; Geis & Dodge, 2002; Savelsberg, 2007; Sampson, in press). Other efforts have examined the field from a more historical and philosophical point of reference (Jones, 1986; Zeman, 1981). These historical overviews offer value through connecting developments of criminological understanding to cultural forces and ideas *writ large*, the Enlightenment for instance. The same accounts are weak, however, in explaining endogenous developments.

The recent efforts at “taking stock” (Cullen et al., 2006) address this shortcoming. These accounts are typically offered by the theoretical perspectives’ major proponents (e.g. Ronald Akers—differential association/social learning, Michael Gottfredson—control theory, Steven F. Messner and Richard Rosenfeld—*anomie*). These accounts offer an idea of why perspectives within the field tend to vary in their importance over time. Many of the field’s major contributions spark excitement in the research community before another idea supersedes it as the focus. There seems to be a structurally induced shortness of attention span within both sociology (Gans, 1992) and criminology (Bursik, 2009). Typically the process begins when the field’s major theories encounter criticism and are subsequently set aside. Eventually they are rediscovered and reformulated. Briefly stated, within the field of criminology there are a multitude of theoretical perspectives, none of which has maintained the exclusive attention of its community for a sustained period. Perhaps the best illustration of this cyclical process can be seen with the lifecycle of strain/*anomie* (Merton, 1938) theory. The criticisms of Travis Hirschi (1969) and Ruth Kornhauser (1978) effectively dismissed the theory as a credible explanation. After a period of dormancy the idea was revived in two important efforts that have renewed the interest of the research community, institutional *anomie* (Messner and Rosenfeld, 1994; see

Messner and Rosenfeld, forthcoming) and general strain theory (Agnew, 1992; see Agnew, forthcoming).

Looking at the literature of the field presents a more objective account of the field's intellectual consensus. These analyses offer indication of what the field values through determining what contributions influence its research. In terms of comprehensiveness and depth the effort of Wolfgang, Figlio, and Thornberry (1978) is unmatched. In *Evaluating Criminology* (Wolfgang et al., 1978) the authors surveyed 99 scholars working within the field in addition to amassing, cataloging, and analyzing the total criminological literature—over 4,000 books and journal articles—appearing from 1945 to 1972. The findings derived from the assessments gathered through the surveys were largely consistent with the pattern reflected in the literature. Seven of the top ten works mentioned by the sample as being influential appeared in the top ten most cited in the literature. This select group of works, to name a few, includes such theoretical and methodological classics as *Delinquent Boys* (Cohen, 1955), *Delinquency and Opportunity* (Cloward and Ohlin, 1960), *The Outsiders* (Becker, 1963), and *The Measurement of Delinquency* (Sellin and Wolfgang, 1964). A minority of work inspires much of the field's research. "2.2 percent of all the works. . .received one-half, and 0.5 percent received one-quarter of the citations" (ibid, p. 37). Alternatively, over half the works collected were never cited. This is a pattern that can be seen in the scholarship of other disciplines and across the spectrum of human enterprise, especially with regard to scientific productivity (Lotka, 1926; Murray, 2003). The results of their analysis produced a veritable canon. This can be conceptualized as the body of work the field's scholars were in broad agreement with respect to its importance.

*Evaluating Criminology* (Wolfgang et al., 1978) also discusses the theoretical orientation of the field's work. Unsurprisingly, at this early point in the field's development orientations were borrowed from established disciplines such as history, demography, psychology, and psychiatry. This determination was based on their being overrepresented in terms of total citation counts; these categories accounted for a disproportionate share of the literature being cited. By



this same measure, what are now regarded as standard criminological subject matter and orientations were underrepresented during this period. Recidivism, official and self-report statistics, victimization, labeling, social disorganization, control, anomie, differential association, deterrence were among those appearing with less regularity than expected. Measured differently (in terms of the average number of citations to literature within each category) many of the latter rank higher in terms of what the authors refer to as “quality”. This may account for their eventual emergence as important theoretical explanations within the field. The quality of this work was able to overwhelm the quantity of other perspectives in terms of effectuating an impact. This finding is evidence of an early development of a distinct criminological perspective. Additional support for this hypothesis is found in the differential between total and average annual citations partially diminishing when controlling for time. The authors explain the apparent disparity as resulting from the mathematical orientation of the more recent works. The inclusion of mathematical modeling required a greater level of sophistication or training from practitioners if it was to be cited. Having been trained in the mathematically oriented methodology younger cohorts were more likely to cite the higher quality work. This is suggestive of the field’s early methodological development and quantitative orientation, marking a departure from the methodology used more commonly in its earlier years (Wolfgang et al., 1978 p. 48-53). The current incremental movement toward an experimental approach and away from an extended history of working within an analytic framework resembles this earlier shift (Sherman, 2005).

More contemporary efforts have been limited due to a lack of funding (Cohn, Farrington, and Wright, 1998). Despite these constraints a trio of researchers—Ellen G. Cohn, David P. Farrington, and Richard A. Wright—have managed to systematically tabulate citation counts of contemporary criminology and criminal justice scholars both domestically (Cohn & Farrington, 1998a; Cohn, Farrington & Wright, 1998; Cohn & Farrington, 2007; Wright, 1995) and internationally (Cohn & Farrington, 1990, 1994, 1998b). Their work identifies the field’s dominant contributors. Although their results vary depending on the time periods covered and

journals collected, names such as Alfred Blumstein, Travis Hirschi, David Farrington, John Hagan, and James Q. Wilson, amongst others, appear with regularity as among the most cited. It is to these scholars the field looks when framing its research agenda. Unfortunately, the work has failed to generate the wider audience it deserves due to its lack of a theoretical framework. The results are presented as a plain enumeration of scholars that have accumulated the most citations over a selected period of time. Two of the authors take a tentative step in the direction of introducing theoretical constructs with the formulation of “citation careers” in a contribution appearing in the *Crime and Justice* annual series (Cohn & Farrington, 1996). These illustrations suggest that publications crest and decline in terms of the number of citations each manages to earn over time. Explaining these patterns could produce an enlightened understanding of the intellectual trends of the field. Those contributions that consistently appear amongst the most cited are of more paradigmatic import than those making brief appearances.

Within the literature review approach a more rigorous methodological tool for evaluating a body of findings has recently been developed. Meta-analysis documents in a systematic manner the consensus the field has reached on an issue by way of its accumulated research findings. This also has a bearing on the question of whether or not the field ever dismisses a theory based on its demonstrated incapacity to account for variation. Maintaining consistent explanatory capacity through repeated testing stands as evidence of a given construct’s durability as an explanatory mechanism; this should serve as a foundation for future expansion. Conversely, this method can point to theory that has failed to muster sufficient empirical evidence. These are concepts that can be thought of as having been falsified or at least dismissed. The meta-analysis is beginning to be applied to the literature that has accumulated within a few theoretical traditions. The statistical approach involves the systematic coding and weighting of variables within the collected research of a given theoretical perspective. What results is a numerical value that summarizes its empirical validity. With regard to Gottfredson and Hirschi’s (1990) self-control theory Pratt and Cullen (2000) found a significant enough effect size (0.2) to lead them to the conclusion that all

future research failing to include the construct in their models risked misspecification. When these scholars turned their attention to reviewing over 200 studies using a macro-level approaches they found that, “social disorganization and resource/economic deprivation theories receive strong empirical support; anomie/strain, social support/social altruism, and routine activity theories receive moderate support; and deterrence/rational choice and subcultural theories receive weak support” (Pratt and Cullen, 2005).

Another account of criminology’s consensus on theoretical and methodological matters results from a series of findings generated from questionnaires administered to the membership of the ASC. The earliest effort polled 182 attendees of the 1986 meetings (Ellis and Hoffman, 1990) on their opinions within three domains: most significant specific factors in crime causation, theories with the strongest support, and perspective subscribed to. The most commonly selected cause of delinquency and minor criminality were “poor supervision in the home or unstable/uncaring family” (24%) and an “economic system which prevents participation by some individuals” (21%). This order is reversed, but again totals to near half the sample response, in reference to popular explanations for serious and persistent criminality (30% and 17%, respectively). With regard to the second domain, respondents cited control theory (27%) and social learning (18%) as those with the strongest support. Lastly, several theoretical perspectives were marked as important: symbolic interactionism (19%), behaviorism (13%) Marxist/conflict (11%) and positivism (11%). Ellis and Hoffman (1990, p. 56) conclude that “no theory or perspective is really dominant at the present time (with the possible exception of control theory).”

Two follow-up efforts have produced findings consistent with the above. Similar methodology and sampling frame used a decade later affirm the continuing popularity of control theory. “Together the two versions of control theory (social and self) were the most favored” (Walsh and Ellis, 1999 p. 1). However, there were marked differences in the popularity of perspectives by self-reported socio-political ideology, with “conservatives” citing family problems and discipline and “radicals” referencing societal inequality in explaining crime

causation. Again there were differences in the explanatory framework chosen by the overall sample. Control (social and self), differential association, and conflict combined to account for less than half of the respondent selection for delinquency/ minor adult offending (44%) and serious/persistent offending (43% Ellis and Walsh, 1999 p. 4). Yet another decade later the same exercise revealed that the support for control theory had eroded as a result of the reemerging popularity of once dormant theoretical perspectives and the introduction of new perspectives (i.e. differential association-reinforcement; self-control and developmental, respectively; Ellis, Cooper & Walsh, 2008). A polling of 2007 ASC conference attendees found, consistent with the initial survey, that a plurality agree that the frustration caused by the economic system is the most important cause of serious/persistent offending while peer influences reappeared as the most referenced cause of delinquency. Less than half (46%) cite social learning, life course/developmental, social control, or social disorganization as holding the most explanatory power with regard to serious and persistent offending (ibid). This indicates that the field lacks a dominant theoretical perspective; the most popular won by a plurality, not a majority. An even greater diversity of opinion than that seen with reference to the popularity of theory can be seen in the results pertaining to the importance placed on the twenty-four causal factors listed. This indicates an even wider dissensus amongst the field's practitioners on the relative importance of the causal factors that merit consideration. The reemergence of social learning—it failed to make the top five in the prior survey—suggests that the popularity of explanatory frameworks may falter but are never forever dismissed. Alternately stated, this is evidence that theoretical perspectives are subject to revision. The results gained through these studies are valuable because there are little data bearing on the question of professional consensus. It should be noted, however, that confidence in the findings is limited by the low response rates for each of the three survey cycles and its being limited to conference *attendees*.

Another manner in which a group of professionals' consensus can be determined is through an evaluation of the content of their meeting's annual proceedings. Two such efforts

have been made with reference to the Academy of Criminal Justice Science (Robinson, 2002; Ruffinengo, Mueller, and Collins, 2008). A comparison of the abstracts submitted for the 2002 (n=705) and 2007 (n=1099) conferences revealed a significant jump in the number of submissions. This finding indicates the success of criminal justice at drawing research attention to the issues it is pursuing. With the exception of a similar spike in the number of papers dedicated to exploring terrorism and a quintupling of the number of juvenile justice related themes the content remained largely consistent between conferences. Listed in descending order of frequency are papers presented at the 2007 conference concerning: types of crime (22%) policing (15%), juvenile justice (11%) corrections (10%), law & order (10%), and terrorism/national security (7%).

The body of work presented above provides a measure of insight into the state of consensus within the field. It also offers guidance to the proper ways in which to approach the question of paradigm, especially with regard to data and methods to be brought to bear. Like all other research efforts past, present, and future, it offers answers to some questions while posing additional others. As such, its applicability is limited.

The present effort applies the lessons learned through these works in the hopes of pushing the research agenda focusing on paradigm forward. With regard to the retrospective accounts offered by the field's notables only one effort (Savelsberg and Flood, in press) has attempted to make sense of opinions offered by a group of scholars. The remaining efforts elaborate on the thoughts given by isolated scholars. In order to answer the current research question an approach more like the former is required. The literature reviews provide an excellent understanding of the intellectual content of the field's discourse. The primary problem with these efforts is their scarcity. Furthermore, the work of Wolfgang et al. (1978) is now thirty years old. The field has evolved in numerous ways since it appeared. The more contemporary efforts of Cohn, Farrington, and Wright are generally focused on five- to ten-year periods and on the contribution of authors, as opposed to publications. For present purposes more attention will need to be

directed at a lengthier time frame and on the theory and methods of intellectual contributions. Lastly, the survey data are weakened by a low response rate and using samples limited to those members attending annual meetings decades apart. Before discussing at greater length how the current work builds on the foundation offered in these studies an explanation of the structure of the work is given. The research themes articulated help to organize the research undertaken (see chapter outlines) and aid in the readers' digestion of results.

### *Research Themes*

Building on the work outlined in the previous section, the sum total of the work to be presented represents an attempt at assessing the state of paradigm within criminology and its implications for the profession. This task has been divided into five portions. This serves to make the task of measuring the status of a concept as amorphous and abstract as paradigm conceivable. This also assists in making both the criteria for assessment and the work's assumptions explicit. The five themes are interrelated to some degree; these categories are not meant to imply that the themes are mutually exclusive. They are intended to be conceived of as the means by which the abstract concept of paradigm can be connected with the indicators detailed in the following section. It is emphasized that these categories are used as a heuristic with which to approach the idea of paradigm; these categories are offered with the caveat that the reader should not construe the exercise as an effort at reifying the idea of paradigm. The variables to be measured are indicative of the construct but not the concept itself. The five broad categories that the work will draw attention to are theoretical consensus, methodological consensus, boundaries, the departure from sociology, and the present and future of criminology. The inclusion of each is briefly justified below.

*Theoretical consensus* signals the extent to which the field's research is directed by a dominant perspective. Those fields/disciplines in which multiple theoretical perspectives are evident are less unified in their approach to science than those with one or only a few dominant perspectives. In the event criminology demonstrates a tolerance for an abundance of theoretical

explanations, some of which are likely to be contradictory to others, it is less paradigmatic. A paradigmatic field will also show less tolerance for the reintroduction of once dormant perspectives. Once it has adopted a framework it will rely on that until its explanatory capacity is exhausted and a new successor is formulated. Permanently dismissing theoretical perspectives reflects the process of falsification. However, at the moment revolutionary perspectives are brought to life there may be a brief window in which the field is more chaotic and other perspectives are sought in an effort to explain anomalies. Making a determination as to where in the progression of achieving consensus the field of criminology is will pinpoint its evolution as a science.

*Methodological consensus* is to be gauged for all the reasons specified immediately above, only within the context of the field's settled methodological approach. Again, consensus denotes a more robust state of paradigmatic health relative to dissensus.

The field may or may not have settled on a particular methodology by which all theoretical approaches are to be tested and empirical facts are to be documented. The types of data that are available to be tested also play a role in mediating theoretical and methodological debates. Methodological consensus and the application of specific types of data to a field's research agenda are indicative of the accepted protocol for deciding the outcome of conflicts over theory. It sets the parameters within which the argument is to proceed and the criteria by which knowledge is to be assessed by the group. Its methodology attests to how the field conducts itself as a science. It defines the context of its inquiry.

*Boundaries* will detail debate and its mediation within two concentrations included under this heading, internal and external. Fields and disciplines seek to regulate, police, and repel thought that offends its principles. Contributions or revisions offered by its practitioners that challenge its accumulated understanding will encounter resistance from the community of scholars who are invested in the perspective or paradigm. Affronts to the established order will either overcome the criticism in some capacity or surrender in whole or in part. Simultaneously,

external borders must be erected and fortified; this captures the essence of the Durkheimian notion that a group defines itself over and against another. Cataloging the hallmarks of a given community's collective understanding of itself is readily accomplished by contrasting it with another. Determining the points of contention and the denouement of the controversy reveals the structure that a field of study seeks to maintain. It is through this dialectic of conjectures and refutations (Popper, 1963) that science trudges forth and identity solidified. This process is analogous to that of molting; as the newer understanding emerges the older shell is sloughed off. This comes at the cost of some momentary chafing however.

Mapping the boundaries of the field reveal what it values by indicating what it is not. Few of the multitude of ideas that are proposed resonate within the group, in fact most are ignored. Alternatively, few contributions generate discussion, much less spirited exchanges (Collins, 1998). The majority of ideas offered manage to contribute to the base of understanding without challenging accepted beliefs. When a contribution manages to provoke reaction this is indicia that its community holds the threatened claim to be well established and worth defending. A leading scholar within the sociology of science calls these reactions, rearguard or otherwise, "boundary work" (Gieryn, 1999). In addition to maintaining integrity internally, the field must simultaneously defend its claims against challenges leveled from other disciplines.

This theme borrows its essentials from Stephen Cole's (1992) conceptualization of the "core and frontier". The "core" represents the fundamentals that the field adopts while the "frontier" represents the periphery of research where the rules tend to be nebulous and less rigidly enforced (Nisbet, 1976). Another scholar refers to these essential elements as "black-boxes" (Latour, 1976). With these foundational assumptions the field builds its understanding. In the Kuhnian sense revolution occurs when the structural integrity of this foundation, erected on an infrastructure of black-boxes, is compromised in light of either new evidence or a newer explanatory framework.



The community of scholars must sift through the evidence offered in adjudicating what constitutes a contribution to its core versus that which is to be relegated to its frontier. Determining the mechanism by which claims to truth are mediated indicate what the field's practitioners regard as legitimate evidence. In a rough sense, outlining the parameters of the debate affirm the scientific standards the field has reached a measure of agreement on. There is likely to be a varied response to a claim, ranging from outright rejection to complete acceptance, which is contingent upon the evidence that is presented. Less scientific, more ideologically saturated, fields may accept *ad hominem* critiques. This can be contrasted with more scientifically oriented fields which have come to rely (nearly) exclusively on the objective evaluation of data. Crudely analogized, recourse to a knife would not be deemed fair in settling an exchange on a point of scientific fact but arouses less opprobrium when combatants are locked in a street fight. This context denotes what a community recognizes as legitimate methodology in terms of advancing claims. Ultimately the community is vested with the responsibility of sanctioning its processes.

The contributions at the core of an approach can be seen in what the field gives formal distinction to through the bestowing of awards. Through this method it points to given contributions or research as exemplars. The remainder of the field is encouraged through witnessing the conferring of "ribbons and gold" (Stephan and Levin, 1992) to make similar efforts at pushing understanding further. Although these ostensibly honor individuals they also serve to commemorate scholastic contributions that have been built into the foundation of the enterprise. With some degree of regularity ideas that may have been initially marked as "controversial" eventually gain acceptance. These are contentions that at first are recognized as a threat to the established understanding that manage, through the give and take of scholarly discourse, to overcome its criticisms. Scholars who offer these insights must often endure the initial criticisms only to earn accolades later in their career when the furor has subsided.

Conversely, once conservative ideas are periodically problematized (e.g., labeling perspective's rejection of the use of official data).

*The departure from sociology* has been a cause for concern because it is predicted that this will leave criminology without an organizing framework equal to the task of generating lasting knowledge. The implication that is commonly drawn is that this will produce a field lacking an abiding source of ideas, resulting in a disorganized science. Additionally, without sociology to guide the younger social science it will leave the latter susceptible to any available tradition wishing to implant its theoretical or methodological approach. If the field falls prey to academic imperialism then it lacks sufficient justification for pursuing an independent course. This professionally driven divergence from sociology serves to distance the field from its historical source of ideas, content, and identity. While some in the field may find this development disconcerting others are likely to be untroubled by it or welcome it as beneficial. To this point an empirical assessment of the intellectual departure has yet to be conducted. If works on criminological topics are now appearing more in expressly criminological outlets as opposed to sociologically oriented journals then the field can be characterized as growing in independence. Publications appearing in criminology outlets are reviewed and consumed by a more interdisciplinary group of scholars in terms of their training, appeal to a different readership, and reference research that is less likely to be sociological in origin. In addition, the similarities or differences in theoretical and methodological content between sociology and criminology outlets will allow a determination of just how independent criminological research has become. The normative question as to how troubling practitioners find this development will also tell how much confidence practitioners have in its continued success as an independent academic venture.

*The present and future of criminology* are sources that hold the potential of undermining the continued success of the field in addition to topics of inquiry left to be explored. There are two primary sources that this work acknowledges that imperil its expansion, both related to criminology's status as a science. A reduction in research funding would serve to reduce the

field's claim of scientific legitimacy as well as put a brake on the engine of intellectual growth. The state of criminology's science may be related to this pragmatic concern through the following line of logic. In the event that policy makers no longer see the validity of criminology's contributions they will no longer fund basic research. Criminology failed to predict the crime decline and has, with only isolated counterexamples (Blumstein and Wallman, 2000; Zimring, 2007), failed to recognize the importance of accounting for this trend. If prediction is the elementary task of a science the field should seek to bolster its scientific credentials on this account. Failing to do so may result in policy makers giving research dollars to competing institutional interests addressing crime such as public health or economics. Fundamentally, the argument is that legitimacy is granted by a profession's clientele (Larson, 1977; Abbott, 1988). Opinions will vary as to how much this is perceived as a potential source for undermining criminology's continued growth.

If criminology is to grow it will need to annex intellectual terrain through engaging questions it has not before. One of the ways this can be effectuated is through the application of its conceptual tools to developing phenomena, such as accounting for war crimes, for example. Another source of knowledge expansion results from renewed interest in more traditional subject matter. Expanding the white-collar crime research agenda and pushing methods in the direction of approaching persistent problems with new tools are just two examples of how criminology may engage in normal science, consolidating its gains and pushing its frontier outward. This approach could solidify its claim to exclusive expertise on the core issues the field contends with. In the event that the field meets with sustained resistance in expanding its research agenda at its extremities it would be well served to deepen its understanding of issues that have traditionally fallen within its purview.

These five research themes structure the inquiry. The current work is atypical in that it is not an effort in applying deductive reasoning. Therefore, a series of hypotheses are not being elaborated and tested. Rather, the work represents an initial foray into exploring the state of

criminology's science. At the heart of the effort is a bifurcated focus. The first focal point is determining the state of criminology's paradigm. The secondary focal point is assessing the practical implications that can be drawn from the preceding findings. Rather than wander aimlessly into the inquiry these five themes help to direct the efforts toward tangible targets.

The substantive chapters are ordered in a progression beginning with the core of the field and ending at its periphery. The first two themes, theoretical and methodological consensus, represent the ideas that are at the center of criminology's identity as a social science. These are the elemental notions that define it as a form of scientific endeavor. Consequently, they are packaged together in a single chapter prior to the work moving toward addressing paradigm in a more indirect manner. The chapter that follows presents findings on boundary maintenance, the departure of criminology from sociology, and prognostications on the future of the field. This draws attention to the state of intellectual coherence in the field's approach as well as the implications of criminology's carving out a unique niche within the university structure. The final element enumerated is the most practically oriented of the three, its primary concern being that of criminology's institutional well being and its prospects for continued success in competing as a profession.

## *Chapter II: Data & Methods*

In assessing the state of criminology's scientific evolution over the last half century qualitative and quantitative data have been analyzed. The primary limitation of the literature discussed heretofore is its abbreviated time frame. Paradigms take time to develop. A brief snapshot of a science could represent the legitimate finding of a paradigm, or merely a passing fad. Alternatively, dissensus could represent a structurally imbedded condition of the field or a momentary divergence from consensus that will presage a Kuhnian revolution. In either event, the interpretation of shorter term trends presents difficulties. In an effort to overcome this shortcoming the present investigation approaches the question with two types of historical accounts stretching back several decades. One relies on the narratives of a select group of decorated scholars and the other an analysis of the field's published peer-reviewed research. The use of two types of data is intended for the purpose of generating topical overlap, thereby increasing reliability. The weaknesses inherent in one approach are compensated for by the strengths of the other. An additional benefit is that each type includes independent data elements as well. Through an appraisal of criminology's history and evolution it is plain that patterns are evident. These trends point the field in the direction of fruitful inquiry and potential development. The work looks to its past in an attempt to inform its future. What follows is intended as a descriptive account of the goings-on within the field from the 1950s to the present emphasizing the five primary themes detailed in the preceding. An explanation of the content of the qualitative data is offered prior to a discussion of the quantitative data.

### *Qualitative Component*

This portion of the work is informed by earlier oral historical efforts within the field (see also, the Oral History Project housed at the University of Pennsylvania) and beyond. These efforts have been used to document phenomena as varied as the hardships encountered by survivors of the Great Depression (Terkel, 1970), the development of our early understanding of

criminal behavior (Bennett, 1981), criminal careers (Shaw, 1930, 1931), and criminological careers (Laub, 1983). Each of these contributions relied on raw materials that were originally verbal accounts. These were then edited, bound, and published. These works improved understanding on issues that were poorly developed or emergent at the time. For this reason it is an ideal methodology for an initial attempt at discerning the state of criminology's paradigm. Several renowned criminologists have made recent contributions to our understanding of the field's development through offering accounts of their own progression as scholars: Albert Cohen (Cavender, 1993), Meda Chesney-Lind (Cavender, 1995), Travis Hirschi (Laub, 2002), Coramae Richey Mann (Cavender, 1996), Robert K. Merton (Cullen and Messner, 2007), Jerome Skolnick (Cavender, 1994), and Marvin Wolfgang (Adler, 2002). These accounts highlight major personal and impersonal influences on their thinking as well as major projects and publications and their importance. Through their work criminology has advanced its understanding and refined its approach to its subject matter. Accounting for the importance of their work, through their introspective reflections, offers insight into the values of the field.

This portion of the work follows in the oral history tradition through its collection of in-depth interviews with luminaries from the field of criminology. The sampling frame was purposive. As a result, the thoughts offered by the group are not intended to be representative of the field's practitioners in general. Rather, what is intended is a general indication of the collected wisdom of the field's elite. Those twenty-five scholars invited to participate, seventeen of whom eventually did, were selected because of their being atypical. Members of the sample were selected because each has pioneered a perspective within the field. The sample includes experts from numerous traditions. Social disorganization, anomie/strain, differential association/social learning, control, critical/radical, developmental, methodology, policing, routine activities, and victimology are all represented. A general familiarity with the work of the field's notables, resulting from years of reading, produced a working list of candidates. These are scholars whose work has been widely and repeatedly cited, demonstrating their esteem within the

criminological community. Ten of the seventeen interviewees rank among the top 33 scholars in a recent ranking of their citations within the criminology and criminal justice research (Cohn & Farrington, 2007). This group has demonstrated a talent in producing scholarship that is integral to the field's accumulated understanding. Published work by several sampled scholars also indicated those who would make meaningful contributions to the understanding of state of criminology's science by virtue of their already having given the subject matter consideration. The current work presents an account of the field's development, as well projecting its promise for the future, from these oral contributions.

The two essential criteria for inclusion in the sample were longevity and success of career. Career duration offers the respondents understanding on how the field has developed with regard to the five research themes over their tenure. Each has seen methodological and theoretical arguments gain favor and unravel. They have also authored and witnessed a number of enduring contributions. Some have participated in its seminal debates and all have witnessed them. Their consumption of the field's work over their extended careers has also granted each an informed perspective on what differentiates fad from fact. The sample averaged 1973 for a date of doctorate producing an average of approximately thirty-five years of experience prior to the interview.

Determining length of career is simple enough to measure; establishing level of success is more difficult to accomplish. A listing of the awards accumulated should serve as an indication of the respect their work has earned. Of the seventeen interviewees sixteen are ASC fellows. The ASC has also recognized eight of the group as president, nine as Sutherland Award winners, and one Herbert Block Award winner. Two now have awards named in their honor issued by the organization. Included in the sample is a former president of the American Sociological Association (ASA). The ASA has recently honored another interviewee with its lifetime achievement award. This same scholar earned the same distinction from the Society for the Study of Social Problems. Also included is a past president of the ACJS, three of the organization's

Bruce Smith Jr. Award and two Founders Award winners. With reference to editorships the sample includes several who formerly managed the field's primary outlets such as *American Sociological Review* (1), *Criminology* (2), *Journal of Quantitative Criminology* (1), *Justice Quarterly* (1), *Journal of Criminal Justice* (1), and the criminology section of the *Journal of Criminal Law & Criminology* (1) as well as the *Theoretical Advances in Criminology* (1) annual series (1). Table 1 lists the names included in this distinguished group of criminologists.

**Table 1: List of Interviewees**

Freda Adler	Robert Agnew	Ronald L. Akers	Robert J. Bursik Jr
William Chambliss	Francis T. Cullen	Jack P. Gibbs	John Hagan
John H. Laub	Janet Lauritsen	Steven F. Messner	D. Wayne Osgood
Robert J. Sampson	Joachim J. Savelsberg	Lawrence W. Sherman	James F. Short Jr.
Charles R. Tittle			

The purpose of soliciting participation from a select group of scholars rather than electing to draw a random assortment presents an accurate account of the field's evolution through the accounts of a few of its major contributors. Nevertheless, there are a few drawbacks to the sampling methodology. The most glaring limitations result from the experience of the cohort. The changes in the overall access to the pursuit of terminal degrees are a welcome, but recent, development for not only society at large but the field as well. As a result, the sample demonstrates a bias in favor of white males; stated alternatively, the sample does not include any minority representation and is limited in terms of female representation. There are disciplinary biases as well. Almost the entire sample obtained a sociology degree and there are no contributions from psychologists or qualitative researchers. The study opted for a focus on attempting a depiction of the evolution of mainstream criminology. What is lacking are the contrasting opinions of those working on the periphery of the field who are likely to regard the trends of the field in differential terms. This is not to gainsay the contributions of excluded scholars and scholarship. The strength of the sampling frame is that it is inclusive of the major contributors within the primary theoretical traditions of the field and permits a focused



conversation on the state of criminology as it has emerged from sociology. The tradeoff is that this involved some collateral discounting the perspectives of new and emergent contributions of those having entered the field at a later point in its development.

The work associated with selecting the sample having been discussed, attention can now be turned to explaining the scheduling of the interviews. Each of the participants was reached via email. A two paragraph explanation of the project outlining the topics to be discussed, rationale for inclusion, and mention of the name of the dissertation chairman (Dr. Richard Rosenfeld) was included in the initial contact. The mention of this last item was meant to convey the idea that the work underway met with the approval of another recognized criminologist. If a response was given at that point informed consent was then obtained and a mutually agreeable time to talk was established. Five of the interviews were conducted face to face at the 2008 annual meetings of the American Society of Criminology, only two of which were fully completed at the time. The remainder of the interviews were completed via recorded phone calls. The interviews range from twenty-eight minutes to an hour and fifty-two minutes in duration, averaging approximately an hour and fifteen minutes in length.

Prior to conducting the interview a copy of curriculum vitae was collected for each of the participants. A reading of these documents allowed for a more informed interview to be conducted. Items relevant to the interview contained therein include educational background, professional experience, awards, and publication history. A review of publication history allowed for an understanding of respective topical areas of interest as well as a reading of important pieces of literature that may have escaped my attention initially. These preparatory measures were meant to convey, within the context of the conversation, the idea that the time spent with them would not be wasted. As with another effort using a qualitative approach in approaching a sample of scientific elite (Zuckerman, 1977) these efforts paid dividends in the unsolicited compliments received at the conclusion of the interview to the effect that the discussion was productive and the questions worthy of consideration. When references were made during the

course of conversation to these works an understanding of their content allowed for follow-up questions. Items referenced during the interview that were not read beforehand were read later in order to gain a better understanding of the points that were introduced in the context of the interview. A familiarity with interviewees' body of work also provided foundation for a focus on the strengths of each of the participants. Each has devoted energy to understanding unique aspects of the field. All of the questions detailed below (see also, Appendix 1) were asked of all the interviewees. However, additional time was spent discussing issues a reading of vitae suggested each had unique expertise in. Whenever possible, questions were posed in a manner that was relevant to each of their research agendas through citing examples drawn from their body of work. This represents an attempt to pose the themes of the work in more concrete terms through referencing familiar examples.

One of the conditions of participation in the current study was waiving anonymity. It cannot be denied that this condition likely contributed to eight of the scholar's declines. It was recognized at the outset that this was a risk worth assuming, especially as the work makes no pretense to being broadly generalizable. Stripping the interview of these identifiers to afford anonymity would have defeated the purpose of their collection on two grounds. First, the interview is an explicit exercise in exploring the careers and ideas of the sampled scholars. Obscuring their identity would undo the work of tailoring the interview to capture the strengths of each interviewee. Second, consumers of this research deserve to be made aware of identity of these scholars. Being human, each has biases that color their perspective. The work is meant to preserve a record of the interviewee's philosophical stance on their scientific approach for future scholars and document the frank self-assessment of their respective legacies. Each of the respondents has labored over the course of their respective careers to promote a particular perspective. Revealing the identities of the sources is an acknowledgement that each is offering a subjective account.

To ensure that inadvertent admissions made in the context of the interview did not pose the potential for harm to themselves or others when published each participant was offered an opportunity to edit their transcript. The sample varied in how much attention each devoted to reconstructing the thoughts attributed. A few subjected the transcript to a comprehensive reworking, including additional references and recasting thought. This accounts for the italicization and parentheses around the comments included in the results; edits appearing in brackets are those of the author. The majority were content with what was committed during the audio recording. Most edits were minor, relating to transcription errors and the proper spelling of scholars names whom have long since passed. All of the alterations were accepted. This process provided the ancillary benefit of allowing the sample to clarify and extend remarks that may have been initially inchoate. Additionally, the revised transcripts improved the ease of reading through the removal of false starts and run-ons. Those segments hindering the effective communication of an idea (e.g. odd punctuation, misspellings or tangled diction) that managed to somehow make it through the editing process have been “smoothened” out of the final version (see Savelsberg & Flood, 2003). What is reflected in the interview data is a product of the process of interview, transcription, review, and edit.

The interview was separated into four general concentrations: intellectual roots, professional activities and recognition, body of work, and conjectures on the state of the field. Its semi-structured format was meant to allow for a conversational flow. As such, Appendix 1 provides a rough indication of the questions asked, not a verbatim account of the interview protocol. For instance, the interviews seldom adhered to the sequence outlined; oftentimes content from one concentration would be addressed within the context of another question. After conducting a few interviews it became apparent that working strictly from the research instrument as originally conceived inhibited the natural progression of the conversation. Rather than interrupt a given line of logic it was decided that it would be best to let the interviewee guide me to the next thematic area. This permitted a continuity of thought. In general the structure and

flow of the discussion reflected the chronology of their careers. The instrument begins with questions pertaining to their training and early career. Concluding questions asked for thoughts on the current state of the field and predictions of its future. Not included on the interview instrument are the introductory remarks and concluding question.

Prior to conducting the interview a brief introduction to the overall project was offered. This was not scripted but generally included mentions of the oral history approach, “taking stock of the field”, and paradigm. Several took this as an opportunity to ask clarifying questions. Generally, this preamble served to inform the interviewee of what kinds of content were of interest. The concluding question asked each if there was any content they think was overlooked in the discussion or if there was anything additional they wished to contribute. A few introduced ideas at that point and others offered to include additional thoughts in their edits of the transcript in the event they presented themselves later. The content of the concentrations comprising the body of the discussion will be outlined in the paragraphs below.

Each respondent was first asked for an account of how they came to be a criminologist. Many answered this question with an autobiographical account of the mentoring they received. In order to bring a dissertation into existence a faculty sponsor must be secured and the subject matter must demonstrate potential to inform the discipline within which they happened to be studying. Due to the limited number of criminology and criminal justice programs operating during the sample’s formative years many migrated to the field from various disciplines. Only three of the sample graduated from criminology or criminal justice programs. The remainder dissertated as sociologists (11), social psychologists (2) or policy analysts (1). The perspectives each found in their graduate department influenced their early career in terms of determining both their research agenda and departmental location. Within these graduate programs they were given instruction and advice on what questions to explore and on the accepted methods with which to approach them. Those who did not have the benefit of a close mentor or arrived within criminology at a later point drew inspiration from figures found on their bookshelves.

Respondents were asked for the names of these scholars. Interviewees were then asked for an account of the theoretical and methodological training they were exposed to and that they now introduce to their students. This question informs on four important points. First, the stability and change reflected in the curriculum over the course of their careers will account for criminology's evolution. Second, answers to this question indicate the sources of the field's influence, its widening or narrowing of focus. Third, agreement between the respondents will indicate the extent to which there is consensus in the field on these points. Fourth, if there is variation between what is taught by those currently teaching from a sociology faculty and those within specialty departments it indicates different disciplinary emphases.

The next concentration (professional activities and recognition) generally corresponds to the thematic content of boundaries. Two primary questions were asked in this portion of the interview. First, each was asked to articulate their reviewer or editorial philosophy of what fits within the confines of criminology. This question was aimed at determining what topics, theories, and methods are accepted by the field as relevant, as distinguished from those that are not. Secondly, each was asked for an account of to what they attributed the success of their career. This was accomplished primarily through a discussion of the awards they had earned. In some instances respondents mentioned the content of the citation that was read when the award was bestowed. Others mentioned the work they suspected earned them distinction. For those that demurred over my asking, as nearly all did in some fashion, I inquired as to what contributions they have offered in which they took the most pride in. This question was founded on the assumption that these awards acknowledge a finding, approach, or the framing of a research question the field recognizes as integral. In a limited sense these contributions can be thought of as truths. Discussion of their work provided a segue into the next series of questions.

The following cluster of questions (body of work) asks respondents for a self-appraisal of the content of their careers. Interviewees were asked for a summary of the thematic content their work addressed over the duration of their careers. Is there a single question or several that inform

the totality of their work? Ultimately, this question aimed at determining the scholars' point of reference; how are they framing the essential issue(s) they address? This question was followed by an inquiry as to where they would place the narrative in relation to the epicenter of the field. In an indirect manner this question asks the respondent what they regard as the basic question(s) the field is attempting to answer. Typically in reference to the earlier discussion on the awards or success the interviewee accumulated they were next asked for an account why their work was successful and where it may have met with criticism. The successes indicate, again, what the field has come to value and the process by which this comes to become part of the field's basic knowledge. Several indicated that their work met with success not when initially published but at a later point. This brought into the discussion speculation as to why this may have been the case. Accounts of where contributions met with criticism and how it was rejected immediately or eventually accepted suggest core tenets of the field. The field, like any other community, will tolerate only so much deviance. Outlining the terms of the debate also served as indication as to how ideologically driven the field is. Lastly, respondents were asked about their interaction with co-authors they had published with. Is consensus difficult to achieve on theoretical or methodological grounds based on the training of each of the co-authors? These predispositions indicate disciplinary boundaries in terms of the priorities each participant emphasized.

The final concentration (conjectures on the state of the field) encouraged the interviewees to offer an assessment of the current and projected condition of the field. The first question asked respondents for thoughts on whether there are any internal or external threats to the continued success of the field and what they might be. Those who mentioned a reduction in research funding were asked to what extent the logic expressed above (failure to predict crime decline producing a reduction in legitimacy in the eyes of policy makers and funding sources) was consistent with their view. At this point several were also asked for their impressions on how influential ideological biases in terms of research funding decisions are on shaping the field's research and what potential policy solutions are typically recommended and accepted. Next,

respondents were asked for an account of what the field's fundamental assumptions or empirical foundation is. This indicates an opinion on whether the field has settled upon a perspective or a set of facts that they regard as foundational. A follow-up question asked whether they regarded these as being unique to criminology. The third subset of questions sought reflections on the departure of criminology from sociology. Is this cause for celebration or worry? What are the implications of this development? Will criminology have enough ideas to sustain itself, absent its association with sociology? The final questions asked interviewees for an account of lingering questions the field should give some consideration to addressing. What does the future hold for the field in terms of its content? Are there any trends emergent? This line of inquiry is aimed at finding where the field is evolving through determining where research is either being generated or is most needed.

#### *Quantitative Component*

The dataset collected by a team of researchers in the sociology department at the University of Minnesota (Savelsberg, King & Cleveland, 2002; Savelsberg and Flood, 2004; Savelsberg, Cleveland & King, 2004) serves as the centerpiece of the quantitative portion of the dissertation. The Savelsberg dataset was amassed with the primary end of determining how criminology's assuming a specialized academic orientation, complete with its own independent academic and bureaucratic structure, affects its science but contains several variables that inform the research question that is the focus of the current work. More importantly, it is the only dataset that is inclusive of all the criminology literature appearing in its major journals over the decades when criminology emerged into its own.

The dataset is comprised of 1,612 articles appearing in nine peer-reviewed journals (Table 2) from 1951 to 1993. The outlets include general sociology journals in addition to a group specializing in criminology and criminal justice content. Journals were selected for inclusion based upon their high ranking in terms of impact, as reflected in citation frequency (Allen, 1990) or on the basis of their reputation within the scholarly community (Shichor et al.

1981). The inclusion of the field’s highest rated journals and those catering to the wider audience of sociologists suggests that this dataset includes the most sophisticated research the field produced over its time span.

**Table 2: Academic Journals Included in the Savelsberg Dataset 1951-1993**

American Journal of Sociology	American Sociological Review	Criminology
Journal of Criminal Law & Criminology	Journal of Criminal Justice	Journal Research in Crime & Delinquency
Law & Society Review	Social Forces	Social Problems

The three criteria for article inclusion are enumerated as follows: 1) research articles or presidential addresses (i.e. research notes and discussions are omitted) 2) deal with crime, delinquency, or social control—either formal or informal—directed at curtailing crime or delinquency and 3) concerned with the contemporary United States (i.e. historical accounts and those applying to foreign countries are excluded). The selection of publications is comprehensive in its scope. The only articles appearing within the outlets listed that have been excluded are book reviews, commentaries, and rejoinders. Nearly every article having a bearing on matters proximate to the explanation of crime and its control have been included in the population from which the random selection has been drawn.

Following on the final criteria of inclusion mentioned, most of the authors in the dataset are Americans. However, this is not an admission that foreign authors and data from nations other than the United States are excluded. Scholars publishing from foreign universities who offered results whose generalizability to a domestic understanding of the causes and control of crime was not explicitly denied are part of the dataset. Similarly, analyses of data from foreign countries have been included, again, provided their applicability to the United States was not explicitly denied. In fact, nearly one in six (16%) of the articles sampled in the latter time frame (see below) draw upon international data. The work is not intended as an account of the proceedings of criminology worldwide however.



### *Extension of the Savelsberg Dataset*

The limitation of the Savelsberg dataset with reference to the present exercise is that it does not include content from several specialty or interdisciplinary journals that began publishing toward the conclusion of the time period covered in the primary dataset. Journals brought into existence following the conclusion of their efforts were also, for obvious reasons, excluded as well. Updating the dataset is necessary if we are to understand the scholarly implications of the departure of criminology from its sociological lineage in a more contemporary context. The remedy proposed involved applying the article selection criteria utilized in the primary dataset to an additional set of academic journals (Table 3). The extension of the data set also includes updated entries from the nine initial journal outlets. What follows is an explanation of the inclusion of additional journal outlets, the sampling of criminological contributions from these volumes, and the coding of variables.

**Table 3: Additional Sources Appended (Year of First Volume in Parentheses)**

Annual Review of Sociology (1975)	Crime and Justice: A Review of Research (1979)	Journal of Quantitative Criminology (1985)
Justice Quarterly (1984)	Theoretical Criminology (1997)	

The research cited in justification for the selection of the nine journals collected by Dr. Savelsberg (Allen, 1990 and Shichor et al., 1981) ranked both journal citation counts and regard within the profession, as measured by ratings offered by a sample of the field's practitioners. These journals, therefore, do not represent a simple enumeration of those ranked one through nine in terms of their impact factors. For example, *Crime & Delinquency* was ranked as a top five publication by Shichor et al. (1981) but was not included in the data. It can be inferred that a few of the journals showcasing research on specialized topics that ranked higher in impact scores were omitted as a result of their covering more esoteric subject matter, thus garnering less attention from the sampled professionals. The work of Savelsberg was intended, as is the contemporary sampling efforts, as a reflection of mainstream research; it aimed at the field's

center mass of research rather than peripheral concerns. The present work therefore made similar judgments in excluding similar journals catering to narrower readership and topical matters such as *Criminal Justice & Behavior*, and *Journal of Interpersonal Violence* in addition to those containing a greater proportion of foreign content (*British Journal of Criminology*) that rank highly in terms of their impact factors. The current work makes generalizations based on a sample of what are widely deemed to be respected mainstream journals. Characterizations of the field's research can thus be safely offered in terms of the trends of conventional research conducted by a maximum breadth of the field's practitioners and intended for a broad cross-section of its consumers.

The additional five journals include one general sociological journal and four with more specialized content. Expanding the scope of journal inclusion is recognition of the reality of the professional emergence of criminology as a field of study structurally distinct from sociology, complete with its own journals. To the Savelsberg dataset criminological and criminal justice articles appearing in fourteen journals (9 original outlets noted in Table 2 + 5 listed in Table 3) from 1993 to 2008 have been added.

Prior to discussing the specific variables collected one critically important difference in the appended dataset requires additional explanation. The dataset compiled under Dr. Savelsberg's direction was the fruit of a prodigious effort. Duplicating this enterprise, with the benefit of its research team and funding expenditure, is far beyond the capacity of the current undertaking. Rather than attempt to collect the peer reviewed criminology literature in its entirety (i.e. the population) the present effort has drawn a random sample of articles from the population of fourteen journals appearing from 1993 to 2008.

Applying a similar definition of what constitutes a criminologically oriented journal submission as Savelsberg produced a population of 2,680 articles appearing over the latter time frame. A reading of the table of contents of each volume, and in instances where there was ambiguity, the abstract, yielded a compilation of all those articles fitting the criteria of interest.

These individual articles were then cataloged by year. From this population a sample of 501 articles (19%) was selected for coding. The number sampled per year is consistent with the year's representation within the population. For example, the number of randomly sampled articles increased from 1996 to 1997 as a result of *Theoretical Criminology's* introduction to the sampling frame. A random number generator was used to determine which specific articles within each year were to be coded on the variables outlined below. The inclusion of the additional articles brings the total number of articles content analyzed to 2,109.

What this yields are estimates of the content appearing in the criminological literature. The differing strategies will add a layer of complexity in comparing the results extrapolated from the companion dataset with the base dataset. Rather than being a simple apples-to-apples comparison the appended data will require a more nuanced apples-to-orchard comparison. This will affect the interpretation of nearly all the graphs included in the chapters canvassing the substantive results. Nevertheless, this was thought to be an acceptable strategy given that special issues—volumes devoted to specific topical concerns—are a rarity within the field. This allows for generalizations on the field's discourse to be safely drawn from the randomly sampled articles as the bias of clearly discernable patterns in publishing poses negligible risks of coloring the results. In terms of interpreting the results there are dramatic drops in the overall numbers depicted in the figures beginning in 1993. The reader is encouraged to focus attention on the relationship between the categories represented with the line graphs rather than the total N. Proportions are used throughout the discussion in order to overcome the disparity in raw counts seen in the visual evidence. This provides for a standardization allowing for comparison between data coding efforts.

With the assistance of a few source materials totaling less than ten pages and additional guidance provided by Dr. Savelsberg an attempt was made at establishing interrater reliability through tabulating the percent agreement on the coding of variables selected for analysis in the dissertation (see below). An effort was made to read a selection of articles coded from 1951 to

1993 to better establish an understanding of how the codes were operationalized. When a consistent level of intuitive familiarity with the application of the coding scheme was established through reading and checking the coding of numerous articles a random selection of twenty publications was then drawn and coded. These codes were then contrasted against those seen in the Savelsberg data. The concordance rates are as follows: topic (100%), level of analysis (85%), unit of analysis (80%), study orientation (80%), data sources (98%), method of analysis (65%), and theory (91%). With the exception of the method of analysis comparison these are above the conventionally accepted minimum of 70% agreement (Carmines & Zeller, 1979). This represents an attempt to make the coding and interpretation of variables consistent throughout the time frame. The disparity with regard to the method of analysis resulted from incongruence in terms of agreement over multivariate and interpretive classifications. The remote prospect of improving the concordance, in light of the nearly two decade separation between data coding efforts, did not seem to merit the expenditure of additional energies.

In a similar manner, interrater reliability was established with roughly the same measure of success with the subsequent round of data collection. Each of the 501 articles was first coded by the author on the variables listed. Two coders were then recruited and offered written instructions on the operationalization of the coding structure. A random sample of 25 articles was then selected and coded. Comparisons were then made in terms of the percent agreement between the three coders. All but three variables (study orientation, level of analysis, and unit of analysis) exceeded the minimum acceptable percentage agreement.

A second round of coding addressed these lingering inconsistencies on the coding of these three variables. A review of the inconsistencies produced a list of articles in which there was total disagreement between the three coders. A one-hour phone conversation attempted to establish greater clarity in terms of improving convergence through pinpointing the source of disagreement. At the conclusion of the discussion 10 additional articles were randomly selected for coding on the three variables. Unfortunately, after this repeat effort two variables evidenced

persistent disagreement (level of analysis and unit of analysis). The disagreements appear to stem from inconsistencies in terms of differentiating the unit and level of analysis from the articles; research often measures variables at one level while making generalizations at another.

The quantitative data are comprised of fifteen variables (see Table 4). Eleven of the variables are drawn from the original dataset (*Capitalized*) and extended in the appended data set. Four additional variables have been created to track developments within the field subsequent to the construction of the original dataset (*Capitalized and Italicized*).

Journal of Publication, Type of Journal, Year of Publication, Theoretical Approaches, Topic of Article, Unit of Analysis, Level of Analysis, *Micro Subtype*, Method of Analysis, *Multivariate Subtype*, and *Temporal Orientation* are all more or less self-explanatory with the assistance of Table 4. This lists all the variables in the left column and the corresponding codes in the right. Those four coding schemes requiring additional elaboration are described in more detail immediately below.

**Falsification of Theoretical Approach.** This is a 3-point ordinal variable that rates a publication's support of a theory from rejection to neutrality to confirmation. A publication that addresses multiple theories has multiple ratings. These codes are drawn largely from the conclusions section. A publication invoking strong language to describe the findings denotes rejection ("fail to affirm") or confirmation ("supports", "affirms", etc.). Equivocal language ("unclear") or narratives emphasizing in equal portions the strengths and weakness of the finding's support for a hypothesis or differing aspects of a theory are coded as neutral. Publications which are do not represent tests of theoretical perspectives but offer extended commentary on their respective value have been coded here as well. Admittedly, these contributions do not represent falsification in the strict denotative sense but do stand as an indicator of how receptive the field is to contending perspectives.

**Purpose.** This variable captures the overarching intent of the publication. Theory construction is comprised of those articles intending to articulate a new conceptualization of a

given phenomenon. Those attempting to simply document a previously unexplored topic fall into the second category of establishing an empirical finding. Those articles which articulate a theoretical perspective and then evaluate its claims to truth through testing propositions using empirical data are considered as theory tests. Those that document or elaborate upon the application of extant or proposed methodology or statistical tools comprise the fourth category. Finally, a catch-all (“other”) includes those that remain. This variable is intended to allow for a determination as to how much of the field’s research is devoted to documenting, as opposed to explaining (i.e. theorizing), on the subject of crime and its control.

Study Orientation. Descriptive articles simply establish the prevalence of a previously unexamined phenomena. Its primary aim is answering “what?” Analytic/predictive contributions attempt to answer the question of, “why?” or account for variation. Modeling involves the creation of hypotheses to be tested. Publications seeking to forecast represent attempts to predict future behavior. Literature reviews entail the summation of a body of accumulated understanding. Finally, “other” catches all that remains. This variable has been coded from an interpretation of the analytic or methodological orientation of the given publication.

Data Sources. These are both primary (gathered by the author) and secondary sources of empirical evidence. The classifications are not mutually exclusive. For example, there may be two or more sources of primary or secondary data used in a given publication. For purposes of the current work these categories have been combined. The investigation is concerned less with whether the data were gathered *de novo* than with what types of data investigators are using to bolster their claims.

**Table 4: Variables and Coding**

Journal of Publication	See Tables 2 & 3 for a listing
Type of Journal	<i>Annual Review of Sociology, American Journal of Sociology, American Sociological Review, Social Forces, and Social Problems</i> are coded as “sociology”. All remaining are “interdisciplinary”
Year of Publication	year in which the article appeared
Theoretical Approaches	<p>anomie, strain (Merton), learning/differential association, illegitimate opportunity (Cloward &amp; Ohlin), subculture (autonomous—Miller), subculture (reaction—Cohen), labeling, neutralization, early socialization deficiency, drift/episode, biological/genetic, free will/rational offender, class struggle (Marxist/conflict), historical/constructivist, control, power/control, feminist, social disorganization, legitimization of violence, and other</p> <p>The data drawn from articles published from 1993-2008 also include: self-control, life-course, general strain theory, Moffitt’s developmental taxonomy and reintegrative shaming as well as the older approach of and routine activities theory.</p>
Falsification of Theory	Rejection/Neutral/Confirmation of the theories above. A substantive inclusion was required for both theoretical variables to be coded; a mere passing mention did not suffice.
Topic of Article	<p>criminal behavior , victimization experience, informal/community control, formal community mechanism , intra-agency relations, inter-agency relations, academic behavior, meta-methods, meta-theoretical, public opinion, fear of crime, and other</p> <p>These represent the dependent variables the articles are accounting for.</p>
<i>Purpose</i>	theory construction, establish empirical finding, theory test, discuss methods/statistics, or other
Study Orientation	description, analytical/predictive, modeling, forecasting, literature review, or other
Data Sources	Personal experience, case records (drawn from official data), official statistics, survey, ethnographic, systematic personal interviews, laboratory observation, simulation, psychological testing, content analysis, other public sources, or other unpublished
Unit of Analysis	individual (MI), interaction/small group (MI)—2 to 14 people, social role (MI)—e.g. gang, gender, or race oriented publications, crowd, community/neighborhood (ME), census tract (ME), court district (ME), formal organization (ME)—these include bureaucratic agencies such as police departments and jails, city (ME), state (MA), society (MA), or method
Level of Analysis	Micro (MI), meso (ME), macro (MA), or muti-level. These categories are aggregations of the codes detailed above.
<i>Micro Subtype</i>	individual, small group (2 to 14 people), or interaction
Method of Analysis	bivariate, bivariate with one control, multivariate, interpretative, historical/comparative, or other
<i>Multivariate Subtype</i>	Ordinary least squares, or other
<i>Temporal Orientation</i>	Cross-sectional or longitudinal

Much of the analysis relies on a descriptive account of the evolution of the field based on these indicators. This account documents where trends are emergent and where they subside. This has been accomplished primarily through the use of the chi-square statistic to determine if statistically significant trends are present. The problem of filling missing or inadequate cells in the analyses that follow presented difficulty that was overcome through aggregating categories (on a few occasions) and years (in all cases). To determine whether the differences in trends over time reflected in the representative figures are statistically significant individual years were combined into larger increments ranging from two years to fifteen including yearly increments in-between. As a result of the chi-square statistic being sensitive to larger numerical values, in the analyses that follow the yearly cycle requiring the least aggregation to meet with the conventional levels of statistical significance (i.e.  $p < 0.05$  and  $p < 0.01$ ) will be reported.

The coding of the theoretical variable aggregated the twenty-six perspectives into four classifications: structural (Marxist, anomie, power-control, routine activities, strain, illegitimate opportunity, labeling, general strain theory, feminist, historical/constructivist), cultural (differential association, reintegrative shaming, Miller subcultural, Cohen subcultural, legitimation of violence), control (control, rational choice, neutralization, self-control, life-course, drift/episode, early socialization deficiencies, social disorganization), and bio-social (biological/genetic, developmental taxonomy). Any given article could contain up to four theoretical constructs. The category of “other” has been excluded from this classification scheme. The presence of single case within multiple categories violates an assumption of the chi-square test that an observation appears in only a single cell. As a result an ANOVA test was conducted to determine if the differences in trends were statistically significant.

The following two chapters are oriented around thematic topics. The work, for the most part, introduces an analysis of the quantitative data prior to bringing the qualitative evidence to bear. The former is conceptualized as providing the structure of the work; the latter provides the



context in which the results should be properly considered. The numerically oriented evidence is adduced with a content analysis of the field's research. The interview data are intended to enrich our understanding and interpretation of its meaning. Intertwining the types of data considered is intended as an offering of a global account of the field's development through their distinct perspectives.

### *Chapter III: The State of Theory & Methods*

The primary focus of the work confronts the substantive concerns raised by Savelsberg and Sampson (2002) to the effect that criminology lacks an intellectual core, conceptualized here as an underpinning theoretical perspective and methodological unity. Paradigm, if nothing else, is the demonstration of consensus on a general theoretical orientation with a companion methodology that aids in the deciphering of empirical events. The chapter, therefore, is partitioned into two portions, theory and methods. Prior to elaborating upon the specific findings a summation of the results is presented. This is to apprise the reader of the larger mosaic before delving into the minute accounts of which this is comprised.

The theory portion of the chapter is divided into five general sections. First, an account of the field's use of theory in its peer-reviewed literature is presented. The overall contributions are split nearly evenly between those with and without a theoretical mention. The overall pattern is U-shaped with a slightly downward trajectory over the course of the study. Secondly, an examination of the appearance of theoretical approaches reveals there is a handful that can be considered mainstays, but none that are inarguably dominant. The catch-all category of "other" is a potential exception. This indicates the field, with its single identifying characteristic being a focus on explaining a dependent variable, resembles an academic tabula rasa of sorts; it is a veritable Rorschach onto which participants can project ideologies. Thirdly, a review of the trends in popularity of specific theories indicates a cyclical pattern. The fourth component includes responses from interviewees on why this is the case. Two general explanations were proffered: the invocation of any given theory is dictated by the larger cultural context in which the field is operating and the field's inability to explicitly falsify an approach means that theory is never completely beyond redemption; this is referred to as "falsification by atrophy". Fifth, and lastly, it is posited that while the field has lacked for a paradigmatic theory one may be emerging. The findings are unambiguous: at present criminology lacks a theoretical consensus.

A reading of the evidence related to the level of methodological consensus tells a more inconsistent story however. The peer reviewed research over the period is weighted decidedly in the direction of being quantitative, multivariate, and individual level. These wide margins are reduced over the latter sixteen years of the study, indicating that the addition of criminology and criminal justice journals contain alternately oriented research. The reactions from the sample interviewed suggest that the field is much less unanimous in terms of its methodology however. While it has developed unique methodology to address the non-normal distribution of crime the field defiantly guards its prerogative to draw from any and all methodology that may inform a more complete understanding of its subject matter. The field does recognize that there are “evolved preferences” in terms of the standards it imposes though. The incongruence between the findings suggests that the quantitative data were broadly measured in reference to the narrower definitions imposed by the respondents. This suggests grounds for future research on the field’s research.

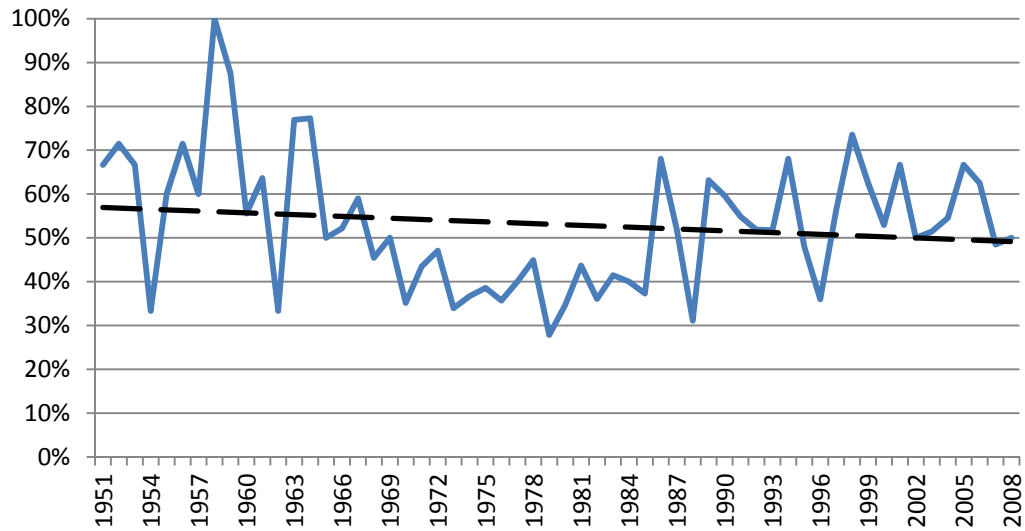
### *Theoretical Consensus*

#### *Generating Empirical Findings and Theorizing*

A review of the trends apparent in the field’s peer reviewed literature suggests that despite the concerns of at least two interviewees (Adler and Bursik) the presence of theory has endured. Referenced within the 2109 articles are 1824 theoretical perspectives, or an average of 0.86 references per article. These references, however, were not evenly distributed throughout the data; more than half the articles (N=1092 or 52%) lacked a substantive reference to a theoretical point of view. On the other hand, the number of articles including at least a single reference to a theoretical perspective (N=1017 or 48%) parallel the field’s overall output with consistency over the fifty-eight years included in the study (Pearson’s  $r=0.88$ ). Nearly a third (30%) of the journal publications included a substantive reference to a single theory and the remainder included references to two (9%) or more (9%) theoretical perspectives.

Upon closer review, however, the suspicions raised earlier appear to have some merit. When percentages (# of articles including theory/total articles appearing in a given year) are plotted over time it is evident that the distribution is bimodal, with a slightly downward trend (Figure 1). From 1968 to 1985 the proportion of research invoking at least one theory dips below the overall study period average. In only four of seventeen years prior to the low ebb of theory is its proportion below the average. Alternatively, in eleven of the concluding twenty-three years the same trend is repeated. The trend-line depicted in the figure maintains relative stability as a result of these under-representations evident in the latter period being just slightly under the overall average. Additionally, the mathematical average over the period is also buoyed by several years in which theory is especially well represented. The early portion of the time frame was marked by the relative prevalence of theory. Theory began a gradual weakening through the mid-period prior to witnessing an erratic recovery through the approximately last quarter century covered in the analysis. One potential explanation for the trend reflected in the data is that the field in its earlier phase was devoted to generating theory by necessity. At this point the field lacked the financial wherewithal to generate data and chose to devote its resources to proposing theoretical explanations. Attention then shifted to evaluating theoretical expectations through generating empirical evidence without referencing theory per se. The inundation of research funding swung the pendulum in the direction of exploring and documenting the contours of crime. The sustained dip below average is also roughly consistent with the introduction of Law Enforcement Administration Assistance (LEAA) dollars into the field which lends credence to the hypothesis that governmental funding influences the field's research agenda.

**Figure 1: Trend in Percentage of Articles including Theory**



*Dominance of Theoretical Perspectives*

The scholarship of the field generally reflects a measure of appreciation for having its research agenda informed by theoretical constructs. The question then becomes: which of its perspectives, if any, is dominant? The answer to this question will be arrived at through approaching the data from two alternate angles. The first involves determining which theoretical perspectives maintain a consistent presence in the literature; those that are absent, and for longer durations, can be considered less integral than those with relatively more presence.

In an additional analysis that is not shown the overall absence of the twenty-six respective theoretical constructs from 1951 to 2008 is depicted. The figure includes 1280 cells to be filled with values. Nearly half (46% or 568) denote the absence (i.e. zero count) of a theory during a given year. Only six are missing for less than a third of the fifty-eight years (listed in order of ascending absence: other, differential association, control, rational choice, strain, and Miller's subculture). An additional six (anomie, Marxist, illegitimate opportunity, labeling, social disorganization, and biological/genetic) are absent from between one-third and one-half of the time frame. The remaining eight are missing for more than half the time period covered in the study.

The six constructs coded from the concluding sixteen years fared slightly better. Two-thirds of the perspectives (routine activities, self-control, general strain, and life-course) were missing for less than half the time period accounted for. Of the twenty theories that overlap data collection efforts seven with the most cells with missing values (listed in order of ascending order of prolonged absence: early socialization deficiencies, neutralization, legitimation of violence, historical/constructivist, power-control, drift/episode, and feminist) are also those with the longest periods of continuous absence. Of the remainder, five are missing for periods of between six and eight years, and seven for half a decade or less. In general, this indicates that there are a cluster of theories that are chronically absent (roughly one-third), another of those that maintain a relatively consistent but tenuous presence (roughly one-third) and a few that can be considered to be mainstays within the field's literature (roughly one-third).

The second manner in which an evaluation of the field's theoretical perspectives can be conducted is in determining the relative frequency of a perspective in terms of raw counts. The 1902 mentions of theory have been divided within the respective categories and can be seen in Table 5. The pattern that emerged in the review above is also evident in this context. Eight of the theories garnered more than one hundred substantive mentions (listed in descending order: other, differential association, control, rational choice, strain, Miller subcultural, labeling, and illegitimate opportunity) and together comprise nearly two-thirds of the total accumulated (64%, N=1219). Marxist theory, social disorganization and anomie fall just below the 100 threshold. With 315 mentions the catch-all category of other more than doubles the tally of its nearest competitor. This classification includes a broad assortment of theoretical constructs beyond the expansive list enumerated in the original dataset including theories that account for the criminal justice system's reaction to crime. This indicates that the field has been, to a significant extent, informed by an array of traditions beyond its immediate intellectual confines. On the other end of the continuum are six theories (feminist, neutralization, power-control, historical/constructivist,

drift/episode, and legitimation of violence) that constitute just over seven percent (N=135) of the total mentions.

**Table 5: Theoretical Representation 1951-2008**

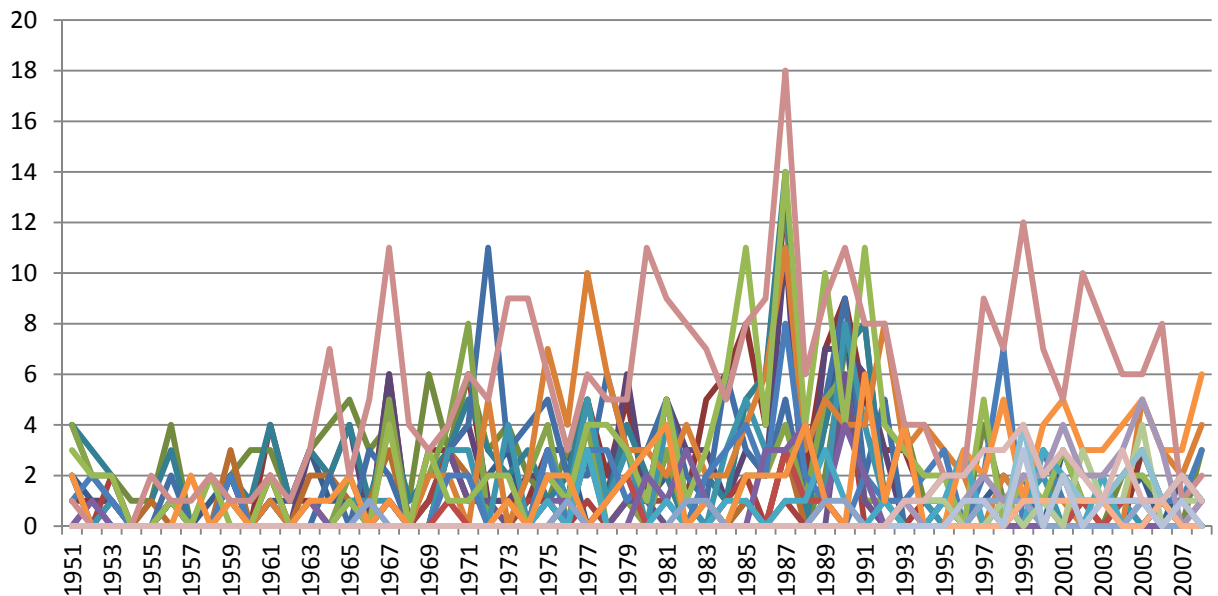
Other	315	Anomie	75	<i>Theory Added in Appended Data</i>	
Diff. Assoc.	155	Biological/Genetic	73	Routine Act.	32
Control	143	Early Soc. Def.	70	Self-control	29
Rational Choice	141	Cohen subculture	46	Life-course	17
Strain	130	Feminist	31	GST	16
Miller subculture	121	Neutralization	31	Reint. Shaming	11
Labeling	112	Power-control	28	Dev. Taxonomy	6
Illegit. Opp.	102	Hist./Constructivist	18		
Social Disorg.	95	Drift/Episode	17		
Marxist	78	Legit. of Violence	10		

As a result of being tallied for only sixteen of the fifty-eight years none of the six additional theories made significant contributions to the overall count. However, four of the six earned sixteen or more mentions (routine activities, self-control, life-course, and general strain). In fact, the thirty-two mentions of routine activities within this truncated time span outstrips the six least appearing theories among the twenty included in both the original and appended datasets. The fact that it gathered the number of mentions it did decades after its introduction (Cohen & Felson, 1979) suggests that it is a construct that the field employs with regularity. With just three fewer mentions, Gottfredson and Hirschi's (1990) self-control theory has managed to inform a nearly equal number of research efforts within contemporary criminology. Sampson and Laub's (1993) life-course theory and Agnew's (1992) general strain theory tallied approximately half the mentions of the aforementioned theories. Rounding out the group are Braithwaite's (1989) reintegrative shaming and Moffitt's (1993) developmental taxonomy.

*Trends in Theoretical Popularity*

When looking at the presence of theory overall from 1951 to 2008 it is apparent that no single theory demonstrates clear supremacy over the others overall. The question now shifts to one focusing attention on short-run trends. Are there periods in which a specific theory takes precedence over the remainder? How long does its popularity last? A look at Figure 2 reveals that there are few periods in which this is the case. Most of the peaks seen in the figure represent the largest category, “other”. The three exceptions worth pointing out are labeling’s peak in the early 1970s, rational choice’s later the same decade, and control theory’s at several points in the 1980s. The overlap of all twenty-six theories is so apparent that there is little point in differentiating each within the figure (e.g. there is no legend). The illustration is proffered as a depiction of the absence of theoretical consensus within the field for anything more than a brief period; the fact that it is not saying much is telling. In a characteristically precise formulation Jack Gibbs describes this state of affairs as one of “epistemological dissensus” in the course of our discussion.

**Figure 2: Trends in Theory 1951-2008**



In a separate set of analyses (not shown—the graph is equally muddled) the percentages of each theoretical perspective is plotted against the remaining twenty-five. A similar pattern to that



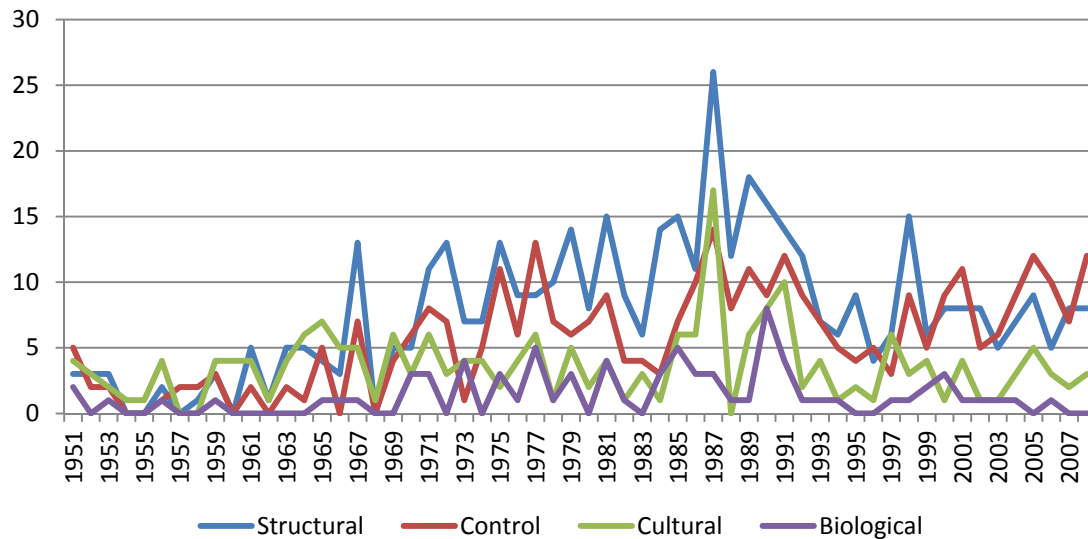
which is demonstrated in the preceding emerged with this analysis. On average, nearly one-fifth (18%) of the theoretical mentions annually are attributable to the category of “other”.

Differential association is the only remaining theory to average greater than one of ten mentions per year (11%) over the duration of the study. Eight others average better than five percent per year: routine activities (7%), Miller’s subculture (7%), rational choice (7%), control (7%), social disorganization (6%), self-control (6%), strain (5%) and labeling (5%). The remaining sixteen theoretical perspectives are underrepresented, in that each contributes an average of 4% or less of the mentions annually. As with the pattern with the theory tallies (Table 5), nearly all of the peaks in representation are those of the “other” category. The early dominance of several theories in the introductory decade (1951-1960) is viewed with skepticism. Over this period an average of just over six articles were collected annually. The percentages advantage those few theories that were mentioned by virtue of a smaller denominator. In twelve of the remaining forty-eight years “other” theoretical constructs represent greater than 25% of the mentions (’64, ’66, ’68, ’73-’74, ’80, ’82, ’97, ’99, ’02-’03, and ’06). The only named theory post-1960 that earned what could be considered predominance in the field was labeling’s constituting nearly a third (31%) of the theory mentioned in 1972. This was the only theory to exceed a representation of 25% during the last forty-eight years of the study time span.

To ease interpretability the twenty-six theories have been grouped into four categories: structural (Marxist, anomie, power-control, routine activities, strain, illegitimate opportunity, labeling, general strain theory, feminist, historical/constructivist), cultural (differential association, reintegrative shaming, Miller subcultural, Cohen subcultural, legitimation of violence), control (control, rational choice, neutralization, self-control, life-course, drift/episode, early socialization deficiencies, social disorganization), and bio-social (biological/genetic, developmental taxonomy). Aggregating the data thus reduces the number of observations from 1902 to 1072. This reduction reveals that nearly half the references within the data are paired with one or more theories within groups or is inclusive of the category of “other” and therefore

omitted from the analysis. Enumerated in descending order of total overall appearances are structural theories (N=449 or 42%), followed by cultural (N=334 or 31%) and control theories (n=210 or 20%), and lastly, biology (N=79 or 7%). In general the ordering throughout the time period follows the same pattern, with the exception of control eclipsing structural theory for the lead in 2000. The pattern is similar when the percentages are plotted against one another; from 1996 to 2008 control theory surpasses the structural theory representation in eight of the thirteen years. Excluding the eleven years in which observations were less than ten, there are fifteen years in which one of the four approaches met or exceeded half of the theoretical representation. Structural theories accounted for twelve of these and the balance represented by control theory.

**Figure 3: Four Theoretical Classifications**



In order to test whether the trends within groups were statistically significant over the time period ANOVA analyses were conducted, the results of which are summarized in Table 6. At two-year intervals the cultural and control theories differ over time. At the five-year mark structural theories evidence statistically significant differentiation. Finally, biological theories fail to meet with the standard definition of statistical significance that the other three met ( $p < .001$ ).

**Table 6: ANOVA Summary**

		Sum of Squares	df	Mean Square	F	Sig.
Structural Theories	Between Groups	4.28766	11	0.389787	2.341259	0.007
	Within Groups	349.1215	2097	0.166486		
	Total	353.4092	2108			
Cultural Theories	Between Groups	11.15049	28	0.398232	4.655087	0.000
	Within Groups	177.9391	2080	0.085548		
	Total	189.0896	2108			
Control Theories	Between Groups	10.53405	28	0.376216	2.892143	0.000
	Within Groups	270.5707	2080	0.130082		
	Total	281.1048	2108			

*Commentary on Cyclicity*

Briefly stated, the qualitative findings affirm the results reflected in the content analysis. There is no single theoretical framework which commands unanimous allegiance from the field's scholars. However, there are a few that are acknowledged as providing the essential elements of a criminological perspective. These are traditions of thought that have endured in one form or another for a lengthy duration; they are more or less a consistent part of the conversation of criminology. The recent surge in efforts at "taking stock" suggests that at the center of the field lie a few conceptual tools which have withstood the rigors of challenges directed at them. When asked to place his primary theoretical contribution in relation to the field's theoretical core Ronald Akers replied:

Remember, it was two years ago when I think Frank Cullen had this *Advances in Criminological Theory: Taking Stock* [Cullen et al., 2006] the empirical status. He opened up with "here is the core of criminology." The first three articles were on what he called the core. One was on strain, by Bob Agnew. One was on control theory by Mike [Gottfredson] and Travis [Hirschi]. And one was on social learning theory by myself and

[Gary F.] Jensen. Without seeming immodest or self-serving here, that makes sense to me. I think of this at least at the social psychological explanatory level. What is at the core of the field at that level? I think those are the three: strain, control theory and learning theory. Now these other theories certainly make contributions: labeling, reintegrative shaming, conflict theories and others. But they're not as central as explanations of criminal and deviant behavior as those three. So that would be my answer.

This is indication that there are fundamental explanations or approaches to the phenomena of crime that criminologists repeatedly seek guidance from. The three formulations mentioned—strain, control, and social learning—have deep roots within the sociological tradition. The former two trace their lineage to Durkheim and the latter to Edwin Sutherland. The others mentioned in the quote—labeling, reintegrative shaming, and conflict—are important in that they have informed the conversation. But ultimately they have proved less resilient, their impact more ephemeral. They are characterized as being of peripheral influence.

The recurring pattern of these theories being proffered, criticized, rehabilitated and revisited suggests that criminology is engaged in an accumulation of knowledge that is not linear but cyclical. When asked why some theories are more resistant to dismissal than others Dr. Freda Adler, the lone interviewee who can be justifiably labeled as firm believer that the field has established a paradigm, suggests the following explanation.

Well they reappear for the same reason that if you want to make up a new word you can't get rid of the old alphabet, because they were tried and tested. There are parts of them that are time tested and there are others that are modified and become more sophisticated. Take social disorganization or social control. You can find their roots back in Plato and Aristotle. The theoretical constructs will come up in new contexts, and they're going to be refined. That is the beauty of our theoretical revisions. Basically we are dealing with social structures and cultures at the societal level (social explanations) and with why certain individuals commit crime (psychological explanations). As I said earlier, societal change

results in a change in the questions asked, the choice of a theoretical framework within which to do the research and in the methodology used in the study. Again, we modify. Take Sellin's culture conflict [Sellin, 1938]. What better theory to begin to answer a comparative research question in today's world with its rapid growth of cultures in conflict. As a result of criminal activity being so intimately connected with these larger and continually changing forces the explanatory framework must demonstrate sensitivity to these cues. Alternatively there are traits that are structurally embedded in the framework of human interaction. Therefore, criminological theory must recognize their importance. As conditions change so does the conceptualization of the problem, hence the consistent efforts at refining at the margins. The foundational theories are never jettisoned entirely. The fundamental insights they offer are simply redefined, extended or clarified. What remain consistent throughout are a few basic elements that aid in the framing of the problem.

Dr. Bursik reiterated this point in our discussion.

I think it's—and I hate to use this term—there's very much of a dialectic process. Where you rediscover, reevaluate in light of new evidence. We're at a point now I think neighborhoods, for now, have hit their peak. I'm convinced of that. The stuff going on now is fine-tuning. But I don't see any big breakthroughs on the horizon. So people will move off into some other things. There'll be a few old farts still around doing neighborhoods. Then it'll come back and people will say, "Well, you know, we never really did look at this enough. There's this problem, but when approached from this way maybe that's not a problem."

BD: I'm wondering how much of that is tied to the evolution of methods.

A lot.

At some point the excitement over the refurbished contribution—in this case see Bursik and Grasmick, 1993—eventually subsides and the field turns its attention elsewhere. The renewed interest over neighborhoods has, in Dr. Bursik's estimation, reached the point of diminishing

returns. In Kuhnian terms it set forth an agenda and the normal scientific enterprise is nearing the point of exhaustion. All of the major questions that can be posed with the perspective have been attempted with the available data and methods. It will be fine tuned and shelved momentarily. Newer methods will evolve and data will be generated, allowing for an eventual innovation of the earlier perspective.

This leads to another related question: Are criminological theories ever falsified? To which Dr. Adler replies.

No. Theoretical constructs are modified over time not “falsified.” Take *Crime and the American Dream* [Messner and Rosenfeld, 1994] for example. It did not negate Merton.

Likewise “general strain”[Agnew, 1992] did not negate strain. They modified it.

These theories recognized a fundamental element of truth in the earlier work within the anomie tradition. Each also made explicit efforts to confront the evidence assembled against it in an effort to reshape the theory. The promising elements were accentuated and those that met with more resounding criticism were minimized. The resulting efforts extended the theoretical insights to confront a new set of questions without refuting the earlier work. Embedded within the idea of modification is an allusion to partial falsification. Elements that have failed to overcome criticism are excised, reduced, or reinterpreted in the new alliterations. These efforts were built not on the ashes of their referents but what remained of their foundations.

These suspicions are substantiated by the quantitative data. Of the 1902 theory mentions 13% (N=248) are falsified (Savelsberg’s research team coded these as “rejected” in their data). Table 7 lists the respective percentages for the twenty-six theories. Twelve of the twenty-six theories are falsified less than 10% of the time and all but one less than 20%. In general, those subjected to more tests are more likely to have been falsified. The field demonstrates a bias toward affirming rather than dismissing theoretical propositions in its peer-reviewed literature; null-findings are less likely to earn publication. The emphasis on documenting affirmative findings suggests that the field is interested in determining the particular strengths of an

explanatory framework rather than its limitations. The benefit of this approach is that it aids the expansion of knowledge. Theory is allowed to flower so long as it is able to find sufficient fertile soil within its intellectual environment. When it has exhausted its potential it either expires or simply maintains in stasis. The drawback is that the field fails to exercise explanations that are deficient in explanatory power with any sense of finality. Only one tradition was mentioned as having been effectively dismissed by any of the interviewees, phrenology—the antiquated “science” of predicting criminality through assessing the contours of the skull, often derisively referred to as “bumpology”.

**Table 7: Falsification Percentages (# of rejections/# of substantive mentions)**

Dev. Taxonomy	0%	0/6	Illegit. Opp.	10.8%	11/102
Life-course	0%	0/17	Drift/Episode	11.8%	2/17
Reint. Shaming	0%	0/11	Rational Choice	12.1%	17/141
Soc Disorg.	3.2%	3/95	Self-control	13.8%	4/29
Hist./Constructivist	5.6%	1/18	Other	14.3%	45/315
Diff. Assoc.	5.8%	9/155	Power-control	14.3%	4/28
GST	5.9%	1/17	Feminist	16.1%	5/31
Routine Act.	6.2%	2/32	Control	17.5%	25/143
Anomie	8%	6/75	Strain	18.5%	24/130
Neutralization	9.7%	3/31	Labeling	18.8%	21/112
Early Soc. Def.	10%	7/70	Marxist	19.2%	15/78
Legit. of Violence	10%	1/10	Cohen subculture	19.6%	9/46
Miller subculture	10.7%	13/121	Biological/Genetic	27.4%	20/73

The results of this analysis, when coupled with the results attesting to the general absence of theoretical perspectives for lengthy durations imply that the field is not engaged in a conscientious effort at demonstrating the empirical inadequacy of its theories. Freda Adler’s earlier quotation suggests a plausible explanation as to why this is the case. The field’s application of a particular theory is not exclusively influenced by the strength of evidence adduced in its favor but rather through social forces beyond the field exerting themselves. Falsification does not occur through the continued testing and rejection borne by empirical tests of its propositions and assumptions. Rather, a given theory fades through its no longer being a

referent the field draws inspiration from due to its collective interest shifting to other questions, some more ephemeral than others. The research question dictates the explanation to be employed. When the field's attention returns to a once dormant agenda the theory is revived and revised; this is why the inclination is to preserve rather than discard theory. Theoretical perspectives should therefore be characterized as guides to framing a research question and the proper interpretation of its results rather than being dogmatic assertions of eternal truths.

Steven Messner elaborates on a specific example of this recurring phenomenon.

I think, for example, that the work that's being done now under the rubric of neo-social disorganization theory is a significant advance upon the classical Chicago School. It's incorporated some of the core insights but also filled in some of the gaps, responded to some of the criticisms of some of the earlier work. It's richer, more sophisticated. So in training a graduate student who wants to work in this area, while I would certainly encourage the students to read Park & Burgess and so on at least selectively, I wouldn't want them to spend all their time on that and not read what Rob Sampson and Jeff Morenoff and Paul Bellair—and I'm leaving people out here but you get the idea—and Chris Browning are doing as well. This is good stuff; this has gone beyond the earlier work in significant ways.

Criminology and criminal justice are engaged in a scientific process of compiling knowledge. The evolution is not linear, but logarithmic or cyclical. Broadly outlined, there are three stages in the life course of a theory. First, a theory is articulated. Secondly, it is tested. This involves applying its propositions to nearly all the permutations of the phenomena that it can be, given the limitations of data and methods. Lastly, it inevitably encounters limitations or criticism which it is unable to overcome. At this point it is relegated to the margins. At an undetermined later point when the field is able to address these critiques, either through the development of data and methods or reinterpretation of the theory, the perspective is resuscitated and the process begins anew.



It is arguable, however, that the problem of falsification is more fundamental than this. Some of the problems associated with achieving outright falsification can be attributed to the field's lack of effort in offering theories that are falsifiable. Perhaps theory tends to persist not because it has intrinsic value but because the field is incapable of making a firm determination as to its validity. Time and energy expended in defining the meaning of a theory and its components must be subtracted from that made available to testing it. A self-described "hopeless Popperian", Dr. Jack Gibbs describes criminology's predicament in the following.

But criminology, say what you will, it's a study. Someone comes along with a theory and then we spend decades debating how to test it. Then we go on to another untestable theory. By the way, I have to say this is probably where you and I part company on Travis Hirschi's stuff. I don't think his theory on control is subject to systematic tests, largely because he left control unconceptualized and didn't stipulate any measurement procedures. So people are simply left to their own when it comes to testing the theory. Now, I'd be the first to say what I'm criticizing Travis for is not peculiar to him; to the contrary. That's been the tradition in criminology.

The only solution to this impasse for criminology, according to Dr. Gibbs, lies in formal theory construction (stating in meticulous detail the theory's propositions). Overlooking the requirement of formally constructing theory leads to a proliferation of questions and not enough direction on answering them. "I would say above all what we suffer from is an inability to answer questions. It's not so much that we pursue unimportant questions." As a result, devising testable theories is the solution he promotes, with predictability being the ultimate aim of criminology as a science.

Dr. Gibbs elaborates further on what he argues is the folly of the discursive method the field has been toiling within in its attempt to generate knowledge. His contention, as he admits, is cynical. It is a characterization of a field "wallowing" in its indecision through its failure to abide by a universal metric by which all theoretical contributions are to be graded. When asked to locate the source of the undulation of theory he states flatly,

Lack of consensus on appropriate criteria for assessing theories. There's something that I think I should make clear. There's a curious phenomenon in sociology and criminology that theories are evaluated in terms of the perspective that supposedly gave rise to the theory. That's very curious; you'd think it'd be the other way around, that perspectives would be judged by the theories that it generates. Now, it's the other way around. You judge theory in terms of perspective that supposedly generated it. I think that's crucial in the question of why these trends, why the short life? Because once the interest in theories declined then interest in perspectives declined.

In Gibbs' nomenclature a perspective is akin to that used by the present author to denote paradigm (personal communication, 2009). He argues that the evaluation of perspectives has been proceeding in the exact opposite direction of the way logic dictates. Satisfying perspectives ought to generate theories that are more adequate than their competitors. In a roundabout way he is stating that much like initiatives holding parents legally accountable for their child's wayward actions theoretical forebears (i.e. perspectives) should bear responsibility for the shortcomings of their progeny in the court of science. The field has been unable to move forward because it is attempting to advance in reverse.

Another hypothesis proffered in accounting for the plethora of theory is that it is an artifact of the problem the field is addressing. In explaining why criminology resembles a field more than a discipline Ronald Akers states:

In fact criminology to me is still more of a field of study than a discipline because it doesn't have any separate theoretical approaches that you don't find in other areas. . .

Criminology's not defined by its theoretical distinctiveness; it's defined by its dependent variable. It's defined by it's being a study of crime and criminal justice: law, crime, and criminal justice. That gives us some advantages. It makes for a more unified field of study than something like, say, family studies.

Rather than lament the non-uniformity of theoretical perspective, it is suggested that this offers functional and professional advantages. Within the field a diversity of opinion can be located within its purview without sacrificing conformity, of which there is a scarcity. With little uniformity to be defended any contribution bearing on its explanation is permissible.

Furthermore, Dr. Akers offers that relative to the field of family studies which is inclusive of a myriad of dependent variables (family structure, child rearing, spousal interactions, marital satisfaction, etc.) criminology is more coherent. This is attributed to the field's general agreement to focus on a dependent variable that is less diffuse than others.

Robert Bursik also finds the lack of an overriding theoretical construct to be an advantage.

I think that's healthy. I think that's healthy for something so complex and so intrinsically multidimensional as criminology. Except in the crudest most ineffective sense how could you have a theoretical perspective that would tie together corrections and adolescent development? I'm not sure. It's like soc; it's an omnibus field with a bunch of little fields. It's like the social sciences being omnibus. Criminology's an omnibus thing. Not enough talk between the components I think. The fact that it doesn't have a common theoretical thread doesn't bother me.

In the above the benefit of having the flexibility of aggregations of normal science is pressed.

The only theory that could potentially account for all the phenomena within the expanse of criminology would be banal to the point of being platitudinous. Rather than seeing criminology and criminal justice as addressing a single dependent variable, as does Dr. Akers, Dr. Bursik sees the field as populated by many. This artifact, combined with the lack of discourse between those working on specific research agendas, militate against achieving a more unified paradigmatic state, a point picked up later by Robert Sampson.

Janet Lauritsen mentions that this reduces the likelihood of arriving at an all encompassing theory but argues that, at least in her research and potentially for the entire field as

well, dexterity counts for more than sure handedness. The task before her and the co-author mentioned is to account for a poorly understood phenomenon by integrating elements drawn a multitude of explanatory frameworks. A generalist approach allows for the incorporation of numerous perspectives to successfully cohere through the process of carefully sifting through the literatures available.

I would say that this [lack of consensus] is not necessarily a hindrance, unless one is trying to develop grand theory. In my own work with Karen Heimer on victimization trends among males and females, we have had to read widely, from quantitative methodology to feminist criminology to more mainstream victimization theory, such as routine activities and other kinds of frameworks. It has been very hard to summarize and synthesize all of the important insights from these perspectives in a way that will clearly guide our ongoing work. For example, when we are struggling with a particular problem, there may be no clear guidance on how to solve it from any one of those particular areas, although we derive different insights from each of them. Our research project investigate the conditions under which would we expect trends in male and female violence to be the same, or different. There seem to be countless ‘explanations’ or hypotheses, and we cannot possibly test them all. So one of our main challenges has been to determine what they have in common, these alternative explanations, and to create something more parsimonious. For us, and others too, it is an enormous project to try and make sense of the wide variety of relevant literatures now available.

In a field still in its infancy with respect to determining the parameters of its dependent variable this attribute offers the benefits that accrue to a malleable approach. This grounded approach permits the breadth of any relevant literature to be brought to bear on the question at hand. The task is to assess the validity of these competing explanations in generating a more parsimonious framework, one that brings us incrementally closer to the truth. The absence of a hegemonic

theoretical construct facilitates flexibility in a manner that would be cumbersome if burdened by a creaky orthodoxy.

Robert Sampson agrees in part but also suggests there are drawbacks. When asked to sketch the implications of a fissiparous development he offers the following.

It works against convergence. I think what specialization does -- again I am ambivalent about it because I do think that in criminology in certain respects it's healthy as in sociology-- is that specialization leads to the standardization of normal science, which we need. There's a lot of excellent research that's being done. But I think it's harder though for people to see the larger picture often. The buzzword is "silos". Everybody's siloed-- I can't stand that word because I hear it so much! But it is true that people don't know what other people are doing and then it also tends to lead to the recycling of ideas and rediscovery of the wheel. There are lots of dangers to it. But on the other hand I think there is a cumulative knowledge base that's been built. There are some very healthy research programs. So I think that the way you try to harness specialization and all these advances is to focus around research programs, big questions that have a grounding in a particular tradition.

This suggests a fundamental difficulty of the field. As it advances, the respective research programs are producing contributions to the growth of knowledge. The problem however, is that there are few elements that can weld the largely divorced elements into a coherent whole. A few interviewees suggested that there are difficulties in training generalists (Lauritsen and Messner) as well as acting as a practitioner with a generalist orientation (Short). These practical difficulties are the natural result of the expansion of the criminological enterprise that thwarts the development of a paradigmatic approach. The advances of the respective research programs offer promise that the fundamental elements that can potentially inform a unitary theoretical approach are being assembled. Until there arrives an agent or framework that can successfully integrate

these disparate agendas Dr. Sampson suggests the field will rediscover the wheel, so to speak, for the duration.

When asked to address the topic of the lacuna of a defining orientation he and Joachim Savelsberg articulated in their critique of the field (Savelsberg & Sampson, 2002) and alluded to in the above Robert Sampson continues,

What we meant, or at least I mean, is usually a discipline has an animating idea in terms of its assumptions about the world, not just a topic. So, criminology is a topic; it's about crime. But whether you agree with it or not sociology has an animating idea about social relationships, social interactions and emergent properties. If we had to say, we could go back to Durkheim if we wanted a canonical definition of the notion of what sociology's about. Economics, of course, is about utility maximization. There's a certain core idea to that. There is a consensus. Psychology and all the other disciplines are the same. Where economics is not just a subject matter; it's a certain assumption about how the world works. The same with sociology. Criminology, by contrast, is about crime and criminal justice. But today, it can be psychological; it can be sociological; it can be biological-- take the work of Adrian Raine. It's much more common now to see articles published on genetics, on neuroscience, psychology, biology. I don't agree with all these things but I think that's perfectly proper.

Dr. Sampson expressed a preference for a sociological approach while recognizing that others have made worthy contributions as well. The principle shortcoming that he points to in allowing the dependent variable to shape the field's explanations is in its creation of an atmosphere permissive of "super" or "phony interdisciplinarity". Specifically, he points to the atheoretical "stacking of variables", reminiscent of the Glueck's approach in an attempt to account for the most statistical variation. While emphasizing that there are costs and advantages to both approaches he characterizes this approach as "dangerous" and "mindless," and much prefers, "a leaner and conceptually oriented approach that doesn't try to be all things to all people."

Joachim Savelsberg relates a point of view on why the field lacks a single minded theoretical perspective in the following. His understanding of this issue is more pragmatic than those espoused in the preceding. When asked to remark upon the state of criminology in reference to his own academic profession, sociology, he states:

But I would certainly say [sociology has] a firmer theoretical orientation. I think whenever you build a field, be it criminology or something else that's multidisciplinary, where you have political scientists, psychologists, sociologists, etcetera talking to each other who in their home disciplines maybe use different theoretical tools but do try to communicate and try to write for reviewers who might be from the other fields, they will tend to downplay the theoretical apparatus with which they came to this field and focus more on technical and methodological data-driven aspects, maybe policy-driven aspects of this field.

Implicit in the quote is the contention that criminology, at this early stage in its formation, is not scandalized by its lack of a firmer theoretical orientation. In fairness, it should be noted that sociology has experienced a decline in the use of nomothetic (spatially and temporally infinite) explanation relative to ideographic (spatially and temporally finite) explanation since the late 1960s and appears to be similarly untroubled (Gibbs, 2009). Criminology seems to tacitly recognize the fundamental problem of its missing a unifying theoretical construct by agreeing to overlook the issue in the short term. Focusing attention on this aspect is likely to feed discord. Instead, the field devotes more of its attention to matters in which there are greater prospects for reaching consensus. Within the field this is more readily found in its data-driven or policy oriented aspects. This suggests that the field is attending to defining its problem at the moment through devoting resources to fact gathering. At some later point efforts can more fruitfully be devoted toward generating theory to account for crime. What will likely complicate these efforts is the field being populated by experts from numerous fields, each with a vernacular informed by

their respective traditions. If consensus is to be developed a *lingua franca*, or uniquely criminological lexicon, will have to emerge in order to facilitate its maturation.

#### *The Present and Future of Theoretical Consensus*

The nearly unanimous consensus of the interviewees is that the field has been progressing while lacking fundamental agreement as to what the guiding framework ought to be, although there are a few contenders earning consideration. Overall, the sentiment reflected in the data can be justifiably characterized as indifferent regarding the prospect of developing an overarching approach. When asked directly whether criminology currently possess a paradigm Francis Cullen agrees that the field has lacked one but is more optimistic over the future.

[T]he emergent paradigm in criminology is now the life-course paradigm, which includes developmental theories like Terrie Moffitt's. In fact, the life-course approach is not just the field's dominant paradigm. I think *it is criminology*. I think it's going to become increasingly absurd not to see the origins of crime as potentially starting in the womb, and not seeing events in adulthood as mattering, as Sampson and Laub would say. I think that you have that paradigm; it is not just a theory; *it is criminology*. It is the future of criminology. This approach is often presented, including in my theory books, as just another theory. But it's not just a theory. It is the way in which criminologists will now look at crime. (Cullen's italics)

Life-course theory is mentioned as providing several benefits from which the field can draw. The foremost advantage is its theoretical contribution being tightly coupled with a methodological approach (i.e. the use of longitudinal data). Part of the reason offered as explanation of life course assuming supremacy in the veritable free-for-all in criminology is generational effects. Future generations of scholars will be taught to recognize the approach as essential to building a common understanding through their graduate training. Enhancing this effect is the fact that several leading researchers (Sampson, Laub, Nagin, Paternoster, Farrington, the Loebers are all mentioned by name) have committed their research agendas to exploring criminology within the



life-course perspective. This provides a signal to future cohorts of scholars that the agenda set forth by the life course perspective is professionally rewarding. The additional advantage is that it can potentially mend the divide between criminology and criminal justice, which he contends is largely artificial. Dr. Cullen alludes to this with the mention that life course theory will come to inform the “interventions we privilege.”

An additional advantage to the adoption of life course theory as the organizing schematic of criminology is its versatility. The framework allows for differing theoretical approaches to locate within its structure without fatally compromising its integrity (see also Laub, 2006). Several respondents (Ronald Akers, Francis Cullen, and Lawrence Sherman) cited biosocial/biological arguments as being in the ascendancy. Dr. Cullen implies that biological influences are evident throughout the life course and can therefore be combined profitably with other approaches.

In addition, all the biosocial data are going to be brought into this issue. Scholars in the review process will soon hear: “How do you know something has an effect if you have no control for biology?” My colleague John Wright is doing a lot of research on that. His goal is to wipe out all social effects with biological data! I’m being a little bit facetious here. But essentially, you’ve got to understand, there’s good biology theories and there’s bad biology theories. Terrie Moffitt has a good biology theory. We all love Terrie so she gets to have neuro-psychological deficits. Then there’s the bad biology, everyone else’s. So that’s what I see as the main paradigm. I tell my students, “This is it. Don’t even do any other criminology.” Even if you do macro-level criminology, you’re probably going to have to somehow tie it into multi-level models. Looking at how context matters at different ages, in different ways. It doesn’t exclude all other criminology, but I think that it is the paradigm of the future. . . Now the remaining paradigm is the mainstream criminological theories—general strain theory, social learning theory, those kinds of things. Together, they still comprise a mainstream paradigm. But they, too, are getting sucked into the life-course paradigm. It’s going to be hard to do a test of general strain theory, for example, if you

don't do it within a life-course paradigm. So I think that we've gone from sort of having mainstream criminology and critical criminology to now having critical criminology and life-course criminology. I think life-course criminology will dominate the field.

The biological approach will not be criminology's only perspective because context will always exert an influence. This leads to a preference for the term "biosocial" which recognizes the attempt to incorporate elements of both traditions in explaining criminality. The melding of the two approaches are facilitated under the auspices of the life-course approach which is characterized as exerting a force akin to vortex in reorienting the field's fundamental theoretical and methodological orientation. The more traditional theoretical approaches will all come to be subsumed by life course theory in that they will be required to make methodological genuflections in its direction.

Finally, Dr. Cullen outlines another paradigm within the field, critical criminology. This, in contrast to the others, is characterized as being dormant; life course and biology are emergent while critical theory is marginalized as a result of its fundamental rejection of positivist methodology. In this regard it is truly an alternate paradigm by dint of its basic disagreement over the appropriate terms of debate. His pointing to a separate paradigm that addresses Sutherland's "making of laws" that is distinct from another prong of his definition of the field ("breaking of laws") should indicate to practitioners that a reconciliation of approaches holds potential for theoretical growth through fostering a more global understanding (see also Gibbs, 1987).

Now, I have to say that there is at least one other paradigm, which, by the way, I have learned a great deal from and am sympathetic to in many ways. And that's critical criminology. Critical criminologists are mostly anti-positivist unless the data support their conclusions. No critical criminologist has ever challenged the inequality-homicide relationship that I know of and said, "You can't believe the fact that inequality is related to homicide because it's based on quantitative statistics." But they're generally suspicious of positivist data; preferring to be much more qualitative. If they work within the life-course

paradigm, it's usually to show that causal factors do not generate effects. Rather, they want to show, for example, that there are gender-specific pathways in crime. But most scholars in critical criminology, and I include peacemaking criminology under this umbrella, are pretty comfortable with attending their own sessions at ASC. It's hard for those folks to get published in mainstream journals. So they have some of their own journals. But they have not been brought into the life-course paradigm explicitly—except, again, to try to argue that there are gender-specific pathways into crime. So critical criminology, that's sort of the other paradigm.

So, what accounts for the anxiety over the lack of an all encompassing theoretical construct? Both former editors of criminology's flagship publication (*Criminology*) included in the sample, Charles Tittle and Robert Bursik, spoke of their efforts at maintaining the field's dedication to viewing the enterprise through the lens of theory. The latter encountered some resistance from contributors who thought otherwise, the source of which, he implies is generational.

Because there's this belief from some quarters that numbers speak for themselves. I grew up in an intellectual context that I don't believe that numbers speak for themselves. You can say with confidence that women make X percent less on the dollar than men do. But what that means though is a whole different thing. They don't speak for themselves. And to do that speaking you need some kind of interpretive framework. To even know what variables to use or consider you need an interpretive framework. Now your framework could be wrong but you just can pull variables out of your ass like some people do. Some of the neighborhood interventions projects—let's just get everything that's ever been done, run this regression with a hundred fucking things in it and see what happens.

The latter part of the quotation is an echo of the earlier sentiment contributed by Robert Sampson, albeit one offered in a more colorful and caustic tone. Essentially, both are arguing that the

empirical phenomenon falling within the field's purview can only be profitably interpreted through recourse to theory. Furthermore, Dr. Bursik states an opinion that theory is an *a priori* requirement for approaching a research question. A collection of randomly assorted variables processed through a statistical methodology merely to account for the most variation would be regarded as a desiccated product; theory is the element which gives empirical findings life by assigning them meaning. In essence he is arguing that methodological development can only take the field so far before it expires. This thought is affirmed by Steven F. Messner.

Methodology helps. My own view is that, I don't think methodological/statistical advances are really the driving engine. I think it's more theoretical, conceptual clarifications, elaborations and then the methodological tools help speed things up. For example, you take HLM. It's a useful statistical tool. But you have to know what are meaningful levels to be working with. You have to think about how processes interpenetrate across levels and so on. There's nothing in HLM software that can do that for you.

This now brings us to the question of what the field's state of agreement on a methodological approach is.

#### *Methodological Consensus*

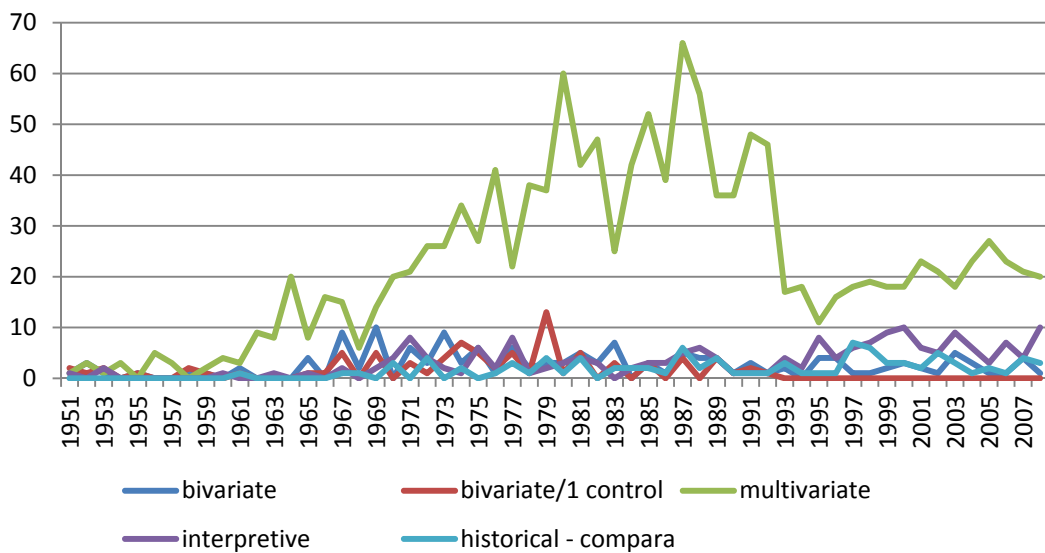
The qualitative and quantitative evidence elaborated in the preceding unequivocally state that criminology lacks a monolithic or definitive theoretical perspective. The evidence with regard to its state of methodological consensus is more nuanced however. The quantitative evidence suggests the field has been settled on an approach for some time. This contrasts with the impressions offered in the qualitative data.

#### *Statistical Methodology and Types of Data, Consistency in Approach*

The field's peer reviewed research over the last half-century plus has been aimed at interpreting empirical evidence using an objectively social scientific approach. Nearly nine in ten articles (87% or 1833 of 2109) use one of the five respective approaches (bivariate, bivariate plus

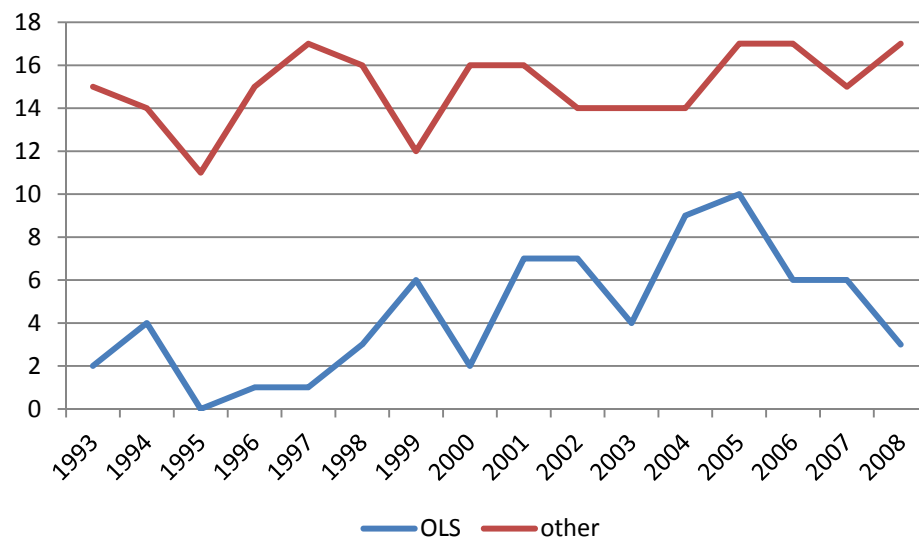
1 control, multivariate, interpretive, and historical/comparative; see Figure 4). Conversely, 13% lack for a verifiably social scientific methodology. Of the 1833 publications relying one of the enumerated social scientific approaches nearly three of four (72% or N=1319) use a multivariate statistical methodology. 1958 was the last year in which this approach was less popular than any of the remaining. There were but brief points (1967-1969 and 1995) when it dips to near parity with the others. For but two years, of two-hundred thirty two possible, do any of the non-multivariate methodological approaches exceed ten appearances in the data. For thirty-one of the fifty-eight years included in the study the multivariate methodology appeared twenty or more times in the literature which includes the ten years in which its tally exceeded forty. The multivariate methodology retains a marked advantage in the literature within the appended data when the percentages were plotted as well. Its supremacy was moderated over the final sixteen years when it's 68% representation dropped by four points and the interpretive approach, the next most prevalent, doubled from 10 to 20%. These differences in methodological trends are statistically significant ( $p < 0.001$ ) at six year intervals.

**Figure 4: Method of Analysis**



The disparity in the use of the multivariate approach were evident at the dissertation proposal stage. At that point it was decided to bifurcate the multivariate category into ordinary least squares (OLS) and “other” in the appended data. During the concluding sixteen years of the study time frame there were 311 publications using a multivariate approach. The majority (77% or N=240) of the multivariate techniques were comprised of statistics other than OLS which comprised less than one-quarter of the multivariate category (23% or N=71). The most popular within the catch-all category were logistic regression (N=98), hierarchical linear modeling (N=23), negative binomial (N=14) and statistical equation modeling (N=12). All the remaining approaches appeared less than ten times from 1993 to 2008. Figure 5 displays the differential between the approaches which reach statistical significance at the two year ( $p<0.05$ ) and four year ( $p<0.01$ ) intervals.

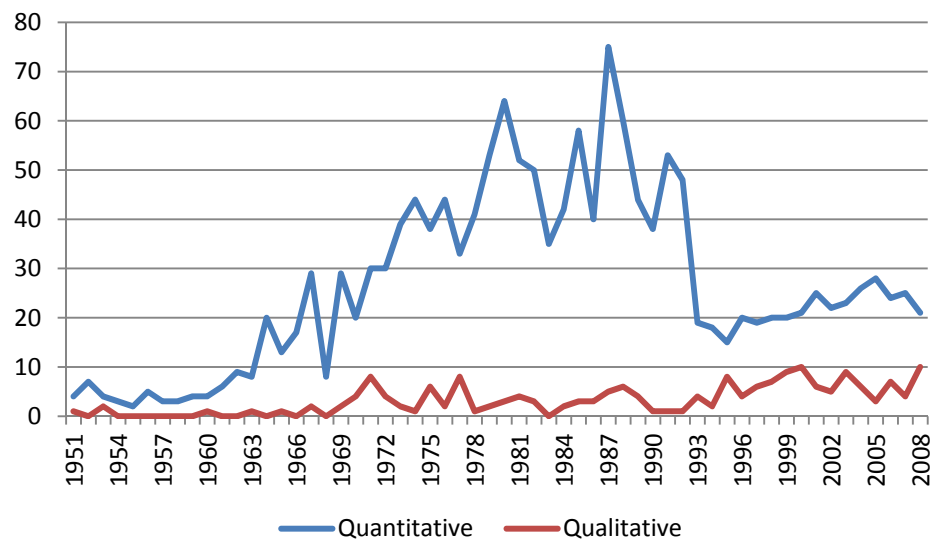
**Figure 5: Multivariate Subtype**



The analyses above attest to a bias in the field toward quantitative methodology. In order to determine to what extent this is true an additional set of analyses were conducted by dummy coding the methods variable into quantitative (bivariate, bivariate plus 1 control, and multivariate) and qualitative (interpretive) categories—the 98 cases of historical/comparative were excluded from the analysis. Of the 1739 articles 89% (N=1555) apply a quantitatively oriented approach.

At two year increments the differences are statistically significant ( $p < 0.001$ ). The gap widens greatly beginning in the 1970's and remained until its convergence in 1993 (Figure 6). In the latter period the presence of quantitative methodology drops from near unanimity (94%) to being merely dominant (78%). Irrespective of this disparity, the quantitative approach is markedly preferred by those producing the field's peer-reviewed scholarship. At only two points does the qualitative approach appear as many as ten times in a year; in thirty-nine of the fifty-eight years recorded the quantitative approach is at least double this total.

**Figure 6: Qualitative and Quantitative Trends**



The narrowing of the gap between quantitative approaches and qualitative is likely to arouse some opinionated discussion of its implications on the field's status as a science—Dr. Messner recounted in his interview the introspective and largely pointless feud over which should assume supremacy that once embroiled sociology. Jack Gibbs argues below that ordaining one approach as more scientific than the other is an academic exercise, in the pejorative sense of the term.

Well, methods has always had this very peculiar status. I know there are people who believe that quantification really killed off sociology. But that's another perspective, the

humanist perspective. Frankly, I see the whole question of methods and quantification a red herring. I really do. When I hear someone rant against quantification, and linking quantification and science as though they're inseparable I always ask them, "Have you ever read Darwin's *Origin of Species*?" There's not a formula or equation in the whole damn book. So this idea that quantification and science are inseparably linked and therefore it's not good for the social sciences and sociology, I just part company with that argument. I don't think it's really constructive. But I know there are people who are sold on the idea that sociology and criminology's salvation lies in some method. I just say, "Har, har!"

The consensus seems set against this position however. The narrowing can be at least partially attributed to the qualitative methodologically oriented articles appearing in criminal justice outlets which generally hold a lower level of prestige (analysis provided in the following chapter).

Others reject the positivist agenda altogether. Dr. Chambliss remarked in his discussion on how he came to reject the social-psychological approach he adopted at Indiana University where he earned his graduate degree. The critical perspective holds the notion that clearly discernable distinctions between criminals and non-criminals with disdain. The fundamental difference is in the questions each purports to answer; one is focused on explaining criminality, the other on legality. His opinion below is colored by the criticism that quantitative approaches hold the field's collective imagination as a result of funding influences exerted by the government, the selfsame entity charged with creating and enforcing the law.

I think criminology is distinctly in error to place so much emphasis upon quantitative research methods. That's not surprising, because that's what the government supports. Everyone wants to get research grants. And if you want to get research grants you have to do quantitative research because that's what the government will fund. But basically what I think drives criminology, and drives criminological theory, is qualitative research. The most important research is, generally speaking, that has had the biggest impact are things

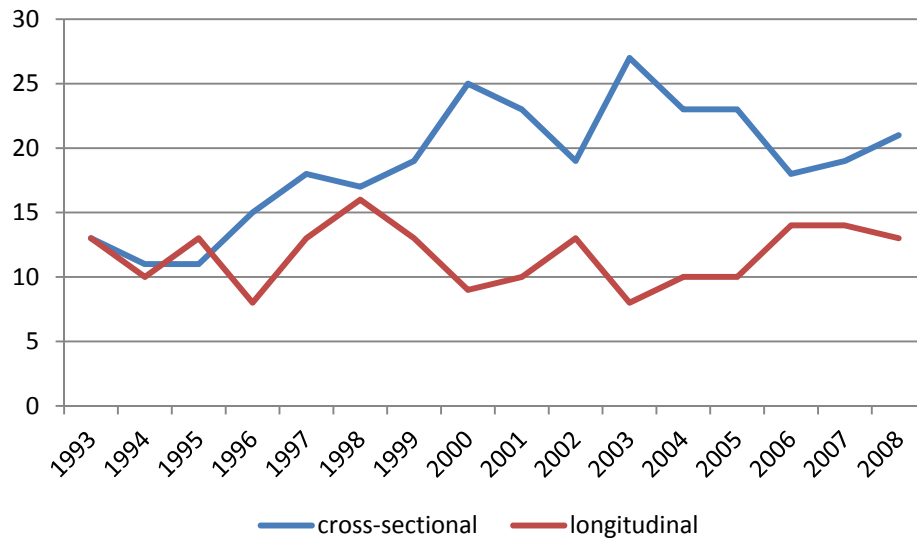


like Anderson's study of the *Code of the Streets* or Gans' study of the *Urban Village*. These kinds of studies have been the ones that have driven both sociology and criminology to a much greater extent, I think, than the quantitative studies. That's not, to me, surprising because the quantitative studies are an attempt to employ ostensibly scientific methods that turn out to be rather unscientific in their application.

As another interviewee (Ronald Akers, citing a remark by Donald Cressey) suggested, the field has a cultivated regard for qualitative contributions as well. This is somewhat curious given the self-evident tilt decidedly in favor of quantitative approach. It is likely the field will always continue to rely on qualitative research for inspiration, even if it fails to elicit much regard as a scientific approach as reflected in peer-reviewed publications.

Making an accurate determination of what interpretive framework to set an analysis within is contingent on what type of data are at hand. An important consideration in this regard is temporal scope. Within the appended dataset a dummy variable denoting cross-sectional from longitudinal data was created. Overall, 489 of the 501 articles fit within the coding scheme. Greater than six in ten (62% or N=302) are cross-sectional and the remainder fall into the longitudinal classification. The trends shown in Figure 7 are statistically significant at the three-year ( $p < 0.05$ ) mark only. For the first three years in the period the two categories overlap prior to the gap widening somewhat before reaching near equality in 1998. Over the remainder of the study the trends oscillate (diverge, converge, diverge). What this indicates of the field's research is that it is prone to fleeting interests.

**Figure 7: Temporal Orientation**



*Counterpoint: The Array of Methodologies Within Criminology*

When the critique that criminology lacks a unifying methodology (Savelsberg & Sampson, 2002) was raised with members of the interview sample the responses ranged from indifferent to indignant. Respondents typically suggested that this is of little importance; indeed, it may provide benefits. Other social sciences are similarly situated with regard to their methodological approach. The explanation provided by Ronald Akers below conveys this opinion well. In response to the question of whether criminology is devoid of a universally applied methodology he offers the rejoinder:

That's true. But that's also true for political science. It's also true for psychology. Those sciences all use the same methodology. They use survey design; they use sampling designs; they use statistical controls; or they do experiments. The experiments are quasi-experiments or you have real experiments. You have random assignment or non-random assignment. You take methods course in political science, you take a methodology course in sociology or criminology, psychology, there's a huge overlap in what you get exposed to. So I think it's true there's no unique criminological approach but there's some unique criminological

data sources, like the Uniform Crime Reports. Lots of people use that. But that's always been the main official data source for criminologists. You now have these victimization surveys. Now we have these huge longitudinal data sets that are available to people. That's not unique to criminology but what area in social science does have a unique approach.

What would it be?

The fact that the field lacks a uniform methodology does not *ipso facto* relegate it to non-science. From this one can infer that Dr. Akers does not regard a unifying methodology as the indispensable hallmark of a scientific enterprise. Criminology lacks a unifying methodology which is not to be confused with lacking adequate methodology entirely; it suffers from a surfeit, not an absence. The adaptability of being able to place the field's graduate students in other disciplines' methodology courses without suffering a diminution in the comprehensiveness of training provides it with leverage for continued growth. Indeed, the potential for intellectual growth overall may be enhanced by the allowance for a diversity of accepted approaches.

The adoption and application of a variety of methodology drawn from any number of disciplines is not tantamount to suggesting this as evidence the field lacks for methodological rigor either. John Hagan argues that the field has not wanted for either methodological or theoretical strength over the course of his career.

I think the field of criminology has always had a strong methodological and theoretical base, both. The classical sociological theories of crime played a prominent role, and methods always being a prominent part of things, looking all the way back to the diagnostic tools that were developed for probation and parole kind of work, studies of the death penalty and so on. The period when I came into the field, in the '70s, there was a lot of borrowing from methods in sociology. You'd have these structural equations, categorical methods, event history models and so on were gaining prominence in sociology. Actually, both sociology and criminology are both more eclectic now

methodologically. So I had great opportunities for training off in western Canada. You know, the field has only gotten stronger methodologically.

BD: It's been alleged by Joachim [Savelsberg] and Rob[ert] Sampson that criminology lacks a unifying theoretical and unifying methodological construct. To what extent do you see that as being the case?

I think that's true of criminology. Now I'm beginning to think it's true of sociology as well. There was a time when sociology—the '60s, '70s and even into the '80s—it was all about Marx, Weber, and Durkheim. There were three dominant schools: structural functionalism, symbolic interactionism, and conflict theory. But you look at the top journals now and that's not where we are; it's a much more wide-ranging eclectic field, sociology is, with lots of borrowing from economics, evolutionary demographic techniques, imports from psychology and education for example. There was a period when LISREL was popular. There's a lot of eclecticism in sociology. I agree with the point they're making about criminology, but I think it may be true of sociology now as well.

The fact that the field demonstrates a tendency to draw inspiration from beyond its immediate purview is no cause for self-doubt. Elsewhere in the interview it was offered that sociology is more amenable to qualitative methodology than criminology however. This preoccupation with quantitative methodology may be a method of (over)compensating for a self-image that is sensitive to how its science is perceived by the wider disciplinary community.

While agreeing with that criminology lacks a unique methodology D. Wayne Osgood remarks that the field has applied little used methodological approaches with profit. When asked if he agrees with the contention that criminology evidences an absence of unifying methodology he offers,

No, not really. I think all the social sciences overlap a lot with methodology. Statistics are pretty agnostic with what they get applied to. And basic issues of research design and all that sort of stuff apply to anything that you want to learn something about. So engineering and medicine and all that, they do a lot of the same stuff we do. But if you compare them I think there's a lot of difference in evolved taste if nothing else. But there's also differences just because you work with crime data and it's got certain features. Certain things that work well with that in particular, and that gives us our own corner on methods that are somewhat different than others. . .Here's what justice system data look like. There are particular weaknesses that it has. So we have, particularly in the last twenty years we've developed a lot of statistical models for non-normal data. In another field that would be the exception rather than the norm. You can't get a measure of crime that's normally distributed. It would take really weird circumstances to create that. The most developed family of statistics is about normally distributed stuff. So they don't do us that much good. So we've been pretty good about picking up negative binomial, tobit, and all these sorts of things, hazard models, event history models. Criminologists do a lot more of that stuff than a lot of other fields just because our data are like that.

Like Dr. Akers he concedes that the field lacks a unique methodological frame of reference, but not entirely. These efforts at accounting for relatively rare events are not separate from the larger conversation on statistics but borrow more from it than the other social sciences. Because crime is exceedingly uncommon criminologists have been forced to incorporate and cultivate statistics that are adapted to account for non-normally distributed data. Engineering and medical statistical modeling attempt to explain such rare events as equipment failure and mortality of study subjects, making them an ideal resource. This sentiment is a reprise of his earlier argument that the field ought to be "stealing from its friends" (Osgood, 1998).

The "evolved tastes" of particular fields are indicative of methodological preferences. These are conventions, forged through years of accumulated understanding, on what constitute

approved means of generating knowledge. His experience in dealing with literatures as diverse as human development, criminology and beyond, the former via his wife and the latter through his being part of several research collaboratives, leads him to the following conclusion.

There are different priorities methodologically. If you were going in front of an audience in human development to talk about parental influence and delinquency they're going to be really attuned to measurement issues like does all your data come from one source. Because they're really skeptical if we were to study your delinquency and find out about parenting by asking you. They would say, "How do we know this it's just not all in your head? We would really like to hear from the parents too. Or even better, we'd like go to the home and observe." So a typical criminological paper on parental influence would have a lot of trouble getting in a really good developmental psych journal because they have high standards in areas that we aren't particularly worried about. At the same time, if one of their papers came to one of our journals, we'd be likely to say, "What's with this sample of two hundred families who live near your university? Who are they? Why should we be interested in them?" And then there might be statistics that aren't as sophisticated as we're used to, not dealing with the weird distributions as well. And they might not measure crime the way we'd want to. So it's kind of more like our judgments of the elements of quality can be really different from other fields. And I often say we ought to not be too parochial about that.

Dr. Lauritsen whose recent work on victimization draws from a variety of approaches spoke of these differing shades of emphasis becoming evident via the peer-review process.

It's easy to notice in reviews of your manuscripts what kinds of training reviewers received when you read their comments because of the kinds of issues they raise in their reviews. A recent example is a paper we had written on trends in victimization. We suspected that one reviewer might be an economist because they had focused most of their attention, not on the substance, but on the analytical technique. Another reviewer

appeared to be a survey methodologist because they focused their questions on those issues, and there was a reviewer who was focused on the substantive findings. I think this is a deliberate decision by the editor. Some reviewers wanted to make sure we were using the data correctly, and some wanted to ask whether we analyzed it correctly. The editor wanted to know if we made a substantive contribution to the literature. We were grateful for this diversity of reviewers because from them we learn important things: We do not know everything that the economists know; we do not know everything that the methodologists know. So we learn very helpful tips and references from the reviewers who were trained outside our discipline.

Dr. Savelsberg's comments in the previous section that the field hopes to achieve some level of consensus through focusing its attention on methodological commonality foretells of problems given the heterogeneity of the research traditions that overlap with the field. Reviewers drawn from a variety of differing disciplines each tend to critique a potential contribution based on their reading of alternate facets of the work. In many regards each reviewer is speaking past rather than to one another. This presents a practical difficulty in attempting to satiate each of the elements raised in the review. Assuming the reviewers are relatively indifferent to the other elements or that they are not intertwined this presents little problem for the contributor; to the extent that this does not hold true it does.

#### *Objections to Generating a Uniform Methodology*

Another scholar takes a more direct approach in confronting the critique of criminology's lack of methodological consensus. Charles Tittle objects to the idea of establishing a universal methodology. He insists that this will not limit the field's claim to scientific authority. Rather, he argues that retaining a flexible approach will aid the search for truth. This conclusion results from his decoupling a unifying methodology from the list of requirements that rigorous science must meet. The dictates of a scientific approach demand the proper application of scientific methodology, not necessarily a unifying one.

I think that our work must be tied together by the logic and assumptions of science, not by a unified methodology. Indeed, diversity of methods is useful. The process of science is first to have an idea. Then, you need to bring every kind of empirical data you can to bear on that. Because there are many tools for organizing and collecting empirical observations, you wouldn't want to limit yourself to just one of those kinds of things. So, yes, we lack a unifying methodology in a sense of a scientific approach.

Dr. Tittle finds that the benefits of the flexibility of criminology's methodological approach are not worth the sacrifice of acceding to a single methodology. He cites economics as a discipline beholden to a single approach and thus as a cautionary example of why the field should avoid strict methodological uniformity.

We need to avoid becoming boxed in like economists have become. They can't get any new ideas because they are committed to one way of thinking and one way of doing things. Once you set such boundaries, going back to Kuhn's argument, they are very difficult to break down. Economics is too unified, which we must avoid.

Consistent with the thoughts expressed by Ronald Akers, the advantages of a multitude of approaches are highlighted and the limitations of a singular approach accentuated. Dr. Tittle gladly concedes the point expressed by field's critics but insists that having a number of methodological approaches provides more hope for gaining understanding. His comments warn against imposing an orthodoxy that would produce stagnation; the infusion of a variety of methodological approaches through tolerating porous professional and intellectual boundaries is a trait the field should accept, if not enhance, rather than suppress.

Dr. Lawrence Sherman echoes this sentiment that criminology would do itself disservice through insisting on adhering to a singular methodology. Reducing the array of methodological tools at its disposal would artificially stunt its potential for growth. Asked for a response to the critique that criminology lacks a unifying methodology he responds:



I would simply reject that as criticism. I would say, “So what?” Medicine lacks a unifying methodology or a unifying point of view. You could say the same thing about biology if you want to stay away from professional practice. The truth about science is that it needs open architecture. It needs to be devoid of orthodoxy. Because it’s orthodoxy, as Kuhn points out, that limits people to normal science. What I think is wrong with Kuhn is that to a certain extent normal science is a caricature of disciplines in at least the last fifty years which have been so polycentric. There is no one normal science; there exists congeries of normal sciences in all the subgroups within the field. So we have normal science in experimental criminology even if we’re not a very big seat at the table.

Instead of developing a monolithic methodology the field has incorporated a number of perspectives, each demonstrating a normal science of its own. In many ways this sentiment affirms Robert Bursik’s conceptualization of the field’s theoretical state being “omnibus”. Dr. Sherman continues,

[C]riminology will advance by integrating perspectives from as many fields as possible at many levels of analysis, with many methods; that we can’t even say *a priori* what all those methods or fields may be since even in my lifetime and what’s left of it there may be new methods and new fields invented.

The mechanism by which the field would arbitrarily curtail its growth would deny itself unanticipated methodological innovations in the name of orthodoxy. Inviting in as diverse a number of methodologies as is feasible will augment further development and new fields that are unforeseen. Lastly, Dr. Sherman reiterates his earlier point that the criticism of the field’s lack of an agreed methodology is misplaced:

But to go up to the high altitude and say, “Should criminology and criminal justice be all experimental? Should it be all control theory based? Should it be all any one method or theory?” I think really misses the point about science as an open form of inquiry, an open

systems enterprise, that is not helped by having a high level of consensus, but probably harmed.

Steven F. Messner points to an example of the problem alluded to in the above.

Sociology experienced an internecine feud when it divided into factions debating the merits of which approach (qualitative/quantitative) was more scientifically justified. Attempting to impose methodological uniformity proved destructive to sociology. Perspective is offered through a comparison of methodological uniformity of criminology when juxtaposed with sociology. The field is not united, but it is more coherent than others. The relativity of the term “uniform” is what makes its assessment so fraught with difficulty.

I think there’s probably more consensus in criminology today than in sociology, methodologically. You do have qualitative researchers, quantitative researchers, but it isn’t a hostile relationship. You have quantitative researchers, people who are known for quantitative research, who will occasionally incorporate qualitative aspects into certain studies. Then you have qualitative people who occasionally do the opposite. So I don’t think these are hostile camps. Also, what strikes me about the field is that there’s more of an agreement about the importance of integrating theory and evidence than you find in some other disciplines. There’s a shared commitment to that. Different people go about getting the evidence differently. It may be qualitative; it may be quantitative. Some use multi-level modeling whereas others are doing narrative analysis. But I think there’d be consensus on that.

The field, while it disagrees amongst itself with regard to methods, demonstrates a commitment to integrating theory and evidence. Having a group of scholars who recognize the importance of the interrelationship between theory and methods suggests a community receptive to establishing the basic tenets of paradigm. These attributes augur well for the growth of methodological innovation in that this will ease communication in this realm and the conversation will continue

to be informed by the maximum breadth of participants. Criminology's lack of a balkanized methodological terrain allows for findings to traverse boundaries with relative ease.

Bolstering the points raised above, James Short suggests that unification can be achieved through attaching methodological innovations to theory (see also Steven Messner's comments on HLM in the preceding section). The field rewards methods that hold the promise of encouraging theoretical advances.

Well, I think that my main contribution was relatively minor. I helped to make the study of self-reported criminal and delinquent behavior more respectable. Once we started publishing that work other people decided they could use self reports too. Others have advanced that methodology in much more sophisticated ways than Nye and I ever did. We were, in a sense, pioneers because we advanced the method beyond the special populations to which it had previously been applied; and we used it to advance theory, which others had not done.

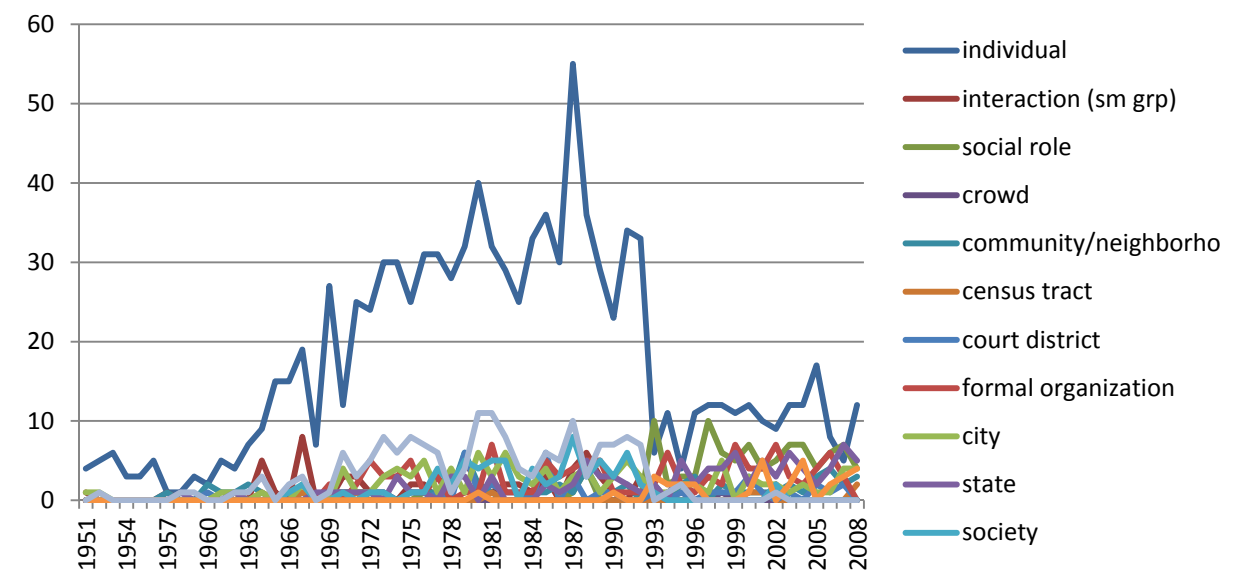
The success he and F. Ivan Nye had with introducing the self-report methodology was that it was articulated as an approach that was applicable beyond a specialized interest. Ronald Akers mentioned his success with an article he authored that replicated the Short and Nye methodology; it was one of the most cited in the field after its publication. Similarly, Dr. Osgood stated that his methodological contributions are among his most cited. To the extent that a broader audience can be generated the findings resonate. That a contribution joining theory and methods managed to attract attention from the body of criminology attests to the field's impulse toward adumbrating a paradigm even at this early point in its development.

#### *Levels of Research/Analytic Approach*

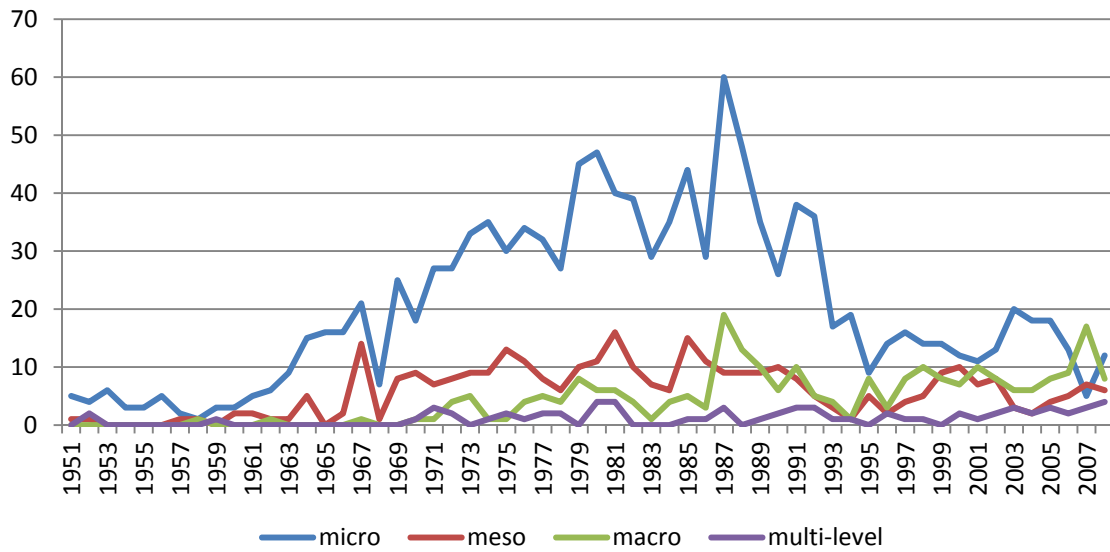
One factor that may ease the exchange of ideas is working more or less on the same level of explanation. Figures 8 and 9 illustrate that the field is committed to an individual level approach. The first figure plots all thirteen of the sub-types on a single graph. The line exceeding all the others in terms of its number of total appearances in the literature per year is

that reflecting individual level research. Of the 1888 articles coded 53% (N=1008) are those of individual level research. At the twenty year interval mark these differences reach statistical significance ( $p < 0.001$ ). Its appearance in the literature is nearly halved (65% to 33%) between time periods. The balance of the offsetting proportion went to “social role” explanations which grew geometrically between data collection efforts. Figure 9 aggregates these finer distinctions into four broader classifications: micro, meso, macro, and multi-level. The differences are statistically significant ( $p < 0.001$ ) using the five-year increment measure. These differences are pronounced throughout the time span but attenuate at the break between datasets as well. The differential is most evident between micro and macro between periods. The advantage of micro drops from a 67% (73% to 6%) gap to one of a modest 25% (51% to 26%). In fact, the macro approach eclipses that of micro in 2007.

**Figure 8: Unit of Analysis**



**Figure 9: Level of Analysis**

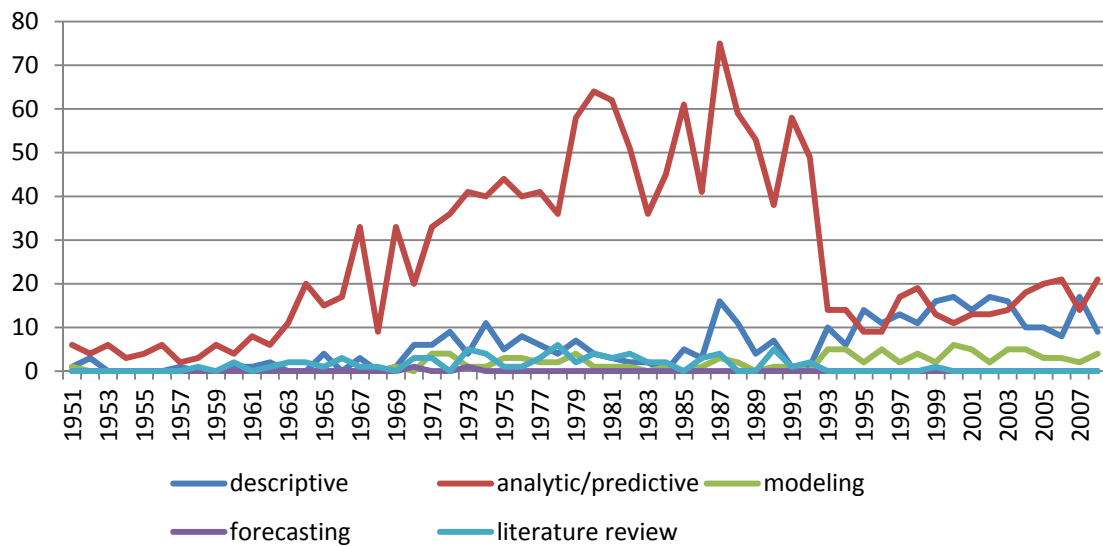


The differences demonstrated in the above were evident prior to the collection of the appended data. It was decided to further disaggregate the micro perspective into three sub-categories that are congruent with the earlier collection effort: individual, small group (i.e. 2 to 14 people), and interaction. Of the 229 references 206 (90%) are coded under the individual heading. As a result of this lopsided distribution none of the time cycles applied meets statistical significance in the chi-square analyses.

Over the course of the entire dataset the study orientation was coded into one of five categories: descriptive, analytic/predictive, modeling, forecasting, and literature review. The first two listed dominate the latter three in terms of their overall presence. During the first forty-two years the analytic/predictive orientation is dominant. Of the 2031 articles applying one of the listed approached three of four (N=1517 or 75%) use an analytic/predictive framework (Figure 10). The next most popular orientation is descriptive (N=231 or 17%) which meets or exceeds a yearly total of ten appearances three times prior to 1993. In contrast, the analytic/predictive orientation exceeds the thirty count mark for twenty-two consecutive years (1971-1993). In the appended data descriptively oriented articles—those aimed at simply documenting the prevalence

of a previously unexplored aspect of crime and its control—constitute 40% (199/500) and analytic/predictive—articles attempting to account for why an event occurs—nearly half (48% or 240/500) of those sampled. In fact each supersedes the other for eight of the final years included. As a result of the general absence of the remaining three categories greater than 20% of the cells have expected values of less than five thus negating statistical significance in the chi-square analyses conducted with the yearly-increment variables.

**Figure 10: Analytic Framework**



There appears to be a disagreement between the findings based on the quantitative and its qualitative companion. The scholars interviewed suggest a wide assortment of methodologies is used in accounting for the causes and correlates of criminality. The quantitative evidence drawn from the field’s research suggests otherwise. What accounts for the discrepancy? The supposition offered here is that the quantitative evidence account for methodology in more expansive, less specific, terms than that conceived by members of the interviewee sample. Only with the most general denotation of the term can the field be seen as having a consensus on matters of its methodology. The field is amenable to adopting methodology from any number of specific approaches that will grant it additional understanding of its subject matter; it is a prosaic,

ideologically neutral consideration. One interviewee, Lawrence Sherman, in fact spoke at some length of the efforts of which he is in the vanguard of to introduce experimental methodology to the mainstream of the field that would co-exist with the now dominant analytic approach. These efforts have resulted in the addition of both a journal and newly minted division within the American Society of Criminology. This adds yet another methodological arrow to the growing quiver with which the field aims to pin its subject matter.

## *Chapter IV: Boundaries, Departure, & Future of the Field*

The preceding chapter constitutes the base of the overall inquiry, its focus bearing on the paradigmatic bona fides of criminology's science. It is argued that the field's theoretical and methodological consensus also has implications for its professional stature among its social scientific peers. The present chapter extends from this foundation, building out from its philosophical core towards its professional periphery in three successive segments. Each is, by increments, removed from the element presented immediately prior. The totality of the investigation, consisting of both the previous and present chapters, is offered as a comprehensive inventory of the state of the field through pairing an assessment of its science with its practice.

The first segment, boundaries, unfolds in three steps. The first offers an account of what subject matter the field's scholarship is built around. The content analysis provides an indication of how the field defines itself in terms of its mandate or the subject matter within its purview (*The Center of the Field's Work Agenda*). The latter concentration draws attention to covering several animating theoretical and empirical/methodological debates and their resolution through inclusion of the interview data (*Internal Debate and its Resolution*). This offers an indirect method with which to determine two essential characteristics of the community's values. First, this gives insight into those elements around which consensus has accumulated. Those frameworks and findings that can be defined as essential to the criminological understanding will generate a defense. Secondly, accounting for what types of evidence are adduced and how debate is generally adjudicated, in terms of what arguments gain traction, serve as indicators as to how closely criminology adheres to the tenets of a disinterested pursuit of truth, the hallmark of a pure science (admittedly, this is an idealized—if not a caricatured—version of Science). The quotient of ideological versus evidential critique serve to indicate how much a field is belief versus empirically driven. The third and concluding portion will briefly discuss selected accounts of



where and how the field maintains its borders with external disciplinary actors in an effort to maintain its identity (*Defining and Maintaining External Boundaries*).

It has been widely accepted that criminology exists independent of sociology as an academic enterprise; on the college campus they increasingly maintain different physical territory. However, the question remains to what extent the two 1) occupy separate intellectual space and 2) demonstrate differing perspectives. A comparison of the attributes of criminologically themed articles appearing in disciplinary versus specialized (i.e. criminology or criminal justice) journals will determine to what extent this is the case. Furthermore, this will establish, empirically rather than anecdotally, how divided the two approaches are and when the departure began. Additional evidence drawn from discussions with the sampled criminologists lends further explanation to accounting for differing dispositions.

The concluding segment of the chapter recounts interviewees' projections of where the field is headed both professionally and intellectually; it is a prospective assessment. Two primary sources that may inhibit the continued growth of the field are a potential diminution in research funding and negative implications arising from its isolation from sociology and subsequent splintering of its research topic into ever more esoteric subject matter, oddly a byproduct of its professional success. A few expressed concerns over what they regard as the field's sensitivity to seeking legitimacy through being perceived as relevant, implying that the growing convergence between state and science bolsters the former to the detriment of the latter. Lastly, suggestions on where the future of the field lie centers on the reintroduction of the themes of culture and power into the mainstream of the field's organizing framework. After all, if the field is to remain a viable intellectual pursuit it will need to continue to deepen its knowledge through incorporating promising elements into its research enterprise, refining its knowledge.

## Boundaries

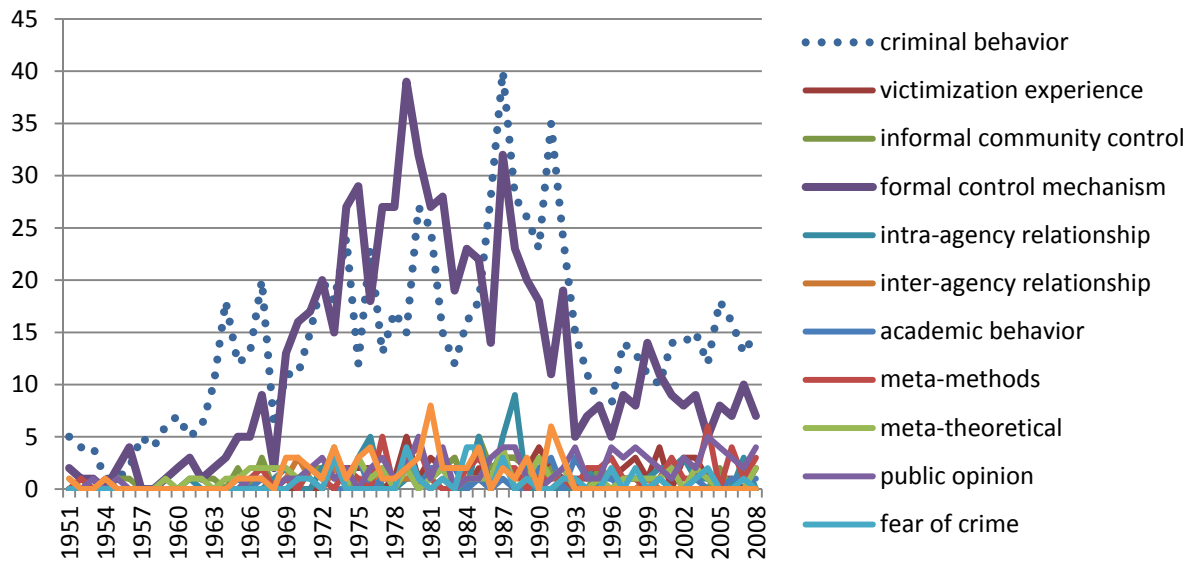
### *The Center of the Field's Work Agenda*

Criminology seeks, like any other field of endeavor, to delimit its task. It cultivates an understanding of a limited realm of the vast social sphere called human existence; the narrowing of its attention provides the promise of a concomitant increase in expertise. A look at the literature of the growing field offers indication as to what the collective enterprise is actively attempting to explain. The interests of the group, as seen through the scholarship generated, have traditionally concentrated around two particular points—accounting for criminal behavior and the justice system's response—as detailed below.

A given article's topical focus was coded for each and classified under one of twelve possible headings (Figure 11). Two topical foci are prominent throughout, criminal behavior and formal control mechanisms. These represent the primary dependent variables the field's research is attempting to account for. When combined, the research focused on explaining criminal behavior (N=832 or 39%) and formal control mechanisms (N=709 or 34%) represent nearly three-quarters (73%) of that published in top peer reviewed journals over the last half-century plus. In the early years the two categories vie for dominance in the literature, with the balance generally shifted in the direction of criminal behavior. From 1969 to 1985, with only three exceptions, research accounting for the application of formal control mechanisms is more prevalent. Over the remaining twenty-four years research articles directed at explaining criminal behavior are more present in twenty. The patterns displayed in their percentages (analysis not shown) over time reveal the same general relationships. Again, it is difficult to deny the influence of LEAA in directing the field's attention to matters attendant to the meting out of justice during the 1970's and early '80's. The pendulum then swung in the direction of criminology (i.e. attempting to account for criminal behavior) through the remainder of the study. As a result of the clustering of values among the remaining ten categories throughout chi-square

analysis revealed statistically significant differences at no aggregation earlier than the twenty year interval mark ( $p < 0.001$ ).

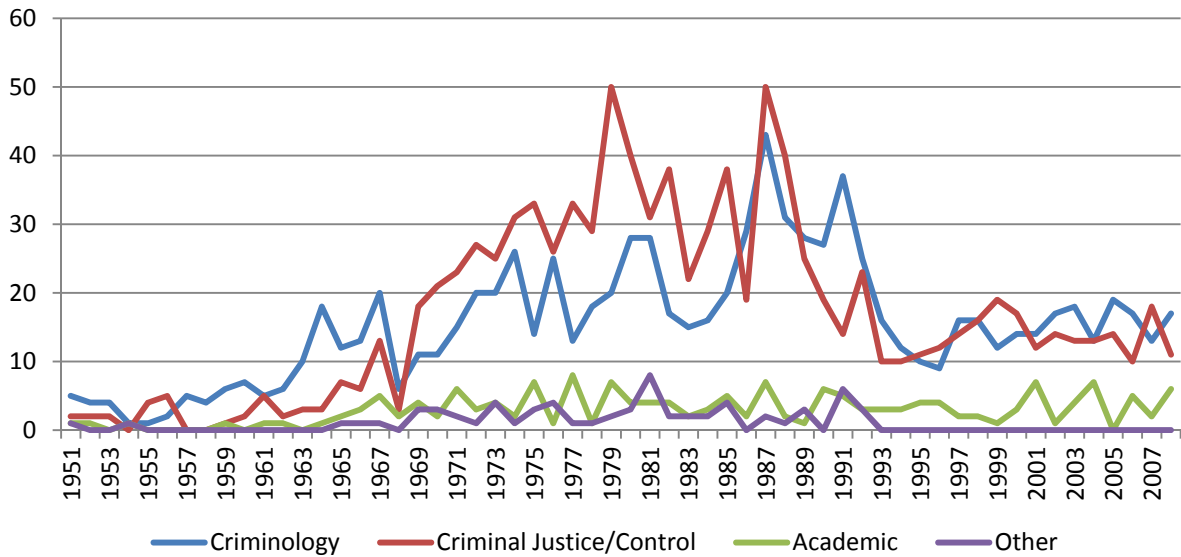
**Figure 11: Article Topic**



The difficulty in establishing statistical significance in the chi-square analysis necessitated the aggregation of data if more meaningful patterns were to be detected. Toward this end, the twelve categories were collapsed into four: criminology (criminal behavior and victimization experience) criminal justice (informal community control, formal control mechanism, intra-agency relationship, inter-agency relationship, public opinion, and fear of crime), academic behavior (academic behavior, meta-methods, and meta-theoretical) and other (other). The dominant trend lines between the aggregated classification scheme (Figure 12) and that immediately above change relatively little; criminology maintains an advantage through the 1970s and '80s before their trend lines weave throughout the remaining time period. Overall, criminology averages nearly half (48%) of the articles, with criminal justice closely behind (41%). Criminology holds an advantage verging on superiority from the beginning through the mid-1960s. The advantage then shifts to criminal justice which hovers around or above the 50% mark until the mid-1980's where criminology is nascent once more. With the exception of

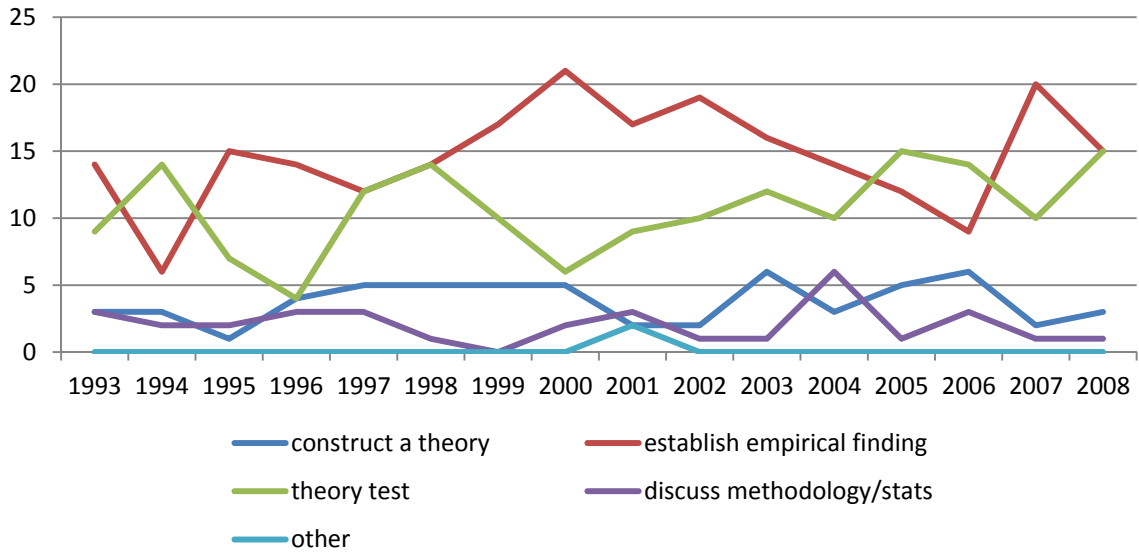
academic articles growing from 7% to 11% between datasets, the differences in terms of output percentage were minor. These differences between categories in the overall sample, however, are statistically significant at five-year intervals ( $p < 0.001$ ).

**Figure 12: Trifurcated Article Topic**



For the concluding sixteen-year segment of the data collected a variable documenting the primary purpose of the article was included. Each sampled article fell into one of five possible categories: theory construction, establishing an empirical finding, testing a theory, elaboration of methodology or statistical tool, or other. Chi-square analysis failed to reveal statistical significance in the differences between any of the yearly aggregations. This null-finding can be attributed to their being roughly two groupings (Figure 13). Less prevalent in the peer-review literature sampled are articles constructing a theory, discussing methods or statistical tools and other topics. The remaining classifications, establishing empirical findings and theory testing, are much more prevalent. For ten of the sixteen years the former exceeds the latter. When combined, establishing empirical findings ( $N=235$  or 47%) and testing theory ( $N=171$  or 34%) represent better than four of five (81%) of publications during the period.

**Figure 13: Purpose of Article**



Criminology's content, at least in terms of its peer-reviewed publications, is focused on the dual aims of accounting for the motives (or constraints) of criminal behavior and the criminal justice system response to these infractions, thus reaffirming the validity of Sutherland's enduring definition of the field. A closer look at the more recent research efforts finds that the field is in the business of, firstly, establishing empirical findings and, secondly, testing its theoretical propositions, thus demonstrating two Kuhnian attributes of a science. These two sets of trends are pronounced enough that they provide safe grounds for generalizing. What this characterization overlooks is the turbulence that is often roiling just below the surface. In exploring this point in the section immediately following we will disembark from that which is settled into the frontier of science where the rules become less firm and more improvised. In proceeding forward we will be revisiting the theme of cyclicity seen in the discussion of theoretical understanding in the previous chapter.

*Internal Debate and Its Resolution (Theory)*

The evidence presented in the previous chapter documents criminology's lack of unanimity in terms of having a comprehensive theoretical framework. Theories are offered,

explored, and atrophy, but never completely vanish. Several theories are also likely to be present in the literature during any given period. These observations beg the question of whether any elements are salient within the field, in addition to other relevant inquiries. What accounts for the reintroduction of theoretical frameworks? What influences their eventual success or failure? How do these perspectives overcome criticism? What accounts for their acceptance or rejection? Why are some more prevalent in the literature at different points? What factors influence their popularity? Most importantly, what do these transitions tell us about the field's fundamental understanding? What remains consistent throughout is debate. Through delineating the contours of the major arguments within the field and the substance of the disagreement, several interviewees have contributed to an appreciation for what the field regards as fundamental to its perspective.

The earliest episode recounted was surrounding that of Freda Adler's *Sisters in Crime* whose timing was fortuitous.

But since it was written in the early 1970s when the women's liberation movement was rapidly gaining strength and acceptance, the apolitical book that I had written suddenly became extremely political. In order to defend my thesis that changing roles of women in the legitimate world, changed their illegitimate roles also, I accepted invitations for debates, talk shows, and so on. I spent three years discussing my work in an attempt to warn that our criminal justice system should prepare for an influx of women. I began my discussions with Barbara Walters and Johnny Carson. For three years I defended my thesis in the media—both television and print. I debated the president of the National Organization for Women, I argued on *Face the Nation* and a host of prime time news broadcasts. I had written an academic book which was put into popular culture.

Events transpiring in the larger cultural milieu served to draw what was a minor concern within the field into fodder for a discussion of the place of women in society *writ large*. The thesis that as opportunities for women generally opened there would be a concordant rise in their presence in

the criminal justice system (as both client and personnel) threatened to reverse some of the advances of the women's liberation movement. The work, because it neatly captured then contemporary cultural anxieties, earned it a hearing in the popular media. Academic audiences are suspicious, at best, of having their arguments filtered through the popular press (Gieryn, 1999). So what accounts for the work having been reissued as a criminological classic a mere decade after its release?

The dominant consensus from the interviewees sampled is that the field demonstrates a commitment to assessing claims to truth based on the accumulation of empirical evidence. Some of the hypotheses at their inception are rooted in little more than intuitive suppositions which arouse interest. The field, if it is intrigued enough, will then summon resources to determine the validity of the claims proffered. Dr. Adler discusses the process in the following.

It was a classic because my hypotheses were supported over time. Women did move up. Women did change. The types of crimes they committed also changed. They also made an entrance and continued to move up as practitioners in the criminal justice system. Back in the 1970s when I taught at the National Judicial College there were two female judges in my class. There were very few female police officers. Remember that when *Sisters* came out there was very little competition. Females were not the subjects of criminological research. I worked for several years on the manuscript. I didn't have any political fears. I was publishing a scientific book. The times dictated otherwise. But when the furor calmed, it became a source for scholars to find research questions and to expand our criminological horizons.

BD: Now, what do you think the legacy of that work is?

The legacy of the work is that it opened a new area of scientific investigation for academicians. Major grant funding appeared to support research in the area of female criminality, hundreds of research projects began in the United States and abroad, journals

came into existence—many supported, and many rejected, my thesis. But the topic of female criminality was established within our discipline.

The reward and recognition for her efforts came in the basic form of its demand for work from the field. Her premonitions were borne out in the data that were subsequently collected; this required an initial investment in research to investigate her provocative claims. The affirmation provided by these first studies represented findings the field could mortgage to pursue further understanding. More importantly, her thesis provided a foundation for the extension of the field's research efforts; it was the androcentric field's discovery of the female. It met a professional need to fill vacant intellectual space with knowledge or at the very least it was a topic that generated a considerable amount of debate. The field's professionals now had both research funding to augment their accumulated understanding and journals within which to publish. This practical concern succored the ascendancy of the field. This allowed academic researchers opportunities to bolster their academic credentials through publishing; scholars could therefore lay legitimate claim to tenure. Succinctly stated, Freda Adler is recognized for having made a landmark contribution to introducing a compelling research agenda. It is difficult to deny that the agenda and, by extension, the field's researchers reaped dividends for having been so closely coupled with policy concerns in this instance. The high-profile debate encouraged policy makers into allocating research funding to preparing an adequate systemic response to Dr. Adler's forecast.

Charles Tittle elaborates on the theme she raises, to the effect that if criticism can manage to generate research interest then the field will reward the messenger and their perspective. His early work challenged the primacy of the then dominant labeling theory as well as the solidity of the socio-economic status/crime link. This aroused some initial criticism but has gained a measure of credibility over the years. When asked how it eventually came to be accepted he replied,



Well I don't know if I overcame it [i.e. the criticisms]. The honest answer is that my challenges made work for people in responding to my critiques, analyses, and so on. My work provoked other people; stimulating them to prove me wrong. Anybody can succeed in our business if they can come up with an idea or a theory that is simple enough that a lot of people can use it, do research on it and get published. In other words, people become well known by making work for the community of scholars.

BD: I think there's a lot of truth to that.

So it's not that I've been influential in a compelling intellectual sense. It's that I've been a pain in the ass for people. And, being a pain in the ass means that I have made work for others, and such work allows them to develop a career.

These efforts were successful at least in the sense they were articulated compellingly enough that they were not blithely dismissed. His agitation managed to spark a genuine reevaluation of the conventional thought on several vital issues throughout his career. In many ways his reopening matters long since settled could have threatened his professional credibility if some empirical support failed to materialize; a science can tolerate only so many Cassandras. Elsewhere in the discussion he mentioned the renowned sociologist Peter Rossi's thundering that to question the SES-crime relationship as being anything other than negative was tantamount to stating the earth was flat. This reaction betrays just how ensconced the finding had become and that the field had invested a considerable amount of intellectual capital in maintaining it as truth. In the end, he offers that a science is not to be identified for a finding it establishes but for an approach to understanding the world.

Francis Cullen relates a similar scenario below. His effort at rehabilitating rehabilitation, the correctional doctrine left in tatters by Martinson's "nothing works" revolution, was poorly timed. Its saving grace was that his stance proved prescient. He mentioned a colleague's comment that his research agenda had him "pissing in the wind" and jokingly referred to his coauthored work, *Reaffirming Rehabilitation*, as being fortuitously named by virtue that anyone

looking to quote “some idiot who still supported rehabilitation” would cite the work. Similar to what transpired with reference to Dr. Adler, cultural events unfurled in a manner consistent with the arguments espoused. His work was one of the first to question the prudence of the compromise between the political left and right in brokering a deal that relocated discretion from correctional experts and bureaucrats to legislators. In many regards his assessment proved to anticipate future events.

And it is important that *Reaffirming Rehabilitation* predicted that the alternative to rehabilitation would be much worse—I don’t think that there’s anyone that can dispute this conclusion. Now, if we had retained a correctional system that had discretion, would things have been bad? They might have been bad, but not this bad. The whole point of my book was that scholars voiced justifiable critiques of the therapeutic ideal. But what they didn’t understand was that the alternatives to rehabilitation were much worse—in terms of how the system would be oriented, how power would be distributed, the kind of people that would be hired to work in that system, and the punitive rhetoric that would be legitimated. So that was the warning, the caution, that *Reaffirming Rehabilitation* voiced. I think that prediction, that warning, is pretty hard to dispute; it was almost certainly correct, or at least partially correct.

His argument added a wrinkle that had been glossed over up to that point. The assumptions upon which the dominant antagonists at that point offered he found dubious. Much of the debate was centered on explaining alternative models and incorporated what he properly diagnosed as a misconception. The contribution he offered brought differing evidence to bear; it recast and renewed the debate.

The odds of revitalizing the moribund rehabilitation correctional philosophy seemed especially weak at one point. The well-nigh universal condemnation, from both the political left and right, served as an epitaph for what was argued to be a self-evidently failed correctional approach. Unexpectedly, however, Dr. Cullen finds that Martinson’s *coup de grace* resulted in a

tectonic shift in the debate, one that ultimately served to resuscitate and relegitimate rehabilitation. It was that shift that allowed he and a group of others—known colloquially as “the Canadians” (several colleagues at the University of Cincinnati were mentioned elsewhere as well)—to resurrect it.

But there was an important ironic and long-term effect of Martinson’s work. It changed the terms of the debate from a critique over state discretionary power to the empirical issue of whether one could show that rehabilitation reduced recidivism. That is, Martinson succeeded in framing the debate as an effectiveness issue. Those who attacked rehabilitation said, “See. It doesn’t work.” Once you put things in those terms, then the validity of rehabilitation rests on this question: Is it effective or not? So, in the subsequent decades, scholars—many psychologists and from Canada—marshaled data, including meta-analytic reviews, to show that offender treatment worked. This scholarship instilled rehabilitation with renewed legitimacy—again, thanks, at least in a way, to Martinson!

In some ways he managed to alter the terms of the debate but in other important respects the debate remained untouched. What may have contributed to the success of renewing interest in rehabilitation was that the terms of the debate were accepted by the parties surrounding the debate. Rather than taking a root-and-branch approach to reasserting the value of rehabilitation, thus requiring the tiring expense of arguing for an entirely new perspective, the terms of the debate were simply acceded to. Thus the argument managed to capture some of the intellectual inertia of the “nothing works” movement in advancing its antithetical claims.

Jack Gibbs is recognized for having reinvigorated the deterrence perspective in the immediate aftermath of labeling theory’s supremacy. The marked emergence of deterrence in the wake of labeling’s demise just a few short years before suggests that the pendulum of the field, as well as that of the culture at large, had swung swiftly in terms of its political orientation. This transition did not come without some friction however. Dr. Gibbs with tongue in cheek described

the exchange between the respective camps as “rather warm”. Here he recounts how his revival managed to reframe the debate.

But I renewed the old question of crime, punishment and deterrence. Rather than look at the death penalty which had dominated research prior to that I looked at the certainty and severity of imprisonment. I thought the findings were rather impressive. It appeared amongst states the greater the certainty of prison, the lower the homicide rate; the greater the severity of sentences for homicide, the lower the homicide rate. But the immediate point, Brendan, is this: prior to my sort of renewing—that sounds all too grandiose I know—prior to my renewing the question about crime, punishment, and deterrence, deterrence research had been dead. The findings on capital punishment all but buried the question. The line of reasoning was very curious. It seemed to be something like this, “Well if something as severe as capital punishment does not deter how could anything else deter?” I think that question was wrong because that ignored the certainty of capital punishment.

In a sense the identity of the theory had been transformed, its manifestation made over. The posing of a new research question, within the field’s foundational theoretical perspective, served the additional benefit of making work for the field once more. Those wishing to defeat deterrence theory would be required to meet the perspective on different plane of the debate; supporters had new grounds on which to defend the perspective.

The sense of solitude Dr. Cullen alluded to at one point in his pressing a faded agenda forward was echoed by William Chambliss. When he began working within a Marxist inspired agenda it was lumped with Stalinism and communism which were thoroughly discredited, if not outright scorned, in academe. Nevertheless, he and his agenda managed to endure and earn a few converts. When asked for a few remarks on to what he attributed his professional success he first quipped, “Of course, the quality of the work. What else could it be?” He then demurred,

I think that to some extent it was being involved in the early stages of several different movements that turned out to have legs, as they would say. I began writing in the sociology of law at a time when nobody was writing on the sociology of law. In fact, when I started writing in the sociology of law I was convinced that no one would ever really give me credit for that and that I should also do other kinds of research if I was going to ever get tenure or ever continue to work in a university. Because the sociology of law was simply not a discipline at that point; nobody had done it since Max Weber. The other thing was that I was one of the earlier people to start the area of conflict or critical, Marxian, or whatever you call it, criminology in the sociology of law. So the fact that I was interested in these things at an early stage I think was one of the important reasons why I've gotten a lot of recognition for what I did. I think that the other thing was, and it's similar to the other answer, was trying to ask different questions than conventional criminology was asking. I was trying to look at it from a different perspective. I think that made my work more visible than it would have otherwise been.

At first the agenda was simply ignored; in many respects this is worse than earning derision, as this is a sign that the perspective is inconsequential to the point of not meriting a rebuttal. At a later point it managed to irritate the field (see the rather testy exchange in the February 1979 *Criminology*). What eventually earned it a measure of success is that it, like so many of the examples cited above, offered a growing field new questions to ask. The radical perspective brought to the field an alternate understanding, one embedded within the heritage of holy trinity of modern sociology—Marx, Weber, and Durkheim. A familiar trope was offered a new varnish in order to attract a new cadre of proponents.

How can the success of this approach be gauged? If a once controversial contribution eventually finds its way into the mainstream the body of scholars has legitimated it by making it a referent in the conversation.

I would qualify what you said about the conflict perspective being marginalized. In fact, I see the conflict perspective today as being more accepted than it was thirty years ago, in this sense, that every textbook, every reader contains conflict perspective as a legitimate perspective. Whereas thirty/forty years ago when I started writing from that point of view, and people like Turk, Quinney, and others were writing from that point of view, it was pilloried as not legitimate, as being political, as being Marxian, as being pro-Soviet Union. It was pilloried as being completely out of the mainstream. So the arguments were very very vicious. Today its importance has been usurped in a way because now people say, "Oh yeah, that's a legitimate perspective. It just asks different questions and has a different point of view." It, in a sense, has become more legitimate, not less legitimate. But I agree with what you said that it's more marginalized in that people pay less attention to it. It's now just become one more perspective amongst all these different ones.

Dr. Chambliss' reference to contributions from a conflict perspective being part of a textbook approach is not a trivial one. Textbooks—as Kuhn points out—are where knowledge that is least susceptible to problematization is memorialized. The base of knowledge represented between its covers becomes the starting point from which future generations are encouraged to begin their journey to Truth. His reference to the pitched debate between the conflict and mainstream elements is indicative of the conservative tenor of the field that prevailed at one point. At some point an ideological thaw of sorts set in and the conflict perspective was acknowledged as valid. The attacks were ad hominem at the outset but eventually mellowed, through what process it is not discussed, to the point where at least a *détente* has been reached; critical and mainstream theory now co-exist. Science develops, he suggests, through a process conjectures and refutations (Popper, 1963).

The animating debate within the field in the 1990s was between life-course theory and self-control. Robert Sampson, one of the primary protagonists, spoke of the battle as being paradigmatic in the sense that both parties were articulating theories with corresponding methodological frameworks. Like Dr. Chambliss he offers that science advances through a process of contention and compromise. As with the earlier experience, conflict arose with a challenge being leveled at the dominant theory. Life-course theory (Sampson & Laub, 1993), although it can be considered a cousin of self-control within the control theory family, was critical of what was then a theoretical juggernaut at that point in time (self-control).

Specifically, after *Crime in the Making* was published, well actually during our writing of that work we began to question some of the arguments in Gottfredson and Hirschi's *General Theory of Crime* which had come out in 1990 on stability of crime and specifically the notion of self-control in crime. Travis and Mike were both mentors. That was interesting. The fact of the matter is that *Crime in the Making* was still a theory of informal social control. It's still a social control theory. Looking back, but even at the time, I think what we were trying to do was to say that, "Look, both self-control and social control are important." We gave credence to changes in informal social control throughout the life course. In other words, age graded informal social control. At the time we thought that that wasn't consistent with the general theory of crime. Now since then some people have argued, "Well self control and social control are more compatible." And that debate is still going on. That was a debate that took place over a number of years. I think it was a healthy debate; it was a good one. I learned from it.

Although there were disagreements over the fundamentals of the stability of criminal propensity he acknowledges that both self-control and life-course theory were built on the same foundation of social control. In the end, he states, self-control and life-course theory are more compatible than

inimical. This was accomplished through contrasting both with Terrie Moffitt's developmental taxonomy; similarity is a relative term. The compromise that is emerging has been facilitated by both being set within the control theory perspective. The terms of the debate have been at least tacitly agreed to; it is confined, the parameters limited.

In recounting the debate he begins with the conclusion. In the beginning the negotiated compromise was far from foreordained. The parties entered battle with their polemics tempered, only to find them dulled through repeated contact.

I think we probably, in *Crime in the Making*, perhaps went too far. And I think general theory went too far as well. I think we both put forth perspectives that were a little bit more—how shall we say?—if you take sort of a purist level and you're pushing the logic of the theory we pushed the logic of life-course theory pretty far. I think it was a healthy move. It was an important debate I think, standing back, in the sense that it did generate research. I thought that there was interesting analysis of the debate in several different quarters, one by Larry Cohen and Brian Vila in I forget what year it was published [1996] but it was a very interesting article that analyzed *Crime in the Making* and *General Theory of Crime*. From the perspective of intellectual history I thought I was a nice adjudication. But I think it also led to debates about method, particularly a concern that we had that methods were driving a lot of the substantive agenda in criminology particularly in the life-course. That was over methods for analyzing longitudinal data.

The debate began with each bringing, at least in retrospect, an unforgiving approach. What made the debate pivotal, as opposed to peripheral, were two items mentioned within the context of the quote. Its centrality is firstly a function of its having been responsible for generating research to adjudicate claims. New questions demand new answers. Secondly, the debate over appropriate methodology undergirds the empirical element of criminology. The analysis of longitudinal data to determine the validity of life-course assertions required methodological innovations, the appropriateness of any given approach inevitably generating yet more debate. This affirms the



insights granted earlier by James Short and Steven Messner that the field holds a reverence for the integration of theoretical and methodological approaches.

*Internal Debate and its Resolution (Empirical Findings/Methods)*

The preceding has devoted space to elaborating on a few of the primary theoretical debates that have transformed the field over the past half century from Freda Adler's introduction of the study of female criminality to the self-control/life-course exchange. The field has also witnessed a few empirical/methodological debates as well. Some have capitalized on this through contributing to on-going theoretical debates, thus maximizing the breadth of audience by increasing its relevance. For example, in work cited immediately below the use of longitudinal data to determine the shape of the age-crime curve had implications on the theoretical debate mentioned immediately above.

Application of the term "debate," which implies an exchange of ideas, may be a misnomer in the instance described by Janet Lauritsen however. Her critique went unrequited.

One was early on, in which I found that it was difficult to use longitudinal, individual-level data to study growth curves in delinquency and victimization because the data lacked reliability over time [Lauritsen, 1998]. I thought at the time that graduate students and other researchers would want to see if that was true in other data sets, especially some of the other individual-level, longitudinal studies. How good are our data for studying delinquency and victimization trajectories over time? Unfortunately, that question has not been studied much - I have not seen many replications with other datasets. Maybe it has been done and the findings are buried in technical reports somewhere. I think it is an important enough issue to have been addressed and published elsewhere. However I was told by someone working in another discipline (a biologist) that whenever you publish something that appears critical of a dataset or methodological approach you should not expect much response because few researchers want to criticize what they have spent years working on. Nobody wants to know whether their own data

might be flawed because they have made large personal investments in it. So the piece about potential problems with the over-time reliability of longitudinal data ended up -- well, apparently it's read in methodological settings - it has been reprinted in methodology books. But what I was hoping future research would do - that hasn't happened. If measures in one dataset suffer from this problem, a similar set of measures in another dataset may not. And maybe that's because of the ordering of the questions in the survey, or the content of the survey itself. We have not gotten many answers about this issue. We can't learn how extensive this problem is because there are few published research studies about this. So this has been a concern of mine.

The shortcomings of earlier research pointed out by her contribution may have been so catastrophic to several research agendas that it left the field dumfounded. The critiques mentioned in the section above all had the practical virtue of generating a response through encouraging research. This contribution lacked for this pragmatic redeeming value. Longitudinal data, by definition, takes years to develop; generating alternate methodology also requires an investment of time; a response cannot be generated overnight. This is precisely what makes Dr. Lauritsen's analysis so devastating and the silence in response frustrating.

As with theoretical perspectives that are falsified via atrophy her critique will be salient through encouraging the redesign of longitudinal methodology. Insiders pay heed to the cautions specified even if the contribution is not cited explicitly. This is the best reaction a flatfooted field can muster in response, extending a tacit promise to make amends with future efforts.

I do know however, that the issue has been tested in the P[roject on] H[uman] D[evelopment in] C[hicago] N[eighborhoods] study because I asked Rob Sampson and Steve Raudenbush for comments on an early version of the paper to make sure I wasn't out of my mind - that my analyses were sound. Their assessment using the PHDCN showed that those data did not have the same problems. But I haven't heard that this

issue was examined in other studies. If you challenge something, that challenge can easily be ignored.

Mention of having the paper reviewed by two colleagues to “make sure I wasn’t out of my mind” is indicative of having encountered a startling discovery, contravening research that is embedded in the literature.

The fact that it has managed to make its way into textbooks while it is simultaneously buried in technical appendices is a paradox. This is suggestive that the field recognizes the power of her argument but has not yet managed to implement ameliorative measures or at least offer a cursory attempt to diagnose the origins of the problem. The field recognizes the symptoms but has yet to grasp the etiology of the problem. At some later date it may take up the challenge Dr. Lauritsen has articulated through generating research. In the event this happens she will earn greater recognition for her contribution as the careers of others cited throughout attest.

D. Wayne Osgood recounts a similar situation involving a debate centering around one of the bedrock findings in criminology, the association between delinquent peers. The events recounted follow a similar pattern. An anomaly is found, remedial measures implemented, data are gathered and tested, and, finally, acknowledgement bestowed.

The biggest one I’ve had to deal with in recent years is nothing like that. It’s about how to measure peer delinquency. From 1980 to ’86 my main job was on a study of kids in training school and particularly on peer group influence. Out of what I was doing I got pretty convinced that it was really off the mark if you just asked kids how bad their friends are because it doesn’t match up worth a damn with what you’ll find if you ask their friends too. A piece I coauthored not too long ago got very strong reactions from some reviewers about pushing that point. So I pushed harder.

The field, to that point, had established an accepted measure of accounting for peer association and delinquency. His co-authored work aroused criticism from reviewers, an indication of the field’s devotion to its conventional method. Rather than capitulate the point it was pressed with

more vigor. Ultimately this argument required an objective measure to determine which party was more accurately capturing the association.

Fortunately for the argument advanced there is a highly regarded dataset available that served to arbitrate the debate, The National Longitudinal Study of Adolescent Health. When asked if this can serve the end of definitively settling the argument he remarked,

I think it will. That really brought this to the fore. Now there's a really good dataset that had that. Now there's a lot more peer network stuff going on. I think the evidence is pretty straightforward to me on that point. I think there's enough new stuff going on with network type stuff. If you know who people's friends are and you're asking them about their friends and you have data from their friends, it just opens up more kinds of interesting questions. I think that's going to overwhelm the folks who don't like it.

The introduction of the dataset has facilitated the adjudication of his contention through spurring research. The dispute is apropos of one of the oldest feuds in criminology, that between social control and differential association. The fact that it is a methodological-empirical problem bearing on an enduring theoretical dispute raises the profile of the issue.

He, like several other interviewees, has faith that his argument will ultimately prevail as a result of the evidence adduced in its favor rather than it cohering with some preconceived ideological preference. In a similar spirit Francis Cullen confessed to voting left while being a committed positivist; that is, his political inclinations are separated from his scientific approach. There is an understanding that the community of scholars comprising criminology is responsive to thoroughly documented empirical findings, rather than being a group beholden to ideological predispositions. Here it appears that a new consensus is emerging replacing the old on community's verdict on the merits of the case. From this point a veritable normal science will begin anew. The status quo is being reset once more.

### *Defining and Maintaining External Boundaries*

The quantitative data gives indication to a few of the defining characteristics of what criminology and criminal justice are about with regard to their research agenda. Part of the process of determining boundaries and in creating a more or less robust identity involves drawing contrasts with other traditions through the method of defining what it is not as well. Francis Cullen elaborates on the early departure of the field into a more coherent agenda that emerged from a more nebulous one, specifically, justice studies.

The first thing is that a number of years ago there was a movement to turn criminology into justice studies. You find, that's why the journal *JQ* is called *Justice Quarterly*. It should have been called *Criminal Justice Quarterly*. But the first editor was Rita Simon who had just become the dean of the school of justice at American University. The people in charge of the committee to name *Justice Quarterly* purposely named it *Justice Quarterly*, in my view, and not *Criminal Justice Quarterly* because they wanted to incorporate that broader sense of justice. That's how you got Arizona State which went from a school of criminal justice to a school of justice. Over the years, it moved so far away from criminology that John Hepburn and Scott Decker created a separate criminal justice program at ASU. I interviewed at Arizona State many years ago when John was on the faculty; he might have headed up the search committee. In any case, the original ASU program has been transformed and the school has every kind of justice study that you can imagine—and it's only tangentially related to criminology. So, justice studies took hold in some prominent places, but it did not, in the end, displace criminal justice. Part of the reason, I think, is that it was too leftist and a lot of students really wanted to do criminal justice—which is why some schools with the name justice studies in reality do criminal justice. And the schools that really went far in the other direction, like ASU, are almost a different discipline now. So I don't think we're a part of justice studies.

Here we find the origins of the ACJS' flagship journal, *Justice Quarterly*. In this recollection the more specifically criminal justice agenda derived from a much broader and abstract conception of justice. This conception is institutionalized in the department of justice studies at the journal's original home, Arizona State University. The agenda of the department drifted to the point of only being peripherally related to the goings-on within criminal justice yet the name of the journal remains. Some of this departure has been attributed to leftist ideological leanings of the justice studies movement vis-à-vis criminal justice (a theme elaborated upon in a subsequent section). Dr. Cullen concludes with the second portion of his reply remarking that law and society concerns fall at the borders of the field.

The other area where it gets fuzzy is the whole issue of law and society because that starts getting into other forms of social control and into critical legal studies, which I see as outside criminology. So I would say that there are two ways of seeing the boundaries of our discipline. Think of a song that ends: "boom." Its over; the ending is clear. Now, other songs on the radio sort of trail off. Criminology is like the latter songs. I think our discipline sort of trails off on its outer edges. I think there's a clear core to the discipline; most people pretty much study crime and its control. But as you get to the boundaries, the peripheries, the discipline sort of fades away. And I think that there are some things that work their way into the field and other things that don't.

The interviewees all explained the field's collective efforts, in one formulation or another, as being directed at improving our understanding of crime and its control. Defining it at its extremes (e.g. deviance, legality) proves more of a chore however.

Endogenous boundary drawing is marked through efforts to define criminology's subject matter through developing a sharper image of where it lies in terms of the fields more proximate to it. Boundary maintenance also requires the defense of intellectual territory from competing disciplines intent on franchising space within it as well. Most mentioned throughout the

interviews were the repeated efforts of economics in pontificating to the field how it ought to conduct itself.

One of the big disappointments with me, and that's one of the reasons I wrote that article back in 1990 on rational choice [Akers, 1990], is that as I said earlier, I have focused on having as many disciplinary approaches as we can in criminology. I'm all in favor of that. I want that, but I want it to be done in the right way. I don't want someone coming in from another field, another discipline, who's never had any knowledge of the field to come and tell us what's going on. That's what the economists did. They did that first in the '60s in terms of the criminal justice system. Then they came in and told us this in the '70s with regard to the explanations of crime. Then they came in and told us this about capital punishment. If you read what they write they don't cite anybody, except other economists. So they come in with a certain amount of assurances that they can predict and solve and so on. Well, economists didn't predict the crime decline either. But you read some of them now and they say, "We knew this all along." Well, they didn't know it. And they have not explained it.

Multidisciplinarity of approach is explicitly encouraged, but only if done in a deferential manner. What Dr. Akers is objecting to is the flagrant omission of criminological contributions by external agents. Economists who deign to inform criminologists—who have devoted their careers to studying crime and criminality—that the accumulated wisdom of the field does not merit mention it earn the ire of many. For this reason economists are often characterized as interlopers. They are often spoken of in the contempt soaked tones which one would use to refer a deadbeat dad who dispenses unsolicited advice on how to raise one's children.

Economists have a record of recidivating when it comes to directing the field of criminology how to study crime. A recent entrant into the competition to attempt to annex a portion of criminology is public health. Janet Lauritsen spoke of how the urgency of having to confront violence in emergency rooms in the early 1990's and the concomitant expense that

accrued as a result gave them primacy in garnering funding to address the problem. While the NIJ, in her account, devoted its resources to improving policing and technology the Centers for Disease Control and the National Institute of Health were meeting the crisis in trauma centers. Here is an instance in which proximity to the problem lends public health additional stature and resources.

Drawing bright lines between the field and its competitors has proven difficult for criminology given that its dependent variable is relevant to any number of social sciences. As a one-time custodian of the field's flagship publication, *Criminology*, Charles Tittle's editorial philosophy played a role in defining what disciplinary approaches were appropriate.

As far as what's relevant, it's pretty hard ahead of time to set forth any standards like that because some of the things that are actually relevant to crime related phenomena don't have much to do, ostensibly, with crime. For instance, if you have a theory about the way people make risk assessments, and there's a big body of literature in both psychology and sociology about how people assess risk, most of it has nothing to do with crime. But it's highly relevant to the crime question because a lot of criminal behavior is risky and you have to know how people assess risk and you have a whole legal system based on the assumption that if you provide penalties people will recognize and shape their behavior around them. In other words the assumption is that potential offenders will assess the risk rationally and act on those assessments. Most of the literature on risk assessment is not by criminologists; it's by psychologists and sociologists interested in things besides crime. When a paper showed up while I was editor that I and reviewers thought was strong work and could be construed, even indirectly, as having some relevance for crime, I was quite willing to print it even if it didn't have a word that said criminology in it anywhere. Some people have trouble with that approach because they like to think that it a paper has to say criminology or say it is about crime. But to me that



doesn't make any sense because criminal behavior is not that different from lots of other kinds of behavior.

This philosophy, one that is shared by several other interviewees, looks askance at establishing predetermined boundaries on what is germane to criminology. Coincidentally, another interviewee, James F. Short Jr., spent a period of his career exploring the ostensibly tangential concern of risk analysis mentioned by Dr. Tittle. Dr. Short mentioned developing an interest in the subject matter to enhance his reputation as a sociologist while removing some the narrowness of his identity as a criminologist. His presidential address to the American Sociological Association on precisely this topic attests the strength of his instincts (Short, 1984). However, in the next breath he recounted his mentoring a doctoral student who applied his work to explaining criminal behavior, lending credence to the editorial stance espoused above.

Rather than imposing boundaries by fiat those approaches that serve the ultimate utility of offering a more comprehensive account of crime and criminality are all welcomed regardless of origin. Criminology has to be as inclusive as possible as a result of crime being not all that dissimilar from a great number of legal activities. At least in the short term criminology will be receptive to including contributions that are seemingly remote.

D. Wayne Osgood also enjoys the eclecticism of the field. His only problem is that there may not be enough. He regrets the tendency toward structure and rigidity and away from entropy. Professional concerns serve as a brake on border crossing however. When asked if he has defined a notion of what falls within the field versus what lies beyond when reviewing a manuscript he offers,

In general, I more often have the opposite reaction. For instance, there are psychologists, community psychologists, political science folks who are doing work about programs meant to impact aggression and violence amongst kids. That's really relevant. And their dependent variable is delinquency. I wish that we'd get more of that work published in our journals, if we want to understand crime and delinquency we should know about that

stuff too. Now they're publishing for the audience that's relevant for where they want their careers to go, which is appropriate. But we shouldn't be ignorant about it. So I don't see much in the way of boundaries as an issue. I see lots of overlap. I don't see anything wrong with an economist studying the same thing I study. But if they do I'd like it to work out so that we hear about it from each other. Sometimes that doesn't happen as well as you'd like.

What handicaps the sharing of findings across disciplines are the professional concerns that are structurally embedded. Professional rewards are predicated on publishing in outlets the respective fields determine as prominent. The boundaries are erected to meet this exigency, not necessarily as a result of the subject matter being isolated. In terms of publishing, professional considerations trump science. Dr. Messner alludes to this in his discussion. He reduces the difference between economics and criminology in accounting for macro level influences on homicide to one of semantics; each has developed its own argot to define roughly equivalent phenomena.

Lawrence Sherman speaks of this multidisciplinary ferment as holding the potential for advancing the field's understanding. A one-dimensional theoretical or methodological approach will not suffice to push us further into accounting for heretofore unexplained variance. He presents a metaphor in which criminology benefits from the structural inclusion of various approaches and their being tested in through their application as policy.

So I'm very optimistic that accumulation will not only be promoted by a multidisciplinary way of thinking about crime but will become more powerful by way of integration and cure the longstanding problem of unexplained variance that we find whenever we use only one level of analysis or one disciplinary/explanatory framework for understanding crime. As for discarding theories, let me give you a metaphor that I think describes what we're trying to do at Penn and that is of a wheel in which the hub is theory, explanatory/predictive theory, which would be constantly forming new

hypotheses and discarding old ones when they should be discarded, with spokes that run from theory out to the wheel of policy application but also spokes running from the rim of policy application that represent different explanatory frameworks and research methods that feed into the hub of theory. So that you'd have a neuroscience spoke; you'd have a statistics spoke; you would have a history spoke; you would certainly have a sociology and psychology spoke, or social psychology or clinical psychology spoke.

Criminology's science is conceived as a step-by-step narrative in which theory is generated and tested. In agreement with Dr. Tittle he places the emphasis on process as opposed to the identity of the contributors. An idea is fed into the system, eventually modified, and then subsequently fed back into the process. In this respect the Sherman Wheel is an object in perpetual motion, as is criminology's search for additional explanatory power. Here, cogently distilled, is an account of the field's cyclical development.

Criminology defines its research mandate as one of explaining the causes of and reaction to crime. How the field goes about this, as seen in both the theoretical trends results as well as the present chapter, is subject to periodic shifts. Older theoretical frameworks are set aside and new methods challenge the established conventions with consistency. These trends are partially responsive to exogenous cues drawn from the socio-political culture. The primary contributing factor to the eventual success of these is the proposed challenge's prospects for exciting research in substantiating or rejecting the claims proffered. An additional contributing factor is in a given framework's potential for creating alternate research agendas through reframing of debate. This creates new research questions either *de novo* (e.g. Adler) or through revisiting elements of a dormant theoretical tradition (e.g. Gibbs). Making work for the field's practitioners can eventually earn scholars recognition in the event the critique successfully weathers the reaction it provokes. Criminology as a field is committed to assessing claims through evaluating the accumulation of empirical evidence; the role of ideology has been muted. Those scholars who raise these criticisms and have their critiques validated eventually earn professional recognition as

a due reward for having risked intellectual capital in voicing concerns. There is, of course, a delay between the offering of these critiques and vindication.

The field is now an independent venture. Within the smaller academic sphere the field has managed to successfully distinguish itself from both justice studies and law and society. The field has also rebuffed, with varying degrees of success, attempted efforts of disciplines such as economics and public health in injecting their disciplinary frameworks into the field—they are accepted in part but fail to dominate. These competitors tend to arouse suspicion and resentments, the former for a perceived arrogance and the latter for successfully siphoning research funding. However, as seen in the reflections on the state of the field's theoretical consensus, there is reluctance, if not outright rejection, of any attempt to impose firm boundaries on the profession. The field appreciates the benefits that accrue by virtue of allowing an expansive breadth of disciplinary emphases within it. Setting foreordained restrictions is inconsistent with its ethos of maximum inclusiveness. The field is *multidisciplinary* rather than *interdisciplinary*; the disparate elements congregate with one another but do not act in concert. This recent diffusion is in marked contrast to the balance of its existence in which criminology developed its enterprise under the nearly exclusive tutelage of sociology.

#### *Departure*

Criminology has managed to establish its own academic market, complete with the accoutrements of respected peer-reviewed journals and graduate departments. A look at the development of the field shows a quick departure from sociology beginning roughly at the time of LEAA's birth in the late 1960s in terms of where each opted to publish research findings. The theoretical and methodological content of the criminological work appearing in the sociological versus criminology/criminal justice journals show statistically significant differences over the time period as well. The separation of the two approaches has been ascribed initially to marginalization, then to creativity. Sociology felt the upstart field wanted for rigor in research in recommending criminologically oriented research to appear in specialized journals. Criminology

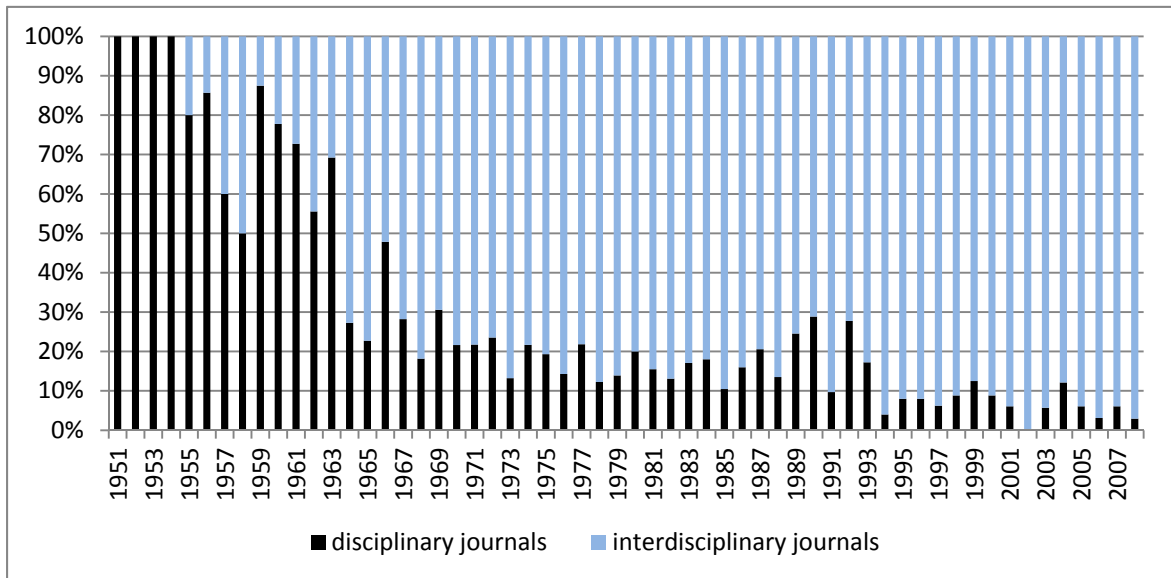
responded through developing its science and its journals. These outlets are now recognized as equals in terms of their intellectual content.

The professional divide has contributed to the divergence as well. Sociology and criminology/criminal justice departments evidence differing dispositions to a fundamental question that underpins the question of social order—what is the proper role of the state? Sociology as a discipline, with its tradition of activism, is generally critical of the application of state power in controlling deviant populations. Criminal justice is less suspicious of these kinds of interventions. Stated alternatively, sociology has a left leaning political orientation and criminology a relatively conservative one.

#### *Exodus from Sociology Journals*

An important signal of a division between studies or disciplines is the degree to which they inhabit different intellectual space. The quantitative data collected indicate whether a criminologically oriented article appeared in either a disciplinary journal (sociology outlet) or an interdisciplinary journal (Figure 14). Over the years criminology has migrated from its home in sociology to specialty journals with a measure of rapidity. 1966 marks the last year in which sociology journals presented near half of criminology's peer reviewed output. From 1967 to 1992 this average drops to 19%. In the appended data, with the inclusion of randomly sampled article drawn from six additional journals, all but one of which was interdisciplinary, this average drops to anemic 7%. As a profession with its own self-sustaining journals, complete with its own network of reviewers and standards, criminology is functionally emancipated from its parent discipline.

**Figure 14: Journal Location of Criminology's Content**



Ronald Akers recognizes that criminology has assumed a unique professional persona through publishing in non-sociological outlets. He attributes this to push and pull factors.

I think it was a combination of things. This is part of what my 1992 presidential address was about is there developed within sociology more and more resistance to the idea that criminology is central to sociology. I think as the editors come in they came more and more to see, “Well, these don’t really measure up. This isn’t really sociology; it’s something else; it’s criminology; it’s cops and robbers. It’s really not good quality.”

That was part of it. The other part of it was simply, and I saw this on my own for a long time I would send my stuff to general sociology journals but more and more when I got them back they wouldn’t even read it. They’d say, “As editor I think this is something more appropriate for *Criminology*, or more appropriate for *Justice Quarterly*.” They would say, “Send this to a criminology journal.” So part of it was the sense that this is not good enough quality. But most of it was, “Now you have other outlets.” In some ways I didn’t like that but in others it’s just kind of a natural reflection of the fact that not only did we have other outlets but they’re doggone good outlets. I’ll put the quality of

*Criminology* up against *ASR* any day of the week. It doesn't have quite the power rating because of the difference in the size of the fields but in terms of the rejection rates, the quality of its theory, the qualities of its research and so on, it's every bit the peer of *ASR*. Some of it I didn't like but others it was a natural development of a field that has grown, gained prominence, certainly now has established pretty much solid academic acceptability.

The quote demonstrates ambivalence over this development, regret over being dismissed from sociology mixed with cheer over the quality of the field's journals. Two factors are invoked in accounting for this state of affairs, one related to topical concerns, the other quality of intellectual product.

Sociological outlets—*ASR*, *AJS*, *Social Forces*, *Sociological Quarterly* were all specifically mentioned—in the earlier stages regularly showcased criminological research. Editors, acting as gatekeepers, elected to defend sociology's identity by excluding criminological content. Part of this may have resulted from its need to maintain a superior status to an emerging field that taught little more than, in the words of one interviewee (Charles Tittle), “handcuffs and guns” at that point in time. The intellectual content of its output was seen as dubious; offering the upstart field the benefit of the sociological imprimatur was likely to impose costs on its respectability within the social sciences generally. As criminology grew it needed outlets to publish in if it was to gain acceptance in the academic sphere. New outlets developed, at which point editors of sociology outlets began suggesting criminology submissions would be better located there. Ted Chiricos' statement that he would be bringing criminology back in after his recent ascension to the editorial post of *Social Problems* was cited by Dr. Akers as a welcome countertrend. Criminology, left to its own devices, eventually developed an intellectual product that is now recognized as a peer; however, several stated the field now tolerates too many mediocre journals.

One characteristic of criminological research that seems to limit its appeal to sociology generally, as well as to its highly ranked journals specifically, is its limited applicability. Joachim Savelsberg explains that criminology's orientation is one that is narrow and more circumscribed vis-à-vis sociology. When asked if he sees discernable differences between standard fare on offer in criminology versus that appearing in sociology outlets he finds there are.

Yes I do. I do because I think an article published in a general sociology journal should also be of interest to people who do something other than criminology. It should be of interest, of course, to other criminologists but it should also be of interest to people who have a broader sociological interest. So when I get my copies of *ASR*, *AJS*, or *Social Problems* I would like to be able to read with interest articles on organizational sociology, or economic sociology or sociology of the family and gain from that reading. So I expect that those articles will be written not just for people who totally specialized in family sociology or in organizational sociology, but also try to draw some links to broader sociological themes. I think authors of criminology-oriented articles should do the same when they try to get published in general sociology journals.

Following up on the point implied in the preceding as to whether criminology is drawing from a limited body of knowledge he stated that, "It might draw on a more specialized literature, on a thematically more specialized literature". The upshot of this is that sociology demonstrates greater intradisciplinary cohesion than criminology; at least two other interviewees (Hagan and Lauritsen) questioned this characterization however. Articles appearing in its primary journals are required to meet a mandate that the work generate an interest from the broad spectrum of sociology. It does this through offering content that speaks beyond a technically focused audience. It attempts this through referencing memes that carry a general, rather than specific, valence; in essence there is a common coin with which ideas are traded within the community. As an example of this he offers that his contributions within the sentencing literature incorporated themes drawn from Max Weber's sociology of law. The Weberian canon is recognized



throughout a substantial portion of the discipline as being at the foundation of its understanding. This is part of an emphasis within his work of infusing criminology with a sociological mindset, one that appreciates an approach linked to bridging leitmotifs.

A chi-square analysis of the evidence collected from the quantitative dataset reveals that these observations are well founded. The data were divided by journal type. Disciplinary journals are those espousing a sociological orientation, the remainder are interdisciplinary (i.e. criminological). There are more than three times as many of the latter than there are of the former (1407 to 402). When divided according to several article characteristics statistically significant differences are found (Table 8). While only a third (33%) of sociology journal contributions lack for a theoretical contribution, better than half (56%) of criminology's do. On three of the four theory classifications (structural, cultural, control, but not biological) statistically significant ( $p < 0.01$ ) differences are found, all with theoretical presence being more pronounced in sociology. Sociology is, somewhat unexpectedly, more receptive to quantitative research and criminology to qualitative. Additionally, sociology is more inclined toward the use of official data; the differences in the appearance of ethnographic data are statistically even however. The differences in terms of the level of analysis are statistically indistinguishable as well. The qualitative/quantitative divide can also be seen in terms of sociology's evidencing more of a multivariate statistical approach and analytic/predictive orientation.

**Table 8: Differences in Sociology and Criminology Journal Contribution Characteristics**

<i>Variable</i>		<i>Sociology (N=402)</i>	<i>Criminology (N=1407)</i>
<b>Theory*</b>	At Least 1 Theory Mentioned	67%	44%
<b>General Approach*</b>	Quantitative	81%	72%
	Qualitative	5%	10%
<b>Data Sources</b>	Official*	28%	15%
	Ethnographic (NS)	6%	5%
<b>Level of Analysis (NS)</b>	Micro	56%	57%
	Macro	16%	11%
<b>Method*</b>	Multivariate	81%	68%
<b>Study Orientation*</b>	Descriptive	8%	19%
	Analytic/Predictive	87%	71%

Column percentages do not tally to 100% due to omitted categories. Only those thought most illustrative have been included.

\*p<0.01

Dr. Sampson shared a story which illuminates on this thematic divide between approaches through a vignette on the acceptance of an article with an orientation on the sociology of knowledge (Laub & Sampson, 1991). Two well regarded journal outlets, *AJS* and *Criminology*, evaluated this contribution based on differing grounds.

Absolutely. They work in different ways too. For example, some people were surprised to hear about the article that we published on the Glueck-Sutherland debate, which was a kind of sociology of historical ideas. We originally submitted that to *Criminology*. It got rejected flat out. We disagreed completely with the reviews and didn't even attempt to revise it. We submitted it to the *American Journal of Sociology* and it got accepted. That's because, in hindsight we probably should have known better. We thought originally, "This is about Sutherland, who's Mr. Criminologist, and the Gluecks. It should really be of interest to criminologists." But at the time the editors and the reviewers didn't think it was empirical. It wasn't a test of a theory. It was kind of viewed as speculative, whereas the *American Journal of Sociology* viewed it as asking a legitimate kind of question. I think criminology right now as a field is a bit conservative

in terms of its working persona. I think it's very much right now in a theory testing mode, normal science. You have to have a hypothesis, a dataset, and a test. Whereas the *American Journal of Sociology*, and sociology generally is much more about letting a thousand flowers bloom. There's much more diversity in terms of methods, ethnography, different kinds of questions and so forth. So, there is a difference. I think that it may hurt criminology actually if it continues its somewhat rigid stance. Now I want to be clear: it's nuanced. The good side of criminology is that it is a little bit more interdisciplinary. It's a lot more interdisciplinary than, let's say, sociology. So again, you're going to get articles on topics that you won't see in sociology. But I do think that comes at a cost.

Criminology is characterized as an interdisciplinary pursuit wedded to specific day-to-day understanding of its scientific personality. The field is locked into a mode of theory testing, which demands a normal scientific gestalt. This approach is analogized to an assembly line approach with a hypothesis, dataset, and test. Sociology, alternatively, has a more fluid orientation. It is one that is permissive of a wider diversity of methodological approaches and attempts to answer a wider range of questions. As a result, a question initially perceived as having something to contribute to criminology's understanding of its heritage is rejected from its flagship journal by dint of its not being sufficiently empirically oriented while it is recognized as legitimate inquiry by a leading sociology outlet.

Robert Bursik suggests that some of the more creative criminological work was the natural outgrowth of the training that sociology, and disciplines more generally, required of its adherents. Using an reference to a noted singer-song writer he states that the sociological worldview is more diffuse in terms of its working identity despite criminology being an interdisciplinary exercise.

Well, then there were only a handful of criminology or criminal justice departments. Just a handful. Berkeley had one. Albany had one. Just a few. Most criminologists were trained in other departments. I was trained in soc[iology]. Some came from economics

or political science or social work. That had a real good advantage because you had to place your material in a much broader framework. You couldn't be a Neal Young criminologist and play a one note guitar solo for forty-five minutes. That was good. So folks in my generation you see drawing from all kinds of literatures that are kind of unusual. Sampson's my generation. Hagan's a little bit before me but pretty much my generation. He's just encyclopedic with the shit he draws from. And you were forced to do that.

The reference to this being a generational phenomenon is critical, in light of the fact that criminology has now managed to carve out an institutional niche in the university structure. This suggests that the limited criminological orientation may become calcified by virtue of a reduction in the infusion of alternative professional orientations. Criminology departments are now able to hire graduates trained entirely within the criminology and criminal justice tradition; it now has the potential to become not only self-sustaining but self-referential. Some regard this as a blessing; others are skeptical.

### *Institutional Perspectives*

In terms of the respective dispositions regarding the working understanding of the application of state power there are differences of opinions that are nearly axiomatic. This is likely the fruit of a reformist tradition that is deeply rooted within sociology. On the other hand, criminology and criminal justice are fundamentally less averse to the application of power by virtue of having largely been midwifed by the state.

Francis Cullen, who was trained as a sociologist at Columbia University but has spent his professional life working within criminal justice departments, admits to a *faux pas* which illuminates the dispositional divide.

I was giving this talk to the sociology department at Cincinnati, where my wife Paula has been the chair for years, and the facial reactions varied from mixed to perplexed. Then something suddenly dawned on me on why this was occurring. I told the faculty and

students at the talk: “The problem is that in criminal justice we think control is good. In sociology departments, you think control is bad.”

These reactions suggest something at the visceral or gut level about the general perspective held by each. What he regarded as uncontroversial, at least in reference to the worldview he has become accustomed to within criminal justice, elicited a vexed reaction from those of another discipline. The question of power is an issue in which the two seem to be at odds in terms of its acceptance.

Elsewhere in the discussion he elaborates on the ideological incongruence. Criminology finds its range of explanation constrained as a result of its sociological legacy, but differs from its former sponsor nevertheless. Criminology’s heritage and long tutelage within sociology has meant it has adopted its understanding of what constitutes admissible policy solutions and theoretical assumptions. His feelings on criminology emerging into its own are ambivalent. Here he elaborates on the downside.

But at times, criminology has become a highly ideological field. And that leftist bias comes from sociology. I think it constrains the education of our students. For example, if I get up at an ASC meeting and I say, “I oppose capital punishment.” No one will say anything. But if I say “I’m for capital punishment”—which I’m not—but if I were to say that, I’d get booed. You can say at a meeting, “Well, social bonds change offenders. If offenders get a good job and meet a good mate, they will change.” And no one will say anything to you. But if I said that planned intervention in a correctional system that involves a well-organized treatment program can change criminal behavior, at least half the audience will think I’m full of shit. Now let me get this straight, Sampson and Laub’s three-item measures showing that social bonds change white males from Boston (where I grew up), that’s unquestioned truth at a criminology meeting. But four hundred studies based on a meta-analysis of what works—criminologists are not sure about that one! Of course Rob and John’s study is a classic—an exemplar of first-rate scholarship and they

were using the measures available to them. That's not the issue. Rather, it is that their findings are welcomed uncritically because they are ideologically pleasing whereas as others—say about rehabilitation or about Gottfredson and Hirschi's self-control theory—are greeted with suspicion and scrutinized. So I think that part of the problem is what criminologists are willing to bring into the discipline. Of course, we don't want to bring in all social science disciplines. We like sociology, but we don't want economics in our field because the economists all bastards! Right? The only thing they want to measure is the costs of punishment, which they want to inflict on offenders! We don't want them around. In short, we like people trained in sociology because they echo our way of thinking.

Those positions that challenge the leftist sociological orientation are required to meet more stringent requirements to prevail. This lack of total impartiality indicates that those arguments that cohere more closely to its worldview are privileged; those that are seen as an affront are wont to generate questions. Criminology is not a value free interdisciplinary field.

Robert Sampson concurs with the tenor of this characterization offered in Dr. Cullen's quotation leading this section. Criminology and sociology begin from separate *a priori* origins. The former is more likely to accept the power of the state to define its content, the former to question this presumption.

I think that's right. In the sense that sociology's always had a little bit more of a muckraking tradition. It focuses in on the social. It tends to look at things from different viewpoints: the law of unintended consequences, self-fulfilling prophecies. These are major concepts. You take Merton's work or others. Criminologists, especially in the policy world, tend to start from existing institutions and don't step back and consider legitimacy. Sociology of law, for example, has never made the inroads into the core of criminology. In other words, don't forget there is a whole other world out there on law and society, associations, and journals. Criminology does seem to start with the

assumption of legitimacy of institutions and works within that framework which is why it's so policy relevant, or sees itself as being policy relevant. Whereas if you talked to someone in Law & Society, they'd be sort of dumbfounded at the notion of policy relevance in the way that ASC sees itself. There are certainly differences of that sort. I think one has to be on guard against the dangers.

The advantage of criminology in assuming a more deferential posture to the state is that it manufactures work that is applied and policy relevant. This provides an abundance of professional advantages. Alternatively, this arguably weakens its academic profile. Criminology has provided a less receptive audience to the sociology of law; its investigation begins at a later, less fundamental point. It is indifferent to a research agenda that it sees as pointless; why problematize settled matters? Therefore the two work within frameworks which disagree on the topic of exploring the legitimacy of law. Within the field this is evident in the critical approach's peripheral position.

Joachim Savelsberg suggests that this state of affairs can be traced to criminology's attempt to cultivate an individual sense of identity. The separation is motivated by its efforts to affect a professional image that is distinct from its parent.

My speculation is that, as you know, criminology has partly consciously attempted to isolate itself or become independent from sociology and develop its own institutions, its own departments, its own journals, so on and so forth. The more criminology did that, the more reluctant sociology programs were to hire criminologists, or maybe only a couple, just to get enrollments up in certain courses for which there would be high demand. Then, of course, there is—maybe in general—a different ideological perspective, and I think that matters too, in sociology versus criminology/criminal justice programs. I would assume that most faculty in either type of program are more on the liberal side. But people in criminology/criminal justice programs are possibly a little more—how shall I put this?—they are a notch more conservative, much more believing

that government is trying to do the right thing, and asking how can we help government do the right thing. Whereas in sociology departments, especially in the area of social control there is a more generalized mistrust of what government is trying to do. But that's pure speculation. I think you know of some studies that were done in the 1980s, I believe, on network ties, on overlapping membership in different sections in the American Sociological Association. Those studies show that criminologists are structurally somewhat—remember that these are sociological criminologists—not totally but somewhat isolated within the American Sociological Association. That might have been a push factor toward establishing an independent criminology. And then, of course, there were pull factors, having to do with government funding and so on. You know my thoughts about that (from my publications on this theme).

Criminology's conscious attempt to create a unique academic architecture—complete with departments, journals, and organizations—outside of sociology has generated resentments from its former sponsor. Sociology, as a profession, is now only willing to hire enough faculty to satisfy student demand; Francis Cullen laments that deviance has been pared down to criminality. This indicates the subject matter's popularity but also the discipline's attenuating and begrudging respect for it as a research agenda.

The concern over how the conflation of the aims of the profession and governmental funding streams and how this may serve to dilute the field's claim to being objectively scientific was raised with several interviewees. Despite this, most agreed that the field's intellectual and professional health is satisfactory. That should not be read as an indication that there were no concerns over the continued expansion of the field however. The concluding empirical portion of the work discusses a few of the anticipated threats to the field's growth as well as promising prospects for the shaping of new facets of its research agenda.



### *Future of the Field*

The dissertation to this point has been an effort at taking stock of the field's development to date, an entirely retrospective account. To conclude the interview respondents were asked for assessments on the current state of the field, its potential for continued growth, and predictions on where future research is heading.

#### *Threats to the Continued Ascendancy of the Field*

There was only a single exception, Freda Adler, to the prevailing opinion that criminology is poised for some measure of plateau in its growth. The predictions aggregated around two primary emergent concerns. The first is that the field may encounter a reduction in the funding of its research. The concern is embedded in the opinion that the field waxes and wanes in terms of its policy relevance. It is simultaneously enhances and hinders the field's vitality. The second set of issues stems from concerns of the professional-intellectual state of the field. The field, as it has grown geometrically, has become removed from its source of ideas. Additionally, as it has grown it has become more fractured, less coherent. Its center mass has now divided into congregations of specialists generating research on ever more specialized topics.

The dominant socio-political development affecting the United States at the time of the interviews was the financial meltdown resulting from the collapse of the housing market. The catastrophic impact was seen to have implications on the funding that the field would come to earn in its aftermath. A cut in funding, Steven Messner notes, would reduce funding for innovative research, much of which is produced in academic environments.

One thing that always worries you, and you don't have to be a Marxist to figure this, is resources. You can't do much of anything without resources. Funding for innovative research sometimes gets cut. Who knows what's going to happen now. A lot of quality research is produced in academic environments. If things get really tough you might not have the same application of basic human labor, scholarly power to developing the

discipline. That will obviously be a hindrance. I think occasionally the danger of ideological interference occurs. That's always troubling.

Funding research that is less basic would likely assume priority in the event the federal budget is further limited. The last comment is a reference to an earlier portion of the discussion in which presidential administrative preferences and their influence on types of research were elaborated upon. The vicissitudes of administrative whim ultimately shape what issues the field directs its research energies to untangling. The inevitable by-product of working within the policy arena is that the association between funding sources and agenda setting ultimately clouds its image as a science.

Criminology seems to both benefit and suffer from its association with policy. Some of this results from the mere innuendo, whether true or not, that the two are closely intertwined. Dr. Akers argues that to the extent that the (mis)conception that the field is beholden to the state's interests holds the public imagination the field could potentially reap ill consequences. Implicit in the association is a tacit presumption that the field's expertise can be pressed into the service of the state in its aim to control crime.

It could be that something happens somewhere along the way that there's some sort of promise, if you will, that the field, even if it doesn't make it explicit, if it's just some sort of assumed or implied promise that there's certain things we can solve. "We can solve the crime problem," for instance. Even to the extent that that's assumed and we don't deliver on that then I think there'll be a backlash.

The comments above were offered prior to a discussion of how the inability, even *ex post facto*, to arrive at a definitive consensus on why crime declined precipitously beginning in 1993. Almost all of the interviewees were dismissive of this failure being of much import. Some cited the lack of available data to make an accurate determination of its causes. Others invoked political science's lack of foresight in predicting the demise of communism. Political science as a discipline has managed to survive despite this, several stated, so why can't criminology follow

the same pattern? This collective shrug may be attributable to a genuine lack of concern over the field's loss of credibility or a skepticism that the critique carries much value. It could also be indicative of an overestimation in the field's sense of self. Criminology is significantly younger than political science. As a result the latter can draw on accumulated reserves of academic capital to sustain it through crises. Criminology may lack such a luxury.

Ronald Akers continues on this note, citing an example of how sociology's development ebbed as a result of its association with policy and an inability to manifest its promise. When asked to remark on where the sources of any looming threats to the continued growth of criminology as a profession he states,

Right now I don't see anything to torpedo it. But here's something that might happen. At this point we have not been that heavily involved in policy, I'm talking about criminology as a field. This is one of the laments that many people make that we don't have enough impact on criminal justice policy as a field. I think we're growing in that. I think more and more people are paying attention to what we're doing and looking at, especially in terms of applications of the theories we have to treatment and prevention. I think if we get very heavily into that. That's been part of our appeal. Look, this is an academic discipline but one that's highly relevant. It's one that immediately speaks to issues that we're worried about as a society. At one time that was the reputation that sociology had. That it was relevant. That if you studied sociology you would know something about society and you could figure out way to try and change it. Well, after a while we didn't deliver on that promise. So starting in the late '70s and going all the way through into the '90s sociology began declining. They lost programs. Well, you saw that at Washington University there. They started losing students and enrollments went down. So by the time I wrote that piece in '92, the Southern Sociological Society presidential address, the number of sociology degrees being granted were fewer than the

number of criminal justice and criminology degrees. Since then sociology has also grown and has gotten back more to policy relevant issues.

Criminology is certainly not immune to these issues. The solution that this seems to lead one in the direction of is in reducing the emphasis on being relevant. It should become more detached and objective, less of an advocate. This is a position advanced by critical criminology, generally, and by at least two other interviewees specifically (Savelsberg and Tittle).

William Chambliss guides our attention to another pragmatic curriculum related consideration in the following.

It's not going to die but it's going to seriously decline in its academic importance in the next ten to fifteen years. We're already seeing signs of it. The money is going to dry up. The number of police is going to dry up. People are going to be decarcerated because states can't afford to keep all these people in prison. So all of these things will decrease the federal and state funding for police officers, the training of police officers, and the like. These are structural changes. We have more police officers than we need. We have far more people in prison than anybody wants to have in prison. All of that is going to cause a decline in the demand for police officers. This will, of course, cause a decline in the demand for the training of police officers which is what criminal justice programs are doing. Even those, like the one's I'm involved in, that try to criticize the entire structure of policing, nevertheless a third of my students are going into law enforcement. Another third are going into law. Another third are confused and don't know what to go into. So that's going to definitely decline. The funding will decline. We're going to get less money for doing these studies that have not proven to be of any use to anybody anyway. I think we'll go back to the 1950s where it's kind of a small discipline within political science or sociology. These independent criminology departments will slowly disappear. All of which I think is a good thing.

In short, a cascade of policy adjustments will reduce the need for the training of criminal justice bureaucrats. Thus the need for separate departments will fade.

Robert Sampson suggests that the field, through the unabated growth in one of its primary organizing bodies, the American Society of Criminology, may become a victim of its own success.

One, I don't think growth for growth's sake is good or necessarily should be pursued. I've seen that in the ASC as an organization. There's this notion that more is automatically better. I'm not sure that's true. The actual quality of the meetings, in my opinion, has not gone down simply because there's more diversity I think, but there's just so much now. I think it's hard to maintain the high quality of the conferences. That's happened in sociology too. The larger they get overall the quality of individual panels goes down, especially to the extent it becomes more specialized. For example, I know there's a lot of disagreement on this, but I find it strange when you have a specialty organization like criminology that within it you then start having divisions. A lot of people think that's crazy. "How can you be against divisions?" It's not so much that I'm against it. But what does it mean when you split off from a discipline and you have a specialty field and then you start having sub-divisions? So you have divisions by groups like race and gender. Then you now have methods, like the Division of Experimental Criminology. We have a *Journal of Quantitative Criminology*. I actually don't think all this is a good idea because it tends to reify boundaries that probably shouldn't exist. If a study is a good study then it doesn't matter if it's experimental or not, or quantitative or qualitative. So I don't think these divisions have a lot to do with the substantive intellectual question; they have to do with administrative and organizational reasons. I think over time, if it continues to proliferate, then you're going to see just that. Then there'll be another society. Well, there's already the Academy of Criminal Justice Science. Then you can imagine the ASC splitting into two. Then you can imagine the

experimentalists peeling off and so on and so forth. They'll have their own meetings. To some extent that's good. You want people to have an opportunity to talk with one another. But I think we need to keep our eyes on the prize and really not splinter too much. Secondly, again I go back to the policy question and the funding that drives criminology. I think this has a history going back to they used to call it LEAA, doing research driven by what the government wants rather than by what the right scientific questions are. I think that's a real danger for criminology to get too close to policy in that sense. I'm hopeful that danger can be recognized. Certainly we should be policy relevant, but there needs to be a certain independence of both the organization and the work of criminologists from the policy world.

There is a negative correlation between the coherence of the field's research agenda and its organization's size. Size seems to hinder effective communication.

Robert Bursik draws our attention to another issue, the institutional segmentation of criminology from sociology. He seems to suggest that its interdisciplinarity has produced an insularity that threatens to leave the field marooned.

We are by nature a synthetic discipline. We drew, in the American case, from urban sociology. Without urban sociology there's no criminology in the United States. In Europe without studies of political economy there's no German criminology. The thing that used to bug me and it's always been one of my mantras. I'm so tired of criminologists who learn about the social world from criminologists. Criminologists have a narrow point of view. Any other discipline has new and invigorating perspectives on this. When you look at what's being done in urban research or adolescent development that's not too bad. Those guys aren't bad. The adolescent development folks, even though I'm not partial to it. A lot of those guys have come out of psych, which is really in tune with that developmental stuff. That, I don't have too much of a beef with. But to have people who learn urban soc from me and Sampson and Peterson,

that's bullshit. Go read some urban sociology and learn what it's about. I agree one-hundred percent. We're losing a lot of our vitality. I used to look at the citations and it used to bug me as editor. It's very group auto-erotic.

BD: What's the implication from all this?

That it feeds upon itself and that it withers and dies. It's just like belonging to a cult that believes in chastity. Sooner or later everybody's going to die. And you're not going to recruit anybody for Christ sakes. You need fresh blood from the outside. We used to get that all the time. But with more and more folks coming out of departments with a strictly criminological orientation some of that's disappearing. You used to count on new cohorts to bring in new ideas. That still happens statistically, but idea-wise you don't see it as much as you used to.

Dr. Bursik uses a felicitous metaphor of procreating through the generation of offspring. Criminology's myopia and self-referential nature suggests inbreeding detrimental to its long-term prospects. Expanding upon this one can see a characterization of the field as sterile. The implication from this narrowness of perspective is that the field, failing to incorporate external ideas while neglecting to generate new ones from within will eventually disappear. Seemingly, its interdisciplinarity has come to mean non-disciplinarity.

#### *Expansion of Criminology's Horizons*

The section immediately above presents a somewhat dour foreshadowing of what the field is becoming, much of it centered on the critique that the field is overly invested in both policy and the criminal justice professions. As an intellectual pursuit there are ample grounds for expansion however; these give reason for optimism. Several interviewees offered advice on where criminology is likely to establish successful outposts on the frontiers of knowledge. What follows is an enumeration of compelling research questions the field's leading intellects suggest merit greater attention of theoretical (themes of power, culture, biology, and state crime),

methodological (time/space nexus, macro/micro division) and empirical research question (accounting for cross-national differences in crime rates, white-collar crime) ilk.

Robert Bursik suggests that the field may advance by looking backwards. The field has lost contact with a basic organizing principle, one that is more pervasive within sociology, power. The sociology of law, as well as the critical/radical approach, could serve to rejuvenate the field.

One of the things we come and go on is that we don't keep in mind like we should, that's at the heart of this whole thing is the issue of power. Somebody had the power to make the law. Somebody had the power to enforce the law. We lose that focus on power. And I think that's what gives so much richness to the earlier stuff because people recognize that. We just kind of chopped it down and said, "Well you've got conflict theories and consensus theories." It's so much more complicated than that. I wish we had kept our thumb on the power question but we don't.

This once powerful orientating mechanism has been compacted into a cliché. He is suggesting that the notion be problematized through injecting complexity into this conventional understanding. Doing so would affirm the cyclicity of the accumulation of the field's knowledge once more; a dormant theme can once again be revived.

Some suggested that the field would be best served through the continued pursuit of normal science. Dr. Tittle stated the field should devote itself to deepening its understanding of the subject matter already before it. Freda Adler pushes the field in the direction of both continuing to integrate the interdisciplinary theoretical strands as well as awakening itself to an international/global understanding. Robert Sampson, in the following two quotes, affirms them both. When asked what lingering questions the field should give consideration to addressing he begins,

Well, I think everything in a way. I don't think this is necessarily the most important question, but one that comes to mind given the way you asked the question is the macro societal question that I've thought that we've never really answered. Criminology,



sociology really doesn't either, have a good answer to societal differences in crime. There's such huge variation around the world. I just don't think there's a really convincing, credible, or consensus viewpoint on that. Certainly temporal variations in crime are similar. Historically, the '60s, the recent decline in crime. If you go back through time there's still a lot of puzzles there, a lot of questions to be answered. That's for sure.

Criminology has much to accomplish in terms of solidifying its understanding on the issues presently before it. There are also compelling questions to be asked on phenomenon drawn from historical patterns. This implies that the explanations offered in accounting for these are less than convincing, or that these questions have generated insufficient attention. Establishing the patterns of crime to be explained cross-nationally has been afforded by the availability of new data. There seems to be a developing literature which attempts to provide a satisfactory explanation on the topic but, as yet, no *fait accompli*.

Continuing on, Dr. Sampson gives insight into what he considers to be promising framework within which answers to these questions can be obtained.

I think that it may well be that to answer those questions we're going to have to take on some new frontiers. For example, something that never had much of an impact in criminology—I never understood why—concerns the cross national question. I've wondered why there isn't much more of an impact of anthropology, history as well. Anthropological notions of societies and changes in cultures, and so forth. I don't think criminology's handled culture very well at all. That's another way of saying it. I think anthropology, especially some of the classic works, has a lot to say about culture. I haven't incorporated it particularly so I'm not saying I've done it. That's an example of a frontier that may or may not be useful.

In this regard interdisciplinarity may help, in that the field can access an anthropological or historical frame of reference without violating any disciplinary taboos in terms of the tools it applies.

Steven Messner speaks of methodological advances he would like to see in a different context below. Here he explains that the expansion of normal science as well as applying the field's methodology to explaining international trends is an invigorating prospect.

Well I think we are wrestling with important questions all the time. I'd say one of the things that we still need to work on, and this relates to the stuff I do so it's very prominent in my mind right now, is bringing together—and this is something that N[ational] C[onsortium] O[n] V[iolence] R[esearch] was quite concerned with—time and space. Temporal dynamics and spatial patterns. We have good research on both separately; but actually bringing the two together and understanding the processes linking them. I think that's a real challenge for criminologists interested in the types of things I'm interested in. I suspect you'd get a very different answer from somebody who's interested in more individual level analysis. Again, going back to my interests here: understanding the interrelationship in levels and in change. If you look at homicide rates for the advanced nations, if you take a historical perspective you see one thing about declining levels and where it's declined and so on. Then you look across nations at the levels and you see something quite different. How do we explain both phenomena in an integrated way? That, I see as a very exciting challenge for the future.

The field has generated an understanding of how crime is temporally and geographically patterned, albeit as separate problems. The next challenge before it—and it created an institutional support for this—is to marry the two. Creating a unified approach will require a breakthrough in overcoming level of analysis issues that have demonstrated a tenacious persistence. Some as yet undevised methodology, statistical or otherwise, will require an

imaginative formulation in order to unlock a deeper understanding of crime through the application of the field's research energies.

Aspects of the contributions of Drs. Messner and Sampson are combined in Ronald Aker's estimation of what the field will be exploring in the near term—overcoming the micro/macro divide and explaining societal differences in crime rates. He suggests these investigations will lead the field in the direction of an old bugaboo of sociology, biology.

The other thing I see down the road is that what we will more and more include in the second part [accounting for cultural differences] is biological processes. I just don't see how we can get out of it because this DNA and all this other stuff is coming through. We have a much better idea of that than we used to. When we say biological what do we mean by that? So I think the links between the social context, social structure, and what happens at the individual level I think more and more we're going to have to pay some attention to that as including biological processes. Thus far it's all or primarily been promise.

Elsewhere in the present work biology is mentioned as being in its ascendancy, as it is by a couple of other interviewees (Cullen, Sherman). Dr. Akers alludes to this being part of a larger fascination with using biological indicators to account for variation in crime. He describes this eventuality as almost foreordained. The zeitgeist has shifted back to where it once was (e.g. Lombroso) yet again.

This last item takes us full-circle in, once again, encouraging the field to reexamine dormant agendas. Several interviewees (Adler, Hagan) mentioned that white collar crime is an underexamined issue. Dr. Savelsberg devotes the early portion of the following quote to encouraging the field to advance its understanding of this issue once more. The concluding portion is spent introducing an agenda that his own work, and that of John Hagan, has been tending to lately, genocide.

There are certain types of crime that continue to be underrepresented in terms of criminological interest. This starts when Edwin Sutherland did successfully introduce white-collar crime, corporate crime into criminology. But I think it is still undervalued in criminological research, given all the harm that results from white-collar and corporate offenses. I think there's way too little work done in that area. So this is a field that's been established and needed to be established just given the amount of harm caused by those sorts of offenses. But it's still undervalued in criminology. Then of course, yes, there are those acts which are committed by state actors. That is typically the case for crimes like genocide, crimes against humanity, war crimes and so on. Those have been neglected, almost completely neglected, by criminologists.

The questioning of state actors in perpetrating criminal activity harkens back to the critical approach. William Chambliss suggests that the critical approach is uniquely suited to explaining such phenomenon. Joachim Savelsberg's approach emphasizes liberating the field from the dominance of the state in pursuing these questions.

While the field is assuredly poised for some kind of reduction in its expansion—its growth cannot continue infinitely—there is no reason to believe that its intellectual élan will be sapped. Ideas are continually, if incoherently, being pursued by the field. There are growing suspicions however that the field's growth as an independent enterprise may further remove it from the orbit of the disciplines and its source of inspiration. Nevertheless, the vanguard of criminology polled in the dissertation has been adroit in pointing the field in the direction of compelling questions in need of a response and suffers no lack of enthusiasm in stating that criminology is equal to the task before it.

#### *Summation of Practical Implications*

Criminology is set in its working identity of explaining the origins and response to criminality. As Francis Cullen pointed out in the course of our talk this is consistent with both Sutherland's definition and the research agendas of many of the field's notable figures.

Sutherland himself, Donald Cressey, Richard Cloward, and Gresham Sykes all managed to balance an interest in developing theory with exploring the control of crime, primarily through prison research. After the effects of the dispensation of LEAA funding dissipated the criminological and criminal justice topics were pursued with nearly equal interest over the remaining decades of the study period.

The field's research is responsive to cultural influences as well. This can be seen in such examples as the emergence of feminist criminology, the rise and demise of labeling, the dismissal and eventual acceptance of critical theory, and the rehabilitation of rehabilitation. A few of the theoretical debates have been fiercely contested but the field is committed to affirming the winner through evaluating the accumulated evidence with collective objectivity. This is especially so when debate manages to generate a compelling work agenda.

The field has managed to create a working personality through defining a measure of internal cohesion and establishing borders with external disciplines. In professional terms the field has managed to separate itself from both justice studies as well as the law and society agenda. Methodologically, it has accomplished this through evolved preferences. The boundaries are still somewhat makeshift however in that the field insists on capturing and incorporating influence drawn from a broad array of sources. This may prove increasingly difficult as the field has successfully erected its own infrastructure of late.

The field now works from a location that is somewhat remote from sociology. This has burnished its professional credentials through establishing respected peer-reviewed journals and generating a research agenda complete with funding. Some question whether the field has become too interested in establishing itself as "relevant" though. Sociology evidences, in the estimation of those interviewed, less of a concern in establishing this. Criminology and criminal justice are more at ease moving its understanding forth from a perspective that shares the presumptions of the state.

The advantage afforded by refraining from questioning the legitimacy of the state is that it allows the field access to research funding. As the economy contracts this relationship may be tested. Oddly, this may prove of some aid in allowing for the exploration lingering historical puzzles, pushing methodology forward, and allowing for the pursuit of an agenda that explores the legitimacy of state power. An ancillary benefit is that this will likely prune the field's weaker journals and re-center its attention on more academically oriented topics.

## Chapter V: Conclusions

The present undertaking has been an attempt at taking stock of the field of criminology. Broadly conceived, it is bifurcated into segments of the core and frontier. The heart of the work examines the state of the field's theoretical and methodological consensus. This portends to be one of the first responses to the clarion call that the field lacks an "intellectual core" (Savelsberg & Sampson, 2002) or "soul" (Laub, 2006). As the field grows, and concerns over its association and influence by political actors escalate, it would be well served to address concerns from within rather than leaving it to be dissected by external agents. It is simply part and parcel of being a conscientious and introspectively alert science. An inventory of its contributions is a necessary first step in appraising its value; a veritable character sketch is required. The assessment must be forthright and the field receptive to notion of confronting its criticism in a sober, objective manner. The field cannot rest on its professional laurels alone; its science must advance and produce meaningful insights if it is to endure. Consumers of the present work should bear this in mind, as the results are somewhat mixed. What follows in this brief chapter are a summary of the conclusions on the major research question, suggestions on what the field ought to consider in terms of a general research orientation, and, finally, a discussion of the limitations of the work and suggestions on extending the present research in a narrower vein.

### *Summation of Findings*

A cursory look at the field suggests a chaotic state of theoretical affairs. The research literature is split between theoretical and a-theoretical (i.e. empirical documentation) contributions. No particular theory evidences clear supremacy over the others for anything beyond a brief interval. Theories disappear from the literature for periods before resurfacing. The category of "other" dominates in terms of its number of appearances within the field's research. These indicators suggest an incoherent theoretical paradigm.

Theory is the integral element in the field's proceedings. It is responsible for providing a format, a language, with which ideas can be exchanged and explored. Theory is a context that defines the aspects of the crime problem to which attention should be drawn. It provides a shorthand understanding of the phenomena to insiders and it attempts to outpace competitors in explaining the issue. To the extent that theoretical frameworks that are divergent proliferate it exacts a tax on these exchanges. It is a situation akin to traveling in a foreign land in which norms, expectations, and assumptions differ. Translation, both literal and cultural, is required in order to reach a basic understanding. Time spent in overcoming these obstacles cannot be devoted to expanding collective understanding. For this reason the epistemological dissensus outlined above is disconcerting, but the field is not bereft of signs of paradigm.

Deeper examination reveals that there is room for optimism in the field's theory cohering. The abundance of theory being generated reveals the underlying dedication of the field to arrive at a more parsimonious explanation of crime and its control—even if it lacks a concrete sense of what theory should aspire to, a non-trivial concern. Additionally, there are a few theories that can be considered to be mainstays within the literature (anomie, control, differential association, rational choice, social disorganization, strain, subcultural theories). These are traditions which inform the intellectual proceedings with some measure of consistency. They tend to develop logarithmically however; the essence of the theory is intact but its manifestation differs by era. Theory emerges; normal science unfolds; limitations are then encountered; finally, it atrophies, lies dormant, and the process begins once more.

The results in terms of methodological unity are somewhat complicated. The quantitative evidence points to a field almost exclusively dependent on multivariate statistical methodology aimed at the individual level of explanation that is analytic/predictive in orientation. The remarks offered by the interview respondents differ from this rather dramatically however. In the estimation of those sampled, criminologists differ in terms of their preferred methodology. They admit of no apologies for this state of affairs; the field's elite regard this as an attribute to be



tenaciously defended. If order is to be achieved it will be spontaneous rather than coerced. From the standpoint of achieving paradigmatic status these results are not as disconcerting as those pertaining to the theoretical state of affairs. The field is still devoted to exploring all the permutations of crime and its control in an attempt to more perfectly define its subject matter. Recourse to *all* the available social scientific methodology bearing on the question should be marshaled in meeting this demand.

The second major theme of the work concerned itself with issues that were less proximate to the notion of paradigm but shaped by it nevertheless (Boundaries, Departure, & Future). Criminology is willing to accept contributions from any number of disciplines, provided due deference is paid to the field. Those failing to do so are likely to be dismissed as presumptuous and uninformed, as is economics. The field has demonstrated a commitment to adjudicating disagreements through the use of data. Ultimately, the accumulated interpretation of these serve to mediate disputes. This attribute also means that the field's assumptions are not fixed and immutable. Questions arise at intervals and evidence reevaluated; seemingly no assumption is inviolable. The insistence on questioning things, especially authority and established wisdom, is a legacy of sociology's influence on the field. The institutional norms between the two are markedly different. Criminology is helped in part and hindered in part by its dissociation from sociology. Releasing itself from the fetters of sociology allows it the prospect of exploring an array of solutions, including anthropology and biology. However, as several have warned, this could also mean assuming an indentured servitude to the state. The influence of policy and politics on the field has resulted in concerns over the field's long-term viability as a profession. The field as a whole may have adopted the short-term solution of putting its expertise at the disposal of the state in its early development but this influence is likely to wane in the coming years, as a result of a devastating recession if nothing else. In an ironic twist the straitening of financial resources could provide a long-term benefit through weakening this link. Weaning itself from the government's underwriting of its research agenda through seeking financial support

from private foundations may also serve a similar end. The future is wide open in terms of the subject matter the field looks to explore in the coming years as well: international/comparative, power and state crime, biosocial influences, culture, reconciling the micro/macro divide all present exciting opportunities for enriching our understanding of crime and criminality.

On the principal organizing question undergirding the investigation—Does criminology have a paradigm?—the answer is, in short, not yet. There are dim but emergent signs of order in terms of a few theoretical orientations that define the field but nothing approaching consensus on the issue. The field's interests are at least partially dictated by exogenous influences, like funding streams and social movements, which serve to limit the field's attention span. An allegiance to explaining a portion, rather than the whole, of the three-pronged definition of the field's dependent variable is the linchpin of the enterprise, which is to say there is an underwhelming measure of continuity across the field and across time. In terms of limiting the methodological scope and establishing disciplinary boundaries the evidence adduced and prevailing sentiment demonstrate an attitude of unabashed *laissez faire*. Much of the field's research belies this however, as it clearly demonstrates a commitment to quantitative methodology but this is an all too permissive definition of agreement bordering on a platitude. If a paradigm is to mature from this quasi-adolescent phase of the field's development it is likely to be spontaneously organized. The field has tenaciously defended its prerogative toward applying the entire array of tools along the methodological continuum. Criminology's default disposition is to resist conformity.

#### *Proposed Global Research Agenda*

Criminology should seek to consolidate its gains along two concurrent tracks, intellectual and professional/organizational. The field demonstrates an enthusiasm for development. The challenge will be in directing its creative energies. Criminology can potentially turn its proliferation of theory and methods from a confusing morass into a cogent account of its science. Unfortunately, there seems to be alacrity for cataloging the field's knowledge inadequate to the

task at present. Francis Cullen, his initiative at “taking stock” while ASC president notwithstanding, laments this in the following,

But at the end of the day, what sociological theory has produced any intervention that’s of any utility? Okay, a couple have borrowed ideas from social bond theory. But my main point is important: You would be hard pressed to find an intervention that a criminological/sociological theory has developed that is now capable of reducing crime—that can be shown to be an effective, evidence-based program. And so what I think I’m getting at here is that at some point, I think we have a field which is self-perpetuating worn-out theories. We don’t organize the knowledge that we produce. We don’t have very good ideas about how to stop crime.

He suggests continued efforts, conducted through the meta-analytic technique and taking-stock, will bear fruit in terms of dismissing shopworn theories. The time has come to begin the process of revisiting these and painstakingly appraising their value. Those that are not worth maintaining should be unapologetically set aside. Demonstrated efficacy in producing policy and predictive potential are the unforgiving standards that should be remorselessly imposed. The fact that the field has produced few interventions of value should make any criminologist chary of rushing into the policy arena with a proposed panacea. The value of the field’s understanding is that it acts as a sensitizing mechanism, not that of its contribution as a science with a capital “S”. Dr. Cullen continues emphasizing the above is not intended to suggest that the collective efforts of the field have amounted to nil.

Let me hasten to say that criminology has had its successes; it has served important purposes. It’s done good things in the world. It’s fought ridiculous punitive policies. It’s falsified boot camps. It’s showed that scared straight programs don’t work. It’s fought racialized thinking of the worst kind. It’s made a case that social processes matter. It’s led to some useful intervention programs. So I’m not saying that criminology hasn’t done a good job.

The alternative implied is that the field may drop its pretense to a rigid fealty to scientific legitimacy, at least in the same sense as that of the natural sciences, and assume a more humble posture of sensitizing the public to the implications of policy adjustments. The upshot is that this will make the field less “relevant” in the manner it has become inured to and more academic.

This prodigious agenda should seek to include contributions drawn from three disciplines, history, sociology, and philosophy. A field as interdisciplinary and intellectually permissive as criminology should find little difficulty in accepting commentary from each. These disciplines have had lengthy developments and could contribute immeasurably to the refining of criminology’s understanding of itself. Criminology has now been working within the positivist framework for over a century. Rather than eschewing this rich history it should embrace its heritage through developing a total appreciation (including its imperfections) of each of its theoretical strands. The focus on discovering the next big finding is predicated on the assumption that the field’s development, like that of its cousins in the natural sciences, is linear. This is a questionable presumption at best. The exemplar the field should seek to emulate is not physics but economics. Understanding is unlikely to be cataclysmic, rather it will emerge piecemeal through the painstaking process of sustained debate. Within each of criminology’s theoretical traditions efforts at accounting for the origins, development, strengths, and shortcomings of each should be detailed. There are a number of compelling questions to be answered with a retrospective account. How did the respective theories evolve? From whence did they come? Why do they fall into disfavor? How are they rehabilitated? How does the field regard those that are dormant? All satisfying questions, each in search of an answer. Each requiring a historical framework, complete with a methodology that is not driven by numbers. The benefit is that the field has acquit itself quite well when opportunities to publish are evident.

The agenda above can be productively augmented through the inclusion of themes drawn from sociology. The sociology of law holds potential for reintroducing the notion of power as an orienting mechanism into the field once more. This would also produce the ancillary benefit of

consolidating both the field's intellectual affairs and the profession through bringing the three elements of Sutherland's definition of the field into one. The divided efforts of the field to understand the individual elements of the tripartite definition in absentia of the others do a disservice in terms of unifying the efforts aimed at achieving a global understanding. Research devoted to exploring a singular element should attempt to broaden understanding on the others as well. For a field that is centered on the explanation of a dependent variable the problem of compartmentalizing its task magnifies the disjuncture. Recognition that each of the elements bears relation to the others, however minimally, serves to anticipate a path forward that is pan-theoretical and would serve to harmonize the study of criminology with that of criminal justice.

Simultaneously, the sociology of knowledge can inform the historical approach in drawing links between the evolution of theoretical/methodological developments and forces of gestalt. For instance, the anti-authoritarian feeling that defined the 1960s undoubtedly succored the popularity of the labeling perspective. Similarly, the backlash against this movement figures prominently in the explanation of the re-ascendance of rational choice theory. How these events impacted the intellectual proceedings of the field is a story left to be told. How is knowledge transmitted within the field? What parties are responsible for theoretical frames of reference enduring or vanishing? These are questions are beginning to attract research attention, a promising sign to be sure.

Lastly, philosophy should be consulted in the defining of the terms of each theory. Theory is the essential tool the field uses to gather knowledge. It establishes the nomenclature used in building an understanding. It defines the parameters of interests, focusing attention on specific aspects of the phenomena before it. And it relies on language to convey ideas. The definitions of which this language is comprised imply a philosophical grounding. Revisiting formal theory construction, with its mandate that each construct be explicitly defined in great detail, may be one method by which this can be accomplished. When working within an intellectual market the terms of trade must be agreed to before trade can flourish. Thomas Kuhn

suggests that paradigm can emerge spontaneously, provided enough empirical content has been amassed that theorizing can inhere. To this contention Jack Gibbs, an ardent proponent of formal theory construction, remarks,

We've tried the discursive mode for about a hundred and fifty years and where has it gotten us? I'd say if fragmentation and incoherence has a pay off where in the hell is it? So that argument just leaves me cold.

Of the three research agendas suggested this is likely to produce the most reluctance to engage in. The strictures imposed by the formal theoretical approach are regarded as overweening. Furthermore, the field as a matter of workaday disposition is ingrained in the empirical tradition. However, it is imperative that the underlying assumptions of theory are made evident and explicit for all to see. Ambiguity also works against falsification. That more articulation is required is attested to by the protracted wrangling over what a given theory intends to say. Authors of theory ought to, as a matter of professional courtesy, be required to offer insights into how a given construct should be measured. Falsifiable propositions should be proffered. Admittedly, the proposal for more theorizing can engender additional quarreling over terminology. The point of the exercise, and its intrinsic value, is to make the field aware of the language it employs.

All three disciplines should seek to contribute to an understanding of how the various theoretical strands interrelate. What is envisioned is a Linnaean taxonomy where the developments of each are charted against one another. As in biology, each of the theoretical animals in the criminological ecosystem should seek to trace its lineage and evolution. The product of this would be an understanding of where the traditions are interrelated, overlap, and depart. An attempt to locate the intellectual influences of each should serve to navigate the enterprise. This process could be furthered through capitalizing upon the on-going efforts of taking-stock within the respective literatures. Who are the forebears of each? What tradition informs the others? Are some more-or-less extinct? What adaptations have been made? Have traditions evolved to form families with others?

Included in this mandate is an effort to map the geography of the criminological knowns as well as its known unknowns. The first step in the process requires an assiduous documentation of what it has conclusively documented and established as fact. Several efforts such as the Maryland Report (Sherman et al. 1997) and the Campbell Collaboration have set the standard in this regard. The field has an emerging awareness of its need to begin evaluating the copious amounts of research it has conducted to date. What it is less aware of is the content of the pressing questions that it has yet to answer. The field has relied, with some success, on leading intellects to direct its attention to emerging issues like state crime.

The field can foster these developments through offering institutional supports. These would not require an inordinate investment of resources. The field, through its growth in the number of journals, has demonstrated an interest in publishing results. One manner in which it could successfully falsify ideas is to christen a Journal of Falsification. Manuscripts purporting to test theoretical concepts using approved methodology that fail to produce the anticipated result could be submitted for review. Reviewers would challenge authors to overcome questions of spuriousness and misspecification. Explanations as to why the approach did not produce a result as expected should serve as an indication of what the known-unknowns are. Additionally, this would reduce the so-called “file drawer” problem faced by the meta-analytic technique. Only those results that have produced statistically significant findings are currently available in the literature. This suggestion may sound farcical but it would serve to bolster the field’s acceptance of a scientific approach in a truly Popperian sense. Additionally, the prospect of having yet another journal in which to publish may prove irresistible to the professional inclinations of the field’s practitioners. Allocating additional journal space to exploring issues surrounding the history and philosophy of criminology could also serve to compensate for the lack of funding this type of research seems to generate.

The American Society of Criminology could also serve as a catalyst in these efforts as well. This would require the continued support, through encouraging sessions at its annual

meetings of content devoted to defining theory as well as assessing its strengths and weaknesses. The organization could also support these efforts through continuing to serve as a forum in which the history and identity of the field can be productively delineated—see recent Sutherland and Presidential Addresses by John H. Laub (2006), Robert J. Bursik (2009), and Nicole Rafter (2010).

#### *Limitations & Additional Specific Research*

As with all research efforts from time immemorial, this work is not without its limitations. These suggest of future efforts to remedy shortcomings and enrich our understanding of the workings of criminology. The qualitative sample is not drawn from a cross-section of the field. Discussants have been purposefully drawn from what could be considered to be the elite of the field. Only three of the seventeen received terminal degrees from criminology or criminal justice departments. The opinions expressed by later generations of scholars may differ as a result. Only future research can determine to what extent this is the case. Contrasting the thoughts of two different cohorts of scholars would allow for a more definitive assessment of how institutional perspectives are at variance between generations. An additional benefit this would provide is recognition of the changing face of the field in terms of its inclusion of more diverse gender and ethnic representation as well as a more contemporary reflection of its intellectual constitution. Perhaps there is an emergent order to the field that has been overlooked by the more experienced sample.

The quantitative findings could be pressed further through an extension of the present research as well. Although the research was inclusive of a great number of peer-reviewed articles by the field having been published over better than half a century it is superficial in the sense that additional understanding can be facilitated through more in-depth analyses of article content. The aspects of the article content that have been more broadly measured can be refined into a more specific coding scheme. Initial results suggest this is a promising vein of research. A more fruitful manner in which this can be pursued would involve the compilation of citation counts.



Each article within the dataset contains a list of references that constitute a rich untapped reservoir of the field's intellectual undercurrent.

Attaching citation count data to the database compiled, in addition to including books and research monographs in the sampling frame, will produce a more textured account of the field's understanding. A simple list of the most cited works would serve to denote a canon of the field's most esteemed contributions; this list is likely to be dominated by books. An analysis of the citation careers of these would amount to a marker of intellectual trends. The coding of articles into differing intellectual strata could further enlighten the field of both its internal proceedings and those drawn from external sources. Articles could be classified into the numerous theoretical perspectives. A tally of each by year would serve as an additional indicator of intellectual trends. Classifying each article by its disciplinary origin would allow for a determination of where the field is drawing inspiration from. Mapping the respective schools of thought could offer a collective cognitive map of sorts. Each of these potential benefits represents exciting prospects for deepening the field's appreciation of its composition and evolution. Much of the field's agenda setting in terms of laying out the major topics of interest are introduced in books. Alternatively, much of criminology's raw empirical research (i.e. atheoretical findings) appears in research monographs; these are likely to be more explicitly policy oriented contributions. The exclusive focus on peer review research presented in the preceding neglects both the theoretically rich scholarship of books and the data driven findings documented in monographs. A truly comprehensive account should not overlook these additional contributions in an attempt to gain a complete understanding of the field's paradigmatic status.

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## Appendix 1: Interview Protocol

### Interview Guide:

#### Intellectual Roots/Biography

- How did you become a criminologist?
  1. Personal influences
  2. Impersonal influences (i.e. literature/ideas/questions to pursue)
  3. Theoretical and Methodological training
    - How has this changed over your career?

#### Professional Activities & Recognition

- Reviewer/Editorial philosophy
  - What kinds of thematic, theoretical, and methodological content are within and beyond criminology?
- Awards/distinctions/honors
  - What was this award or recognition honoring? (Alternate question: what are you most proud of accomplishing in your career?)

#### Body of Work

- Does your work adhere to a single narrative? Several? What is it?
- Where would you place this in relation to the epicenter of the field?
- Success w/given works/themes? Where has your work encountered criticism? How was the debate settled?
- Collaboration w/co-authors. Difficulties in finding common conceptual ground?

#### Conjectures on the State of the Field

- Internal/External threats to continued success of the field? (Legitimacy and the crime decline.)
- Assumptions and empirical findings the field is founded upon and their sustainability. Does the field have a firm intellectual foundation?
- The relationship between ideology and science
- Thoughts on the departure of criminology and criminal justice from sociology.
- Lingering questions the field must address. Forecasting the future of the field.